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An Enquiry into the Statistics and Pathology of some points connected with Abscess of the Liver, as met with in the East Indies. By Edward John Waring, Residency Surgeon at Travancore.—*Trevandrum*, 1854.

Mr. Waring is already favourably known as the author of a work on practical therapeutics, compiled with considerable skill and care from the best English authorities. The present work is also an useful compilation, and is distinguished by the same qualities of care and judgment as its predecessor. In it, Mr. Waring has brought together short notices of almost all the cases of hepatic abscess of which he can find records in the Indian journals, and in works published on Indian pathology.

The work opens with a summary of 300 cases thus collected, which occupies 110 pages; then Mr. Waring proceeds to analyse his facts.

We need scarcely remark that no very accurate deductions can be made as to the effect of race, sex, and age, on the production of hepatic abscess, for, with the exception of nine cases, all the instances collected by Mr. Waring were in Europeans (chiefly soldiers), and except in six of these cases, all were men. The European soldiers are generally young men, and the majority are below thirty. It does not surprise us, therefore, to find that nearly one-half of the deaths were between the ages of twenty and thirty, for this is the average age of the soldier.

A safer deduction can be made as to the effect of residence in India, for it appears that *nearly twenty-three per cent. of the fatal cases were in persons who had not been one year in India*, and more than fifty per cent. were in persons under three years. "Length of residence in India seems to confer a great immunity from suppuration of the liver," says Mr. Waring; but we should feel disposed to reverse the expression, and say, "The effect of an Indian climate on an unacclimatized European frame is to predispose to suppuration of the liver." This predisposition may be expressed in figures; it is much the strongest in the first year, is about half as strong in the second and third years as during the first, and in the succeeding years is about one quarter as strong. That is to say, of two men, one under one year's residence in India, the other over three years' residence, the chances that the first will have hepatic abscess are as 4, while in the second case they are as 1. No length of residence in India will, however, secure complete immunity.

From an analysis of 70 cases, Mr. Waring finds that the habit and make of body had the following effect:

Robust, corpulent	45·714 per cent.
Slender, feeble, sickly, delicate	35·714 „

As regards complexion, in 19 cases, 11 were fair and 8 dark; but these numbers give little information, unless we know the average number of dark and light persons from whom the sick were derived.

In 40 cases the habits were specified; 32·5 per cent. were sober, and 67·5 per cent., or more than double, were intemperate.

The average duration of the cases in hospital was between thirty-eight and thirty-nine days; but the abscess must have existed in many cases for a long time previously, and several of these patients had been in the hospital before for the same disease.

The following table expresses the antecedent disease to the abscess, as far as noted in the records of the cases:

	Number.		Per cent.
Hepatitis, acute and chronic	131	or	43·666
Dysentery, acute and chronic	82	or	27·333
Dysentery and hepatitis, or hepatic dysentery	14	or	4·666
Fever, or common continued fever	14	or	4·666
Intermittent fever	5	or	1·666
Remittent fever	3	or	1·000
Diarrhœa or purging	6	or	2·333
Diarrhœa and intermittent fever	1		
Admitted with hepatic abscess evidently formed	6	or	2·000

It must be observed that the term "common continued fever" is erroneous; it is used by the army medical officers because it is one of the headings of the form used by them in their returns, and which form was framed originally from 'Cullen's Nosology,' and is only adapted for the diseases of cold climates. The "common continued fever" in an Indian return is simply a malarious fever with ill-marked remissions.

Mr. Waring then enters on the very interesting inquiry of the connexion between dysentery and abscess. In 72 per cent. ulceration of the large intestines co-existed with hepatic abscess; in some of these cases, however, the dysenteric attack was, according to the author, secondary—i.e., had followed the formation of abscess in the liver, while in others it was as clearly primary.

With respect to the disease commencing as dysentery, and leading to abscess of the liver, Mr. Waring remarks:

> "It would appear that the complication of hepatic abscess with dysentery is most frequent in Bombay, and least so in Calcutta; whilst Madras holds an intermediate place. In two hospitals in the same station or locality—e.g., the

Medical College Hospital and the General Hospital, both in Calcutta, a great discrepancy exists as to the frequency of this complication; being in the former establishment under 4 per cent., and in the latter 8 per cent.; and, in two successive years, in the same hospital, the Bombay European General Hospital, 1850, 51, and 52, and it is to be presumed, amongst the same class of patients, there exists a wide difference as to the frequency of this complication. Can the mode of treatment adopted be sufficient to account for these discrepancies?

"Some further, but brief observations on this interesting subject may be acceptable.

"In the first Burmese war, 1824, 25, Dr. G. Waddell states that he did not find disease of the liver in any one of his dissections of those who died of the dysentery which proved so fatal to our troops. In the last Burmese war, 1852, 53, Mr. Stewart, of H.M. 18th Royal Irish, states that in fully three-fourths of the fatal cases of dysentery, the liver was found more or less implicated, from simple engorgement to abscess. The latter statement is borne out by Dr. Taylor, of H.M. 80th Regiment. Of 24 cases of fatal dysentery examined by this gentleman, 4 presented abscess of the liver.

"Of 61 fatal cases of dysentery, dissected by Dr. John Wilson, in China, hepatic abscess was found in two instances only.

"In the epidemic dysentery which prevailed in Dublin in 1818, Dr. Cheyne met with hepatic abscess in 4 cases out of 30 bodies examined.

"Out of many hundred cases of dysentery examined by Dr. Baly, which occurred in the Milbank Penitentiary during seven years (1840—47) not one was complicated with hepatic abscess.

"Of 64 cases of chronic dysentery, from India, Ceylon, and the coast of Africa, examined by Dr. Knox of Edinburgh, the liver was diseased in two only.

"Dr. Abercrombie states that of all his dissections of dysenteric patients in Great Britain, he never saw the liver affected, except in one or two chronic cases." (pp. 121, 122.)

On this point we may be permitted to add the observations by the reviewer,* which are not included among the cases collected by Mr. Waring. In 23 cases of *fatal* acute dysentery (in Burmah and India) there was consecutive hepatic abscess in 5, or in 21·74 per cent.

At a subsequent page (123) Mr. Waring informs us that out of 260 *fatal* cases of dysentery in the Madras Presidency (in six different regiments), there were 68 complicated with abscess, or 26·15 per cent. These 260 deaths occurred out of a total of 2758 cases of dysentery; so that the mortality from acute dysentery, with and without abscess, was 9·43 per cent.; and hepatic abscess occurred, of course, in 2·46 per cent. of the whole number of cured and fatal dysenteric cases.

The solution of this remarkable difference between the effects of dysentery in different places and seasons, is at present uncertain. It is usually referred to some malarious influence exerted on the liver in hot climates, from which the dysenteric patients in cold climates are free; but this explanation does not account for Dr. Wilson's cases in China. Mr. Waring's suggestion, that it may be due to treatment, cannot, we think, explain it. Possibly there may be several causes. Thus, it is not improbable that the scorbutic dysentery is not attended by hepatic abscess in the same way as the non-scorbutic inflammatory dysentery is. In the first Burmese war, and in the China expedition, the dysentery was of the scorbutic form. A still more probable explanation, in many cases, is the following. We have already seen that no less than half of the whole number of cases of hepatic abscess in India, are in persons who have, within three years, come from a cold climate, and that the tendency to abscess manifestly diminishes after this time; therefore the per-centage of hepatic abscess in acute dysentery would be much smaller in any body of men who had been four, five, or ten years in India, than in any regiment which had not been three years there; and the discrepancies between different observers may be simply owing to their having to do with acclimatized or unacclimatized persons. Thus, our own cases were chiefly in European soldiers who were under three years' service in India. The influence of food, also, may have something to do with it, as suggested by Dr. Baly. We can therefore, without reference to any peculiar malaria, explain, it seems to us, the discrepancy of evidence, to a certain extent.

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* On the Dysentery and Hepatitis of India. 1846.

With respect to the explanation of hepatic abscess consecutive to dysentery, Mr. Waring does not agree with Dr. Budd, that it arises from contamination of the portal blood by absorption of septic matters from the colon or by true phlebitis, for the following reasons:

> "1. If this hypothesis were correct, abscess of the liver would occur much more frequently than it does, in the course of dysentery; for out of 2758 cases of dysentery treated in the Madras Presidency, abscess of the liver occurred 68 times; being in the proportion of 2½ per cent. nearly. I think that it must be admitted, that if Dr. Budd's explanation were the true one, the proportion would be very much larger than this return shows it to be, and we must, therefore, look for some other and more predominant cause, than the one which he has so ingeniously supplied.
>
> "2. On referring to the preceding histories, we find that out of 204 cases in which the state of the large intestines is described, they were not in a state of ulceration, being generally healthy in 51, or in about 25 per cent.
>
> "3. Looking at the diseases under which the patients were admitted in 300 instances, we find hepatitis recorded as the primary affection in 131 (or 43 per cent.), whilst only 82 (or 27 per cent.) were admissions from dysentery.
>
> "4. Although it must be admitted that, in the majority of cases, some derangement of the bowels was coeval with, or even anterior to, the evident manifestation of hepatic abscess, yet in several instances, Nos. 15, 19, 54, 91, and 163, for example, the dysenteric symptoms did not appear till an advanced stage of the disease, and in some not until the existence of abscess was placed almost beyond a doubt." (pp. 122, 123.)

Mr. Waring then refers to the usual number of abscesses, and gives the following table:

"*The number of abscesses found* in each case:

	Number.	Per cent.
One, single or solitary	177	59·000
Two abscesses	33	11·000
Three „	11	3·666
Four „	17	5·666
Five „	5	1·666
Six „	1	0·333
Seven „	1	0·333
Eight „	1	0·333
Nine „	1	0·333
Ten „	1	0·333
Eighteen „	1	0·333
Numerous „	36	12·000
Diffused „	3	1·000
Number not stated, or doubtful	12	4·000
	300	100·000

We have referred in another place to a case in which there were more than 90 abscesses.* Of these cases, the right lobe alone was affected in 163, or 67·355 per cent.; the left lobe alone in 16, or 6·611 per cent.; and both lobes in 35, or 14·462 per cent.

The position of the abscesses was observed to occupy the different parts of the organ as specified in the following table:

On the upper and superior part of the right lobe	in	19
On the upper and outer part of the right lobe	in	7
On the convex surface of the right lobe	in	11
On the concave surface of the right lobe	in	4
On the inferior edge or margin of the right lobe	in	4
Deep seated, or in the centre of the right lobe	in	2
In the upper part of the left lobe	in	1
In the centre of the left lobe	in	1
Between the right and left lobes	in	4
On both surfaces	in	3

When several abscesses exist in the liver at the same time, they may, as Mr. Waring remarks, occupy very various positions in that viscus.

The quality of the pus in the abscesses is thus described:

Pus, purulent matter, well digested, good, pure, healthy, white, and laudable pus	48
Thick, creamy, or white, thick pus	18
Sero-purulent matter	6
Curdled matter, curdy, curdiform, or whey-like	6
Thin, flaky, purulent matter, or fluid	5
Dark pus, dark chocolate-like matter, dark serous	6
Coffee-like, with flakes of pus	4
Thick yellow, and thick yellow greenish matter	6
Greenish, purulent, and brownish or reddish matter	6
Fluid, like wine lees, with flakes of pus; dark yellow, very offensive; matter, mixed with sloughy shreds; turbid serous fluid; dirty, fœtid pus; glutinous, dark-brown, thick fluid; partly greenish, partly liverish-coloured matter; matter of various hues; reddish-brown, fœtid; yellow, curdly water; thick, sanious; reddish puriform; partly serous, partly puriform; lardaceous, subalbid, and cheesy matter; one each, respectively	16
	119

The quality of the pus was mentioned in 69 cases. The smallest quantity was four ounces; there were between twelve ounces and three pints in 36 cases; in 2 cases there was a gallon; in 1. "several pints." The termination of these abscesses is given in a table of such value, that we subjoin it entire:

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* Op. cit. p. 226.

	Number.	Per cent.
Remained intact	169	56·335
Evacuated by operation, a solitary abscess being present	29	16·000
„ „ there being numerous abscesses, one opened, and the others remaining intact	18	
One abscess opened by operation, another subsequently bursting into the abdominal cavity	1	
Opened spontaneously into the thoracic cavity	14	4·666
„ „ into the right lung	28	9·333
„ „ into the abdominal cavity	15	5·000
„ „ into the colon or large intestines	7	2·333
„ „ into the stomach	1	6·333
„ „ into the hepatic vein leading to the vena cava	2	
„ „ into the hepatic vein at its junction with the vena cava, and another communicating with the cellular tissue around the right kidney	1	
Communicated with the hepatic ducts	1	
„ with the right kidney	2	
„ with the gall-bladder	1	
„ with an abscess in the iliac region	1	
Opened spontaneously through the ribs in the back	1	
One abscess had opened into the colon, and another had passed off by the hepatic ducts into the duodenum	1	
One abscess had opened into the stomach, a second into the duodenum, and a third had been evacuated by operation	1	
One abscess had opened into the abdominal cavity, and a second into the lungs	1	
Terminated in erysipelas of the lower extremities, simulating phlegmasia dolens, the abscess opening into the lungs	1	
Doubtful	5	
	300	100·000

Mr. Waring then refers again to the co-existence of ulceration in the colon with abscess, and found that this occurred in 147 cases out of 204 in which the point was noted, or in 72·158 per cent.; while in 51, or 25·000 per cent., there was no ulceration; in 6 there were healed ulcers, or "abrasions." After some discussion on this point, which we need not extract, he passes on to the question of the size of the liver in abscess, a very important point as regards diagnosis. In 132 cases,

The liver was enlarged, generally congested and softened, in	113
„ slightly enlarged, or not much above its natural size, in	6
„ of its natural, or normal size, in	5
„ sound and healthy, in	2
„ in a state of cirrhosis, in	2
„ in a state of gangrene, in	1
„ small and pale, with an irregular surface like citron, in	1
„ small, light-coloured, buff, in	1
„ extremely atrophied, being no larger than a hand, in	1
	132

The greatest weight recorded in these cases was eight pounds and a half. When thus enlarged, the liver was found to pass downwards and to the left, to a variable, but often to a great extent; sometimes, but less often, it encroached on the thoracic cavity. Three cases are referred to, in which it ascended to the 4th rib, two in which it reached the 3rd, and one in which it reached the 2nd.

The gall bladder was noticed in 46 cases, and in 2 only had it remained unaltered. In 8 cases it was small and contracted, in 12 distended: in one of these cases it was as large as an urinary bladder distended with urine; in another it descended four inches below the inferior border of the liver, and presented a soft elastic tumour, "which might easily have been mistaken for hepatic abscess." In the other cases it was more or less inflamed; in one, gangrenous.

After a brief reference to the condition of the other organs—the stomach, lungs, heart, and kidneys (what of the pancreas?)—Mr. Waring proceeds to the important subject of the

Symptoms of Hepatic Abscess.—He has tabulated these symptoms carefully. We extract a portion of the table:

No. of cases in which the symptom is noticed.	Symptoms.	The symptom was	
		Present.	Absent.
173	Pain in the hepatic or epigastric region	153	20
	„ increased by pressure	51	13
	„ increased by full inspiration	23	4
101	Enlargement or fulness of the side, or some evident local sign of hepatic abscess	90	11
84	Vomiting and nausea	74	10
34	Hectic fever	34	—
75	Cold sweats, or profuse or clammy perspirations	72	3
22	Evening or occasional exacerbations of fever	22	—
74	Prostration, exhaustion, or great debility	74	—
57	Emaciation, or loss of flesh	57	—
76	Pain in the right shoulder	48	24
	„ left shoulder	2	—
	„ both shoulders	2	—
63	Cough	59	4
47	Rigors	25	22
41	Thirst, more or less urgent	40	1
41	Appetite, impaired or total loss of	33	8
36	Want of sleep, or restlessness	35	1
32	Breathing, difficult and oppressed (dyspnœa)	32	—
6	„ hurried and frequent	6	—
15	Painful decubitus on the left side	10	5
5	„ „ on the right side	3	3*
3	„ „ on either side	3	—
2	„ „ on neither side	2	—
8	Flatulence, flatulent eructations, much flatus	8	—
2	Jaundice	2	—
4	Yellow suffusion of the eyes	4	—
3	„ „ skin	3	—

We shall very briefly consider some of the statements, and shall venture to compare with them the conclusions derived from the series of cases (21 in number) which we have recorded, but which are not included in Mr. Waring's book. It may be worth while to see how far the facts of the larger and smaller series agree.

Pain in the side would appear, at first sight, to be a very valuable diagnostic symptom, but we are inclined somewhat to question this, for the time at which the pain came on is not given in the table; in many cases, we have noticed that it does not occur till shortly before death, when usually the diagnosis is settled by other symptoms. Pain in the side is also so common in other cases, that no reliance can *per se* be placed on it. Our own cases of abscess in India, 21 in number, had not led us to anticipate so great a frequency of this symptom, for it was absent altogether in 8 cases, and in 6 others it was slight; in 3 of the cases without pain the amount of suppuration was enormous.

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* We copy this evident misprint from the original.

Pain in the right shoulder was present in 48 out of 76 cases in the table, or in two-thirds; in our 21 cases it was only present in 7, or exactly one-third. Possibly in many of the cases in which no mention is made of the presence or absence of this symptom (and which cases are, of course, excluded from the list), the pain in the shoulder was absent; as if present it would scarcely have been left unnoticed. We are inclined to think, then, that our smaller number of cases, in which the presence or absence was always noted, may give the truer per-centage of its frequency. This symptom occurs in so many cases besides abscess, that its diagnostic value *per se* is not great. Mr. Waring remarks on this point:

"1. Pain in the shoulder is most frequent when the right lobe is the seat of abscess; but—

"2. Abscess may exist in the right lobe without this symptom being present in any degree.

"3. Though this symptom is most frequently present when the abscess is situated in the superior or upper portion of the liver, it cannot be regarded as a constant or diagnostic sign.

"4. The pain in the shoulder may be present when the abscess exists on the posterior or concave surface of the liver, or in the left lobe, or when it is deep-seated, or when the liver is studded throughout with small abscesses.

"5. The absence of this symptom is no proof that abscess does not exist in the right lobe." (p. 147.)

Rigors were noticed in 25 out of 47, or in little more than half the cases. With this observation the facts of our cases quite accord. Sometimes, however, the severe and rapidly recurring rigors are very striking.

Cold and clammy perspirations were present, it will be observed, in 72 out of 75 cases. This statement corroborates the opinion of Annesley on the frequent existence of this symptom; it is, in fact, a very important diagnostic sign, and yet must not be overrated, for hepatic abscess will occur with a constantly dry, harsh skin, and constant clammy perspirations are sometimes present in dysentery without abscess.

Jaundice, or yellow suffusion of the eye, was recorded only in 9 cases out of 300, or in three per cent., and as this symptom is not easily overlooked, it is but fair to conclude that in most, if not all, of the remaining 291 it was absent. In our cases it was present in 1 out of 20 cases, or five per cent. In this case in which it occurred the abscess was seated near, and compressed, the common duct.

Vomiting and nausea were present in no less than 74 out of 84 cases; and it would seem that the former symptom was present in 69, and the latter (without the former) in 5. From an analysis of the site of the abscess in these cases, Mr. Waring concludes that vomiting is more common when the left lobe of the liver is affected. An examination of our more limited series of cases does not accord very closely with this, as vomiting was present in less than half the number, and in several cases the abscess lay completely over the stomach without exciting vomiting. Possibly, in many of Mr. Waring's cases in which no notice is taken of the symptom, it may have been absent.

Cough also appears to have been a more common symptom than we should have supposed, from the analysis of our cases.

Evident enlargement of the liver, as judged of by palpation and percussion, was present in 90 out of 101 cases in which the symptom is referred to. This is, in fact, one of the most important signs connected with abscess, though when the abscess is small and central, or seated towards the superior surface, enlargement may be slight, or may be wanting. Still the fact that enlargement was present in no less than 90 per cent., gives this sign a high diagnostic value.

The condition of the urine is described very imperfectly. After a table enumerating all the facts noticed in the cases (amounting only to 45), Mr. Waring continues:

> "There is nothing pathognomonic in the character of the urine, in cases of hepatic abscess. Its most frequent state is high coloured and scanty, or containing a white deposit, commonly considered as purulent or puriform in its nature. It is worthy of remark, that in those cases in which the urine was purulent there was no communication between the abscess and the kidneys. In what manner, therefore, was the purulent matter eliminated from the system through the kidneys? During the progress of a case, the urine assumes very different appearances at different times." (p. 155.)

With respect to the deposit called "puriform or purulent," we need only remark that in by far the greater number of cases, if not in all, it is simply phosphatic, from alkalinity of the urine produced by decomposition of the urea, or it is a mixture of the phosphates and vesical mucus, or vesical pus. The old notion, that the pus of an hepatic abscess could be absorbed and eliminated through the kidneys without change of form, is quite given up. No reference is made to the still doubtful point, whether or not the urea is diminished in great hepatic abscess.

The stools are described very imperfectly in the cases. Mr. Waring thus sums up the facts of 176 cases:

Stools, more or less vitiated, deranged, or unhealthy, in	46
„ dysenteric	36
Free, regular, natural, feculent	20
Diarrhœa, chiefly colliquative, and towards the close of the case .	15
Bowels constipated, requiring purgatives to procure evacuations .	6
Stools contained purulent matter	16
„ „ coagula of blood, or grumous blood	15
„ black and watery, resembling coffee grounds	6
„ perfectly white, latterly (case 160)	1
Bowels irregular, sometimes relaxed, at others costive, &c., in . .	12
„ irritable	3
	176

He then remarks:

"There are two forms of stool from which an unfavourable diagnosis may be drawn. 1st. Black, watery stools, like coffee grounds. 2ndly. Stools containing coagula of blood, or grumous blood; these are rarely observed till towards the

> close of the case. With respect to those cases in which purulent matter was passed in the stools, it was very evident that in some instances this depended upon the abscess in the liver opening into some part of the intestinal canal; but in others, no distinct communication was found to exist after death, between the abscess and the intestines, although in some the abscess was adherent to some portion of the intestinal parietes. In the remaining cases, no apparent communication whatever appears to have existed. The question, how, under these circumstances, purulent matter was passed by stool, remains to be solved by future inquirers." (p. 156.)

In these cases we believe that the pus is simply furnished by the ulcerated and thickened colonic mucous membrane, at least it was so in the cases noticed by us.

The pulse, tongue, and skin presented no symptoms of great importance; the pulse is noted as quick in most cases; two exceptional instances are referred to, however, in which the pulse averaged only 65 and 58 per minute. The tongue is usually furred; "the smooth shining tongue" described by Annesley is noted only in 5 cases.*

Having thus considered the symptoms, Mr. Waring proceeds to examine the only other point for which his cases give him the requisite materials —viz., the results of

The operation of puncturing the abscess.—During the present century, in India, 81 cases in which abscess of the liver was punctured have been recorded. There were—

Recoveries	15	or 18·519 per cent.
Deaths	66	or 81·481 per cent.
	81	100·000

The average duration of life after the operation was eighteen days; 1 patient died the same day; 7 the day after; 4 lived to the fifty-first day.

The circumstances which induced the fatal issue are thus tabulated:

The presence of other abscesses in liver besides the one opened . .	19
A combination with dysentery	17
Gangrenous or sloughing condition of the abscess walls, &c. . .	4
Abscess communicating with the lung	2
,, ,, ,, colon	1
,, ,, ,, pericardium	1
Two other abscesses opening spontaneously	1
Another abscess bursting into the abdominal cavity subsequent to the operation	1
Impervious state of the hepatic ducts, delirium	1
Escape of matter through the puncture into the abdominal cavity, causing excessive inflammation	2
Abscess making its way by ulcerative absorption through the coats of the stomach	1
Hæmorrhage into the sac of the abscess	1
	51

In *none* of 15 cases of recovery was the abscess combined with dysentery.

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* With respect to the possibility of producing mercurial salivation in hepatic abscess, Mr. Waring remarks that the results in 54 cases were—salivation in 18, soreness or ulceration of the gums in 17, no effect on the mouth in 19. Nevertheless he still thinks, "when abscess is formed it is a matter of great difficulty to bring the system under the influence of mercury." In this opinion we cannot agree.

Mr. Waring thus gives his conclusions on the propriety of the operation:

> "*The proportion of cases of hepatic abscess which hold out a reasonable hope of cure by the performance of an operation for evacuating its contents*, is much smaller than most persons are aware of. There are two conditions or circumstances which preclude the probability—it may almost be said, the possibility—of the operation proving successful. 1. The existence of a plurality of abscesses. 2. An ulcerated state of the large intestines, or in other words, the co-existence of dysentery. Let us see in how many of the 300 fatal cases, given in the first part of this paper, the operation under these circumstances would have been applicable.

Total number of cases	300
There were a plurality of abscesses in	108
	192
Of the 177 cases in which the abscess was solitary, there was more or less extensive ulceration of the large intestines in	76
	116

> "Thus it appears, that out of the whole number, only in 116, or little more than 1 in 3, could the operation have been undertaken with any reasonable probability of success; and this number would, of course, be still further diminished, by taking into consideration the cases in which the abscess, though solitary, communicated with the lung, colon, or some other viscera, and those in which other organic disease existed.
>
> "Important, as it must be admitted, statistical data such as the above are, in all disease, yet too much reliance should not be placed upon them when the question actually presents itself, whether or no an operation should be undertaken in any individual case. Under such circumstances, the surgeon must rely on his own judgment as to the propriety of undertaking it; the condition of the patient, his strength of constitution, the extent of the disease, the complication with other lesions, &c., all these should have the first consideration." (pp. 180-81.)

This is certainly a very unfavourable judgment, but we are inclined to think it a correct one. The fact that dysentery was not present in any of the cases of recovery is a most valuable one, and shows what cases should be selected and what avoided. It is, perhaps, in part owing to the absence of dysentery, that puncture of hepatic, simple, and hydatid cysts in this country so often does well.

There is one argument for the operation not mentioned by Mr. Waring—viz., the great temporary relief which is sometimes given by it. On this account alone, and without much hope of recovery, we should not hesitate to perform the operation whenever the dysenteric affection is not very severe.

Mr. Waring concludes his most instructive work by an account of *Twenty-five cases of spontaneous recovery.*—The mode of recovery was as follows:

By the abscess opening into the lung, and the matter being discharged, by expectoration, through the bronchi	10
By the abscess opening into the colon or some part of the large intestine, the contents being voided by stool	7
By one abscess opening into the lungs, and another into the colon	1
By the abscess discharging its contents into the abdominal cavity (?)	3
By the elimination of pus by the kidney	2
By spontaneous absorption (?)	2
	25

The diagnosis of some of the cases under the three last headings may be doubted.

Mr. Waring seems to entertain little doubt that the pus of an hepatic abscess is eliminated through the kidneys, but, as already said, we cannot accord in this opinion; and we believe that the two cases referred to, in the table given above, were very probably merely cases of wrong diagnosis. Besides, in neither case was the purulent nature of the white sediment noticed in the urine proved by microscopic examination; and therefore we can have no hesitation in putting out of court all evidence hitherto given on this point, as too unsatisfactory to be considered.

We have now given a full abstract of the contents of this very excellent treatise. Although the meagre clinical details of many of the cases detract considerably from their value, we are yet sensible of the deep importance of those facts which are furnished by them. Mr. Waring's labour has not been thrown away; and his work is, as far as our own judgment goes, one of the most useful which have been published for a long time on hepatic abscess.

E. A. Parkes.

Review II.

1. *Die Hämodynamik nach Versuchen.* A. W. Volkmann. (Cap. xii. *Von den Kräften welche das Blut bewegen*).—*Leipzig,* 1850.

On the Forces which move the Blood. By A. W. Volkmann.

2. *Le Système Capillaire Sanguin.* Thèse par L. A. Segond.—*Paris,* 1853.

On the Sanguineous Capillary System. By L. A. Segond.

3. *Principles of Comparative Physiology.* By W. B. Carpenter, M.D., F.R.S., F.G.S. (Chap. v.)—*London,* 1854.

(*Concluded from No.* 30, *p.* 390.)

In our last number we reviewed the arguments in favour of a special force in the capillary circulation. We shall now consider those which are opposed to the doctrine. (See No. 30 of this Review, p. 373.)

1. This argument has been included in the previous sections. The various objections which can be urged against each statement have been already considered.

2. The following are the well-known experiments which Dr. Sharpey performed:

> "A syringe, having a hæmadynamometer attached, was adapted to the thoracic portion of the aorta of a recently-killed dog, which vessel had previously been ligatured immediately above the renal arteries, and the *vena cava inferior* was opened where it passes through the diaphragm. Beat bullock's blood was then injected and passed out of the *vena cava inferior,* after traversing the double capillary system of the intestines and liver, in a free stream, with a pressure of three and a half inches of mercury; when a pressure of five inches was used, it flowed out in a full jet.
>
> "When the aorta was not ligatured above the renal arteries, but left pervious, so that the blood might flow through the lower extremities, the same pressure was fully sufficient to drive the blood freely through their extensive vascular ramifications.

" If the same instrument be adapted to the pulmonary artery, it will be found that a pressure of from one and a half to two inches of mercury will suffice to drive beat bullock's blood through the capillaries of that organ, so that it may flow in a free stream from the left auricle or pulmonary veins."*

It is added, in a note:

" In performing these experiments, it is necessary that the bullock's blood be previously strained through a thick cloth, in order to remove any small coagula that may be present."

It is assumed in these experiments, that the force employed was certainly not greater than that naturally exercised by the ventricle. There must be considerable difficulty in estimating the force exercised by the heart. Moreover, blood forced through the vessels of a dead animal by the continuous pressure of a syringe, is not a very exact representation of the forces employed in the circulation of the living animal, or of the pulsation of the heart. The two forces cannot be accurately compared. The natural conditions of the circulation are thus far too imperfectly represented. Again, we often see the circulation carried on with considerable vigour, when we know that the action of the heart must be far weaker than natural; for example, in cases of fatty degeneration, &c. (see p. 87); so that, unless we admit that so long as the circulation continues, the force of the heart's action is alone sufficient for the task, the difficulty is not overcome. Mr. Erichsen admits that " these experiments are not conclusive evidence against the existence of any motor power in the circulation besides the heart."†

But it may be readily granted, that in health the power of the heart is alone sufficient to circulate the blood in the higher animals, without compromising the question. Does it follow, that because the heart alone could circulate the blood, that therefore it does act without assistance? This we know to be an error; for it is generally admitted that other forces exist—as the pressure of muscles upon the veins, the respiratory movements, &c. Now, as the existence of these accessory forces is not denied, it is clear that if other facts and arguments indicate the presence of a power at the capillaries, supplemental to the heart's action in assisting the circulation, it will not be opposed by the above considerations.‡ Instances are not wanting elsewhere of a " power in reserve" in the human body. Many beautiful examples are continually presented to our notice, showing clearly that more than the mere necessities of the case have been considered, and in the circulation especially, which is continually varying, exposed as it is to the influence of numerous disturbing causes.

3. A little consideration will render it obvious, that the evidence furnished by this statement, in regard to the question at issue, will depend simply upon the manner in which pressure on the artery controls the flow of blood through the vein. Therefore, we find the above proposition variously stated by different authors; and this variation occurs with respect to the time which elapses between obliteration of the arterial current, and the cessation of the flow in the vein. It is stated by one, that when the current is stopped in the artery, that in the vein *gradually* ceases; while by another, that the circulation in the vein is *completely and at once* controlled by pressure on the artery. Now, so far as the

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* Erichsen, loc. cit., p. 20. † Loc. cit., p. 21. ‡ See Volkmann, op. cit., chap. xii.

present question is concerned, everything depends upon this difference. If the circulation through the vein is *instantly* and *immediately* controlled by stopping the stream of blood through the artery, then assuredly a strong argument is adduced against the existence of a force acting upon the circulation in the vessels between them. This would be strong evidence against the idea that the blood is moved onwards by any force operating at the capillaries. But, on the other hand, if any sensible period of time elapse between the obliteration of the arterial current and the cessation of the flow of blood in the vein, then, on the contrary, strong evidence is furnished to prove, that after the influence of the heart is abruptly removed, some power still exists to sustain for a time the venous circulation.

Now these different statements are founded upon certain experiments, the most celebrated of which is that of Magendie, which is often referred to, and very differently interpreted. He thus describes it:

> "After having passed a ligature round the thigh of a dog, as I just now described—that is, without including the crural artery or vein—apply a ligature separately upon the vein near the groin, and then make a slight opening in this vessel. The blood will immediately escape, forming a considerable jet. Then press the artery between the fingers, to prevent the arterial blood from reaching the member; the jet of blood will not stop on this account, it will continue some instants; but it will become less and less, and the flowing will at last stop, though the whole length of the vein is full. If the artery be examined during the production of these phenomena, it will be seen to contract by degrees, and will become completely empty. The blood of the vein then stops; and at this period of the experiment, if you cease to compress the artery, the blood injected by the heart will enter, and, as soon as it has arrived at the last divisions, will begin to flow again at the opening of the vein, and by little and little the jet will be established as before."*

Now so far as this experiment goes, it certainly proves the very reverse of what is usually ascribed to it. It is to determine whether the blood is assisted in its passage through the capillaries by any local force. It is stated, that if the artery be pressed so as to remove the influence of the heart, "the jet of venous blood will not stop on this account, it will continue some instants." Here, then, we see that the blood still flows in the vein, not only when the influence of the heart is removed, but also when the venous circulation is deprived of any assistance it might derive from the inspiratory acts. If we assume for the moment the existence of a local force moving the blood through the capillaries, we should expect to witness the very phenomena observed in the experiment. The escape of blood from the vein becomes less and less, until the flow at last ceases, the whole length of the vein remaining full of blood, while the artery is found completely empty. The circulation continues through the capillaries so long as these vessels are supplied with blood.

In other experiments we have a repetition of the same phenomena. The flow of blood through the vein does not cease altogether when the artery is compressed, and the passage of blood through it consequently stopped. The explanation which is offered of this fact, that it is due to the elasticity of the venous system, can scarcely be admitted. If this were true, the vein would not remain distended with blood. That a partial compression of the arterial tube should affect the flow of blood in the vein, is what no one would doubt; that the quantity escaping from the vein should correspond with the quantity passing through the artery would be admitted without the evidence of these experiments. "Indeed," as Dr. Alison has observed, "a little management gives nearly similar results in a common bloodletting."* The question at issue is not the extent of the heart's influence; but the existence of a supplemental force in the capillaries.†

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* Précis Elémentaire de Physiologie, par F. Magendie, pp. 290, 291. 1834.

A consideration of these experiments must show, that so far from opposing the doctrine, they ought rather to have been referred to as evidence in support of it. They are not in either view entirely free from objection. The application of the ligature, and, in another experiment, the removal of the main nerve, must interfere more or less with the normal condition of the limb. But however much these disturbances may obstruct, they cannot be conceived to develop a local force; so that these experiments may be fairly applied to, for evidence in support of the doctrine, without overlooking the elastic force of the artery.

Now it may be asked, Do the foregoing facts furnish a satisfactory answer to the present inquiry? Is it venturing too far to state, that a careful consideration of the evidence which has been advanced warrants the conclusion, that in health, as the blood flows through the capillaries it is subjected to the operation of a force acting upon it at this part of its course, and that its circulation through these vessels is thereby assisted? Many doctrines in physiology which are generally admitted are not so satisfactorily supported.

It does not appear possible to estimate very exactly the amount of this force. Its direct influence is evidently confined to the blood while passing through the capillaries. It is not supposed to be in any way capable of maintaining the circulation to any extent beyond these vessels.

It is quite clear that in the higher animals the heart is not only the grand agent in the circulation of the blood, but that no force, nor even the aggregate of all the supplemental forces, is sufficient to maintain the circulation through its entire course independent of the heart's action. Yet, while it is admitted that the injection of blood into the capillary vessels of every part of the system is due to the action of the heart, it is contended, that in these vessels it falls within the influence of a force whose operation is not only capable of modifying the distribution, but also of assisting the progressive motion of the blood.

There appears to be no valid argument against this doctrine. Its truth is not impugned by any fact which has been hitherto established. The evidence by which it is supported, it must be remembered, is founded upon various facts, and ought to be weighed as a whole. It must not be assumed that the doctrine is untrue because a particular fact or argument may appear, when viewed singly, weak or inconclusive. Such an error seems to be not uncommon. It often happens that a cause is more damaged by weak arguments advanced for its support, than by the strongest objections which may be raised against it. For this reason, many points which appeared doubtful have been discarded from the previous inquiry. But it must necessarily occur, that where so many facts are concerned, all will not appear equally valuable and conclusive. For example, some of the facts which have been alluded to, tend only to show the probability that such a force exists, while others, perhaps, are conclusive on this point.

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* Op. cit. p. 27.

† These experiments are quoted, and some other arguments are advanced, to refute the conclusions which are usually drawn from them, by Dr. G. Calvert Holland, in the paper before referred to.

But it has been contended, that what appears to be an active force assisting the capillary circulation, is really only the removal of an obstacle. Although this objection may apply to some instances, yet it will not hold good for the rest. For instance, the fact that the blood is retarded in its passage through the capillaries, in proportion as the changes which it ought naturally to undergo are less perfectly accomplished, is assumed by some to depend only on the presence of a direct obstacle which is thus created, and not on the absence of any normally acting force. It has been argued that no hindrance is presented to the passage of the blood so long as the natural changes occur, but that when these are interrupted, the flow is no longer free. But what is the nature and where is the seat of this obstacle? That it cannot be due to any contraction of the vessels has already been proved.* Under these circumstances, it appears difficult to admit the presence of an obstacle founded on the interruption of the natural changes in the blood, without acknowledging that the changes, when normal, must generate an active force. And then, on the other hand, it has been shown that it is not only possible to retard the flow of blood through the capillaries, but by certain local applications to accelerate its flow. Here, then, is an actual demonstration of an increase of some power, and it only remains to establish its seat. If not at the capillaries, where is it to be found?

Those who are not satisfied with the evidence which has been adduced in support of the existence of this force, seem to expect, that if present at all, it would be present to a degree that could not escape common observation, and that it would be made manifest by more palpable effects. But this, from its very nature, is impossible. However small in amount this force may be at any point or part where we can submit it to experiment, in order to estimate its effects, yet it must be remembered that its extent is co-equal with that of the entire capillary system; and thus, although more or less obscure in any individual portion, the aggregate of the whole must be considered, and its influence measured, not by its concentration at any isolated part, but by the extent of its diffusion.†

In consulting various authors upon this subject, other observations will be found, which are intended to support one or other side of the question. But these, either from their insignificance or inapplicability, are now valueless, or from their uncertainty subject to weighty objections. Such are the following:

In support of the doctrine:

1. When the blood has escaped from its vessels into the surrounding textures, or even when removed from the body, its particles have been observed to continue in motion for some time. This movement has been said to be spontaneous, and not to be dependent on physical laws. It has

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* See Dr. Reid's experiments, &c.

† See Hunter's Works, Notes, vol. iii. p. 231.

therefore been supposed that the blood is endowed with a power of self-propulsion, which is exerted in the vessels during life.[*]

2. In an amputated limb the circulation of the blood has been observed to continue, under the influence of moderate heat, for several minutes; and when the circulation has ceased in the minute vessels, it may be restored regularly and forcibly, and continued for several minutes, by the application of heat.

3. The circulation of the blood in the capillaries, which continues after the removal of the heart's influence, "may be immediately checked by certain applications to the parts themselves."[†]

4. The amount of force which has been found sufficient to stop the flow of blood along the chief artery of a limb, is considered by Sir C. Bell,[‡] Mr. Guthrie,[§] and others, to be exceedingly small "when compared with those obstacles which the blood moving along that artery must encounter in its passage through the smaller vessels."[‖]

5. "The great difficulty of thoroughly injecting the capillaries in a recently killed animal," and the greater or less force required to inject a fluid through the capillaries, according as it is more or less stimulating, most probably depend—as does also the fact described in Hunter's experiment—upon the muscular power of the smaller arteries. (See No. 30 of this Review, p. 380.)

Against the doctrine:

1. Various facts are quoted to show that the influence of the heart's action is extended through the capillaries and into the veins. Besides Magendie's experiment (p. 14), the facts are referred to—that the pressure of the blood in the veins varies with the force of the heart's action—and

> "That the impulses of the heart are visibly continued on through the small arteries and capillaries, and even into the veins in some states of the circulation. This phenomenon is most apparent at the time when the action of the heart is weak; and in such states of the circulation this remittent flow of the blood may be converted into a merely oscillatory movement, without any regular progression, by the gradual increase of the pressure applied to the artery which supplies the blood to the capillary vessels under observation; a fact which shows distinctly on the one hand that the force of the heart is continued on through the capillaries: and on the other, that when a resistance is opposed to the progress of the action of the heart through the arteries, no other force then operates sufficient to cause a continued and progressive motion of the blood."[¶]

Now, the full measure of the heart's influence may be readily admitted without affecting the actual question under consideration (see p. 13), but the other conclusion drawn from the above facts—viz., that when a resistance is opposed to the action of the heart, no other force then operates, cannot be admitted. Neither the heart's action, nor any other force, can overcome every resistance. It is very evident, in the case above quoted, that the venous system would be distended with blood (for no one contends that the circulation could continue naturally in the higher animals without the heart's action), and thus the entrance of more blood would be opposed. This fact is clear from the oscillatory motions observed. It is not proved, even in this case, that no force is operating to move the blood onward, but it is merely shown that it is insufficient to overcome the resistance.*

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[*] See Müller, op. cit., vol. i. p. 222.

[†] Carpenter's Principles of Comparative Physiology, p. 265.

[‡] An Essay on the Forces which Circulate the Blood, &c., by C. Bell, p. 67, et seq. 1819.

[§] Op. cit., p. 226, et seq.

[‖] Alison, op. cit., pp. 29, 30.

[¶] Dr. Allen Thompson, op. cit., p. 672; also, Müller, op. cit., p. 218, &c.

2. The extent of the heart's influence has been illustrated by a reference to the systemic circulation in fishes. In these animals, the capillaries of the gills intervene between the heart and the systemic aorta. So it is argued that the heart's action is here sufficient to circulate the blood through two distinct systems of capillaries. But this is the question. Is it so? Indeed, the circulation in fishes has been referred to as an example where the force of the heart's action alone is manifestly incompetent to circulate the blood through its entire course.

Various attempts have been made to explain the nature of the force thus acting upon the blood in the capillaries. Some of the so-called explanations are little more than mere expressions of the fact. Many of the reasons which have been assigned are vague and fanciful, and are neither compatible with actual observation, nor supported by sound and logical argument. Other supposed causes, even if admitted, would be insufficient to produce the result. Some explanations are very ingenious, and seem only to require proofs in their favour. Indeed, it appears to be much easier to prove any hypothesis false, than to supply its place with one more likely to be true.

Some have assigned the cause of this motion to be in the blood itself, attributing to its particles a power of spontaneous and independent motion. Some, indeed, have not hesitated to declare that the blood is endued with a power of self-propulsion (see pp. 16, 17).

The most recent observations on this point are those of Mr. Wharton Jones. In speaking of the cause of the variations in the capillary circulation, he says,

> "It is interesting to consider the different capabilities for endosmose and exosmose, in reference to the liquor sanguinis, possessed by the red and colourless corpuscles, as indicated by their different states of distension, and to compare this with the difference in the attractions and repulsions they exhibit. The changes constantly going on in the blood are attended with variations in the capabilities of the corpuscles for endosmose, and in their attractions and repulsions. These appear to be the cause of the variations which are constantly occurring in the capillary circulation. The force of the heart alone, and not any action of the capillaries, determines the general passage of the blood from the arteries into the veins; but it is to the attractions and repulsions of the corpuscles that the varied peculiar movements of the blood in the capillaries are owing. In considering the circulation through the capillaries, in short, it is always to be remembered that the blood is not a mere inert fluid, but one containing in suspension innumerable organized and living corpuscles, endowed with peculiar attractions and repulsions."†

Again, the movement has been conjectured to depend on "the electrical relations of arterial and venous blood (the first of which is, in a slight degree, positively, and the last negatively, electrified.")‡

An attraction exerted on the blood by the walls of the capillary vessels has been assigned as a cause,

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* See Magendie's experiment.

† Observations on some Points in the Anatomy, Physiology, and Pathology of the Blood, by T. Wharton Jones, F.R.S.: British and Foreign Medical Review, vol. xiv. p. 600. 1842.

‡ Alison, op. cit., p. 84; Allen Thompson, op. cit., p. 673.

> "But we cannot conceive how such attraction could aid the circulation of the blood, for it would cause the blood to become stationary in the capillaries, unless it be again admitted that this attraction of the capillaries for the blood is exerted only while the blood retains its arterial character, and ceases when it has become venous. It is only by such an affinity that the capillaries could assist the circulation."[*]
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> "Dr. Tanchose suggests, as a cause for the motion of the blood in the capillaries, the ceaseless removal of particles from the blood to supply materials to the various secretions, &c., a constant tendency to a vacuum being thereby produced."[†]

Contractions of the capillaries have been supposed to assist the passage of the blood through them. It is very doubtful what power of contractility the capillaries possess; for although their diameter clearly varies under different circumstances, yet this may be due to the elasticity of their walls, and dependent simply upon the quantity of blood that passes through them.[‡] But, even if it were admitted that the capillaries possess the power of active contractility, it has yet to be shown how this could assist in the propulsion of the blood.

It has been supposed that endosmotic and exosmotic currents, which have been assumed to occur through the walls of the capillaries, between the blood within and the fluids around them, may assist in the movement of the blood. Assuming such currents to exist, the diffusion would be greatly facilitated by the continual flow of blood through the capillary tubes. Fresh portions being thus continually supplied, the difference between the fluids within and around the capillary vessels would be constantly maintained in a high degree, and the tendency to an equilibrium would be thereby opposed. But it is not yet explained how this could contribute to the progressive motion of the blood.

Dr. Alison's explanation of the cause promoting the flow and regulating the distribution of the blood in the capillaries is well known: that it depends on the "vital attractions and repulsions" connected with the chemical changes constantly occurring in the capillaries between the blood and the surrounding tissues, by which nutrition and secretion are effected. This doctrine is illustrated by the phenomena of asphyxia, by the fact ascertained by Dr. Reid, "that when the flow of blood in the systemic circulation becomes decidedly venous, and unfit for carrying on the process of nutrition, it passes less freely through the capillary arteries into the veins," also by the observations of Mr. Wharton Jones. (See No. 30 of this Review, p. 388.)[§]

Professor Draper's ingenious explanation is also generally known. It is founded upon the law "that if two liquids communicate with one another in a capillary tube, or in a porous or parenchymatous structure, and have for that tube or structure different chemical affinities, movement will ensue; that liquid that has the most energetic affinity will move with the greatest velocity, and may even drive the other liquid entirely before it." Arterial blood is thus drawn into the systemic capillaries, by the affinity which exists, so long as the chemical changes proceed between it and the surrounding tissues; and the venous blood in which the changes have already been effected, having no longer any affinities for those tissues, is driven onward into the veins.

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[*] Müller, op. cit., p. 224.

[†] Allen Thompson, op. cit., p. 673; Acad. des Sciences, Séances d'Avril, 1838.

[‡] See Handbook of Physiology, by W. S. Kirkes, M.D., assisted by James Paget, F.R.S., p. 130. 1851. Carpenter: Human Physiology, pp. 498-9. 1853. Wharton Jones: Guy's Hospital Reports, vol. vii. part 1, p. 10.

[§] Alison, op. cit.; Reid, op. cit., pp. 41, 42.

> "The relation of arterial and venous blood respectively to those tissues comes to this—the affinity of arterial blood is expressed by the affinity of oxygen for carbon and hydrogen—the affinity of venous blood, by that of carbonic acid for carbon, and of water for hydrogen. Compared together, therefore, the former is the representative of a highly energetic force, which in the latter is diminished down to zero. . . . . The oxygenizing action of arterial blood is, therefore, the true cause of the systemic circulation."

In the pulmonary circulation the reverse obtains; here the force is the affinity of venous blood for oxygen, which will be drawn onward, driving the arterial blood before it, in which such affinity has ceased. In the liver also the blood of the portal vein, which contains the materials from which the cells are developed and the secretion formed, is attracted onward, and carries before it the blood from which such elements have been removed.*

The explanations advanced by Dr. Alison and Professor Draper are by no means opposed. Indeed, the idea of Dr. Alison appears to be the foundation of Professor Draper's views. It may be said that the former clearly defines the cause, and the latter shows more particularly how it operates. The doctrine, that the cause of the force moving the blood in the capillaries is to be found in the chemical and physical changes that attend the process of nutrition, secretion, &c., is quite consistent with all the facts of the case, and offers an easy and satisfactory explanation of the various phenomena observed. For example, it tells why "the supply of blood in a part is proportionate to the activity of its changes, and not to its mere structural development." It explains why the flow of blood through the capillaries should continue for a while after the removal of the heart's influence. It accounts for those variations in the velocity and direction of the capillary currents which have been so often noticed, and the varying activity of the circulation in different parts or organs at different periods. It explains the increased or diminished flow of blood to parts under different circumstances, without the necessity of being driven to assume that the arteries are active agents in the change. The phenomena of asphyxia are thus completely accounted for, and its application to some other morbid conditions will be presently noticed. And while explaining, it derives support from these and other facts. These changes are doubtless of a much more complex nature than Professor Draper represents them to be, "every distinct organ attracting to itself the peculiar substances which it requires as the materials of its own nutrition, and the nature of the affinities thus generated being consequently different in each case."† Thus, "it appears that the conditions necessary for the energetic flow of blood through the vessels, are nothing else than the active performance of the nutritive and other operations, to which its movement is subservient."‡ No support can be given to any doctrine which would place an active force in the walls of the capillaries themselves—there are no facts upon which such a theory can be based. Many arguments have been directed, not so much against the general question, as against this or any other particular point. But arguments opposed to such unstable hypotheses are not valid when applied against the actual presence of a force which, from the previous considerations, is supposed to exist at the capillary vessels, not resident in, but operating through, their walls; which is "exercised, not by the capillaries, but by that relation, whatever be its nature, which exists between every tissue and the blood, and by which the condition of the tissue determines the quantity of blood to be supplied to it, as in the rudimental state the condition of each organ or tissue determines the first formation and supply of blood to it."* In short, which is dependent on those changes to which the blood is subjected as it flows through the capillaries.

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* A Treatise on the Forces which produce the Organization of Plants, &c., by John William Draper, pp. 35, 36, &c. New York, 1844.

† Carpenter: Human Physiology, p. 501.

‡ Carpenter: Principles of Comparative Physiology, p. 267.

From the foregoing considerations are we entitled to conclude:

1. That during the passage of the blood through the capillaries, it is subjected to an influence which affects its circulation?
2. That this force or power thus operating on the blood in the capillaries, contributes to its progressive motion through these vessels?
3. That this force is dependent on those changes between the blood and the surrounding tissues in which the process of nutrition, secretion, &c., essentially consists?

When we consider the close connexion which necessarily exists between the capillary circulation, and the various forms of morbid action and their results which constitute disease, we cannot fail to perceive the importance of this doctrine when applied to pathology.

In conclusion, a few instances are selected to illustrate the explanation which it appears to furnish of various phenomena observed in, or of the nature of, different diseases; and these are now alluded to not only as examples of its practical application and immediate importance in medicine and surgery, but as additional evidence of its truth which the observation of disease and its consequences reveals.

There are some well-known instances on record in which the heart has been absent during the whole of fœtal life, in which instances, nevertheless, the greater number of the organs were generally well developed, with the exception of the brain. (Rokitansky.) With such monsters a perfect twin fœtus has existed, and the circulation in the former has been attributed to the heart of the latter. In the well-known case recorded by Dr. Houston,† however, it cannot be conceived that the circulation in the monster was effected by the heart of the twin fœtus. To what agency can the circulation in such a case be attributed?

Whatever power may be attributed to the heart's action when healthy and natural, yet its structure has occasionally been found so diseased, so degenerated, and that with so little interruption to the circulation during life, that it can scarcely be conceived that this organ was the sole propelling force. Such cases are familiar to every one, and offer a very strong objection to a previous argument. (See pp. 12, 13.)

How is the well-ascertained, ubi stimulus ibi fluxus, to be explained? How is the increased vascularity of a part which so immediately follows the application of some local irritant to be accounted for? Can it be imagined that the application of a stimulus causes a relaxation of the coats of the vessels, and thus allows an increased quantity of blood to pass into them? Can this account for the increased rapidity of its flow also?* Besides, this supposition is clearly opposed to analogy. How could such a result be thus produced? Is it not the effect of a stimulus to augment for a time the functional energies of the tissues to which it is applied—to increase the demand for blood—so that a greater flow and a more rapid current is determined to the part, as a consequence of this exaltation of the affinities naturally existing between the blood and the tissues?

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* Kirkes, op. cit., p. 130.

† Dublin Medical Journal. 1836.

"This would give us a clue to explain the two kinds of congestion long recognised by practical men, the passive and the active forms. The former is owing simply to a relaxed, flaccid state of the parietes of the bloodvessels, which permits them to *receive* a greater quantity of blood from the heart, and to a *minus* state of the capillary force—the other to a *plus* condition of that force—in virtue of which the tissue *attracts* a greater quantity of blood."†

Upon this view, the question may be asked—How far is it consistent with sound principles of pathology to treat local congestion, the result of the application of some irritant, by general depletion? We should expect, and experience confirms the belief, until the effects of the stimulus have ceased, to produce less effect upon the contents of the congested vessels, than upon the circulation in healthy parts, by diminishing the amount of blood in the system, and the force of the heart's action, by general depletion. While blood can be obtained, it is attracted more powerfully to the diseased part than elsewhere, for the reason previously given, and is, therefore, less dependent on the condition of the general circulation.

With regard to the great question of inflammation, Mr. Paget's observations relate so directly to the present inquiry, that we cannot resist quoting them. After describing the phenomena observed in an inflamed portion of a bat's wing, he says:

"Now, to what can we ascribe these changes in the movement of the blood?

"It has been commonly said, that as the vessels contract, therefore the movement of blood becomes more rapid in them, as when a river entering a narrow course moves through it with a faster stream; and that then, as the vessels widen, so the stream becomes, in the same proportion, slower. But this is far from true. The stream becomes slower as the artery or vein becomes narrower by contraction; and then, as the tube again dilates, the stream grows faster; and then, without any appreciable change of size, it may become slower again, till complete stagnation ensues in at least some part of the bloodvessel. I think I can be quite sure that the velocity of the stream, in any vessel of an inflamed part, is not wholly determined either by the diminution or enlargement of the channel, or by the stagnation or congestion of blood in the vessels beyond. That much of the change in rate of movement depends on these conditions, cannot be doubted; and it may seem unnecessary to question their sufficiency for the explanation of that change, after Mr. Wharton Jones's observations. But I think other forces must still be considered, whose disturbance may contribute to the result. Whether we name it vital affinity, or by any other terms, or (which may, as yet, be better) leave it unnamed, I cannot but believe there is some mutual relation between the blood and its vessels, or the parts around them, which being natural, permits the most easy transit of the blood, but, being disturbed, increases the hindrances to its passage. Such hindrances appear to be produced by the addition of salts of baryta, or of potash, to the blood; and by an excess of carbonic acid in the blood, that should traverse the minute pulmonary vessels. The presence of an excess of urea in the blood probably produces the like effect: and some of the facts connected with other than traumatic inflammations appear quite inexplicable without such an hypothesis as this. At any rate, the belief that the more or less rapidity of movement of blood through small vessels may depend on other than evident mechanical relations, cannot appear absurd to any one who has seen the movements of fluid in the Chara or Valisneria, or any such plants, in which a circulation is maintained without any visible source of mechanical power."*

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* See Wharton Jones: Guy's Hospital Reports, p. 18.

† Todd and Bowman's Physiology, part 4, § 1, pp. 373-4.

The synopsis of the first lecture began thus: "Inflammation to be studied as an altered mode of nutrition in the part."

No hypothesis accords so perfectly with all the phenomena which inflammation presents as this. For example, inflammation may result either from a local disturbance or from a morbid condition of the blood. In either case, the mutual relations between the blood and certain tissues would be no longer natural. This theory is also consistent with the subsequent increased alteration in the character of the blood. Again, some of the signs or effects of inflammation may be found where there are naturally no bloodvessels—as in the cornea, vitreous humour, articular cartilages—yet probably not "without enlargement of the vessels of the adjacent parts, and especially of those vessels from which the diseased structure derives its natural supply of nutritive material." (pp. 294, 295.) Such examples might be greatly extended.

Cases of gangrene of a portion of the lower extremities have been recorded, in which the death of the part seemed clearly to depend upon a local decline of the circulation, and in which no obstruction could be discovered in the vessels of the part. A remarkable case of mortification, after fever, of both feet in succession, is recorded by Dr. Houston.† Speaking of the leg which was last amputated, he says:

"The injection passed along the anterior tibial artery, through all the vessels of the foot, even with more minuteness than in the former instance, and after entering the anastomosis of the posterior tibial artery, flowed out in a retrograde direction at the inner ankle, from the remote extremity of the aperture produced in that artery by the ulcerative process.

"Why," asks Dr. Houston, "did not the blood impelled by the heart of this man flow into all these open vessels with the same readiness as the injection, seeing that there was no coagulum or other mechanical cause to impede its entrance? Why was there no hæmorrhage from the open though dead mouth of the posterior tibial artery, when the injection found egress therefrom with so much facility? The answer is obvious—the life of the part being extinguished, the *vital attraction*, by which the blood was induced to enter, had ceased to operate, and that fluid had deserted the vessels; the heart, with all its power, was not competent to overcome this negative obstacle."

Dr. George Johnson gives the following explanations of the proximate cause of albuminous urine, which, he remarks, were first suggested by the experiments of Dr. John Reid (*op. cit.*). The observations of Drs. Reid and Alison, he says, will assist us to ascertain the immediate cause of albuminuria and the dropsy which are so commonly associated with diseases of the kidney.

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* Op. cit., vol. i. pp. 310, 311.

† Dublin Medical Journal, pp. 217—219. 1836.

> "Assuming that the renal circulation is affected by an imperfect elimination of the urinary constituents, in a manner analogous to that in which the pulmonary circulation is influenced by the retention of carbonic acid in the blood, we should expect to find that the circulation would first be retarded in the intertubular capillary vessels. The obstruction, which would be in proportion to the extent of morbid change in the contiguous tubes and cells, would, of course, exert an influence extending backwards in the order of the circulation, so that the Malpighian capillaries, and the arteries which supply them, would become gorged with blood; this engorgement being exactly analogous to that of the right side of the heart, and of the venous system in animals after death from asphyxia."

Dr. Johnson then appeals to various facts and experiments in confirmation of this explanation. That the blood is impeded in its passage in the intertubular capillaries, and that the impediment re-acts backwards upon the Malpighian capillaries, he refers to the satisfactory and conclusive evidence afforded by the condition of the renal bloodvessels in cases of nephritis, both acute and chronic.

> "The hypertrophy of the renal arteries is analogous to that of the right ventricle of the heart, occurring, as it so commonly does, in connexion with emphysema of the lung and chronic bronchitis. In these cases there appears to be an impeded circulation through the pulmonary capillaries, consequent upon long-continued imperfect aeration of the blood; and hypertrophy of the right ventricle is a natural result of this impediment."

That an albuminous condition of the bile is extremely rare in diseases of the liver, is explained by the fact, that the liver has not two sets of capillary vessels. For further detail I must refer to the original source.*

The inefficiency of the attempts to cure bronchocele by tying the arteries supplying the gland, is generally acknowledged. Is not the failure of this plan of treatment to be attributed, in great measure, to the unusual activity of the nutritive process in the hypertrophied gland causing a corresponding increase in the demand for blood? The collateral channels through which the blood is powerfully drawn by the exalted forces operating in the part, are rapidly enlarged, and furnish a supply dependent on, and soon equal to, the demand.

Hence, also, the great importance, and the frequent difficulty, of *completely* strangulating morbid growths, of obliterating *all* the vessels supplying them with blood, when we wish to destroy their vitality.

The above cases furnish additional proofs that the amount of blood flowing to a part depends on local causes, and that the enlargement of the supplying vessels is a consequence, and not a cause, of the local increase.

It is a well known fact, that parts which are completely severed from the body will, under favourable circumstances, re-unite, and maintain their vitality. That this happy event may ensue, it is of course necessary that the circulation be very rapidly re-established in the separated part. But how can this be imagined to occur by virtue of any power from behind? Can it be believed that the blood again enters the vessel of the part simply impelled by the action of the heart? It is, indeed, difficult to account for the facts of this case, without admitting the agency of an attracting force; and hence the necessity of maintaining this in full vigour, by warmth, &c., and the inutility of attempting restoration when it has ceased to exist.

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* Diseases of the Kidney, &c., by George Johnson, M.D. 1852.

Finally, it has been clearly ascertained, that processes of nutrition and secretion have been continued after the heart has ceased to act—i.e., after what is ordinarily termed death. Urine, sweat, and other peculiar secretions, have been formed by their glands; and this could scarcely occur unless the capillary circulation were still continuing.*

In discussing a doctrine like the present one, it must be of course extremely desirable to keep, as strictly as possible, within the limits of the question under consideration, and to avoid entering upon other matters, which, from the close connexion they may appear to have to the especial subject, seem naturally to demand a corresponding amount of attention. For instance, the influence which is undoubtedly exercised upon the capillary circulation, through the medium of the nervous system, might seem, at first sight, an indispensable element of the present inquiry. But, after a careful consideration, it has appeared to us that the question would be much complicated, and the inquiry not in any way assisted, by the introduction of this or any other subject, which, however closely connected with some of the phenomena which have been referred to, are nevertheless not directly concerned in the present investigation.

But, in considering questions such as this, it is impossible to exclude altogether what, at first sight, may appear to some to be irrelevant, or at least unnecessary detail. For the various actions which are simultaneously proceeding throughout the body, and which together constitute its life, are so closely associated, so intimately connected with each other, that, even if it were desirable, it is not possible to isolate, for a separate consideration, any one from the rest. And in the higher forms of life, more especially, not only is the difficulty of investigating each function as well as structure enhanced by its increasing complexity, but, yet further, by its closer relation with, and more immediate dependence on, the others.

William S. Savory.

Review III.

Mémoires de l'Academie Impériale de Médecine. Tomes XVII. & XVIII. *Paris,* 1853-4. 4to.

The two volumes of the French Academy 'Transactions' that have accumulated upon our hands contain some interesting prize essays; but, upon the whole, the value of their contents is somewhat below the average, while some of the papers are most inconveniently lengthy. The most valuable essay, and the one to which we will devote most attention, is the one—

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* See Carpenter's Human Physiology, p. 496.

On White Swelling. By M. A. Richet.

M. Richet believes that many erroneous ideas prevail respecting the pathology of articular diseases, which has been retarded by the uncertainty that has so long prevailed in respect to certain points of structural anatomy—as, e.g., the vascularity and nutrition of the cartilages, and their investment with synovial membrane. The microscope has now led to the solution of these questions, and, by availing himself of the results of its teaching, M. Richet believes that he can steer clear of many of the difficulties that beset the path of earlier pathologists. His own researches date some time back, he having published upon the subject of white swelling in 1844.

While approving the mode adopted by Brodie and Velpeau, of viewing these affections through the pathological changes they give rise to, he believes that they and other writers have not sufficiently followed the succession of these changes as they are observed in the different tissues. His object is to supply this deficiency, in showing that the different stages of the same chronic inflammation have been mistaken for special affections. He believes that all "white swellings" may be included under two fundamental varieties—viz., chronic inflammation of the synovial membrane and of the articular extremities of the bones. The changes which take place in the fibrous tissues are, as Brodie has advanced without demonstrating, always consecutive, while ulceration of the cartilages is not admitted by M. Richet. The synovitis, osteitis, or osteo-synovitis, may undergo modifications, by the constitution and temperament of the individual, or by the causes that have induced them, such modifications being of the more importance, inasmuch as they often affix a special seal to these affections, causing them to be regarded as distinct maladies.

The Pathological Anatomy of White Swelling, constituting the basis of the essay, is given in minute detail; but we are only able to glance at some of the more salient points. M. Richet has, during several years, taken every opportunity offered by cases of arthritis or experiments on animals, of tracing the progressive changes that take place in the *synovial membrane*. Between the fifth and twelfth day after irritation has occurred, a pseudo-membranous-like exudation is effused from its surface, and becomes attached to granulations that are there more or less developed. When the chronic stage of synovitis arrives, these granulations may expel the false membrane covering them, and become themselves developed into fungous vegetations; or the pseudo-membrane may become more and more intimately united to the surface of the synovial membrane, undergo organization there, and prevent the farther development of the granulations. The two cases are respectively termed by the author—*Fungous Synovitis*, and *Pseudo-Membranous Synovitis*.

In *Pseudo-Membranous Synovitis*, layers of pseudo-membrane, intimately connected with vessels, unite the synovial membrane to the fibrous tissues of the joint; and when sufficient irritation is not induced to cause death or amputation, a natural cure may take place through the agency of a fibrous transformation. Retraction ensues, and all the soft parts becoming closely applied around the ends of the bones, the joint then appears less than the opposite one. This form of synovitis is not infrequent after acute rheumatism, and it constitutes one of the varieties of incomplete anchylosis.

In *Fungous Synovitis* the granulations, in place of becoming organized, become, under the influence of a special diathesis (as, e.g., the scrofulous), œdematous and fungoid, and are, after different periods in different individuals, converted into reddish, softish vegetations, analogous to those which spring from carious bone. Containing some arterial vessels, they are chiefly made up of a venous network, in the midst of which is found a translucent jelly, exhibiting small spots like extravasated blood. The vegetations present an epithelial layer at their surface, and within, the fusiform fibres and elongated nuclei characteristic of fibro-plastic tissue. Brodie and other pathologists, from want of having sufficiently studied the alterations of the synovial membrane, have made of this a special affection, of a malignant nature. Occasionally, it becomes arrested in its course, a conversion of the fungosities into fibro-cellular tissue taking place, and a more or less complete anchylosis ensuing. In other cases, the fungosities become indurated, having cartilaginous plates diffused amidst their tissue. This induration is, however, only observed here and there, amidst the thickness of the changed synovial membrane, and gives rise to the development of irregular, so-called foreign, bodies, varying in size and density, being sometimes found loose in the joint, or only attached by a pedicle.

Brodie admits primary *ulceration* of the *synovial* membrane; but the cases he adduces are too briefly narrated to justify the appellation. In M. Richet's opinion, the most frequent sources of ulceration are small centres of suppuration, met with in the altered synovial membrane, which, by breaking both externally and internally, establish sinuses, the extremities of which have all the characters of ulcers. The synovial membrane may also become ulcerated consecutively to the morbid changes taking place in it, while, when much distended with fluid, a sudden movement may cause its rupture.

The *Fibrous Tissues* are endowed with a very feeble vitality. The author's researches lead him to regard the ligaments as insensible, although, as their insertions into the bones are continuous with periosteal or osseous tissues, tearing or stretching these may cause pain. By no experiments has he been able to induce inflammatory action in the ligaments or capsule, even when the synovial was quite red, and the joint full of pus. When, in exceptional cases, they do become somewhat reddened, it is not the redness of inflammation, and it is almost always consecutive to lesions of surrounding tissues. One of the changes most frequently met with is a puffiness of the capsule and ligaments, produced by serous infiltration into the inter-fibrillary cellular tissue which separates the ligamentous fibres, these assuming, also, a dull tarnished appearance. This relaxation allows of great separation of the articular surfaces. At a later period, the fibrous parts become hypertrophied, or even indurated.

The Articular Extremities of the Bones.—M. Richet believing that, however well *osteitis* in general has been described by Gerdy and Miescher, the form that affects the spongy tissue in the vicinity of joints is imperfectly known, describes it, from his own observations, with a minuteness that defies our following him. It must suffice to say, that he admits three stages of primary osteitis. In the *first* of these, a section of the bone presents a vascular surface and enlargement of the cells, its compact surface being pierced with numerous minute holes for the passage of vessels. The secretions of the periosteum become diverted to the surface, and the bone is increased in size, in consequence of new layers deposited at its surface, as well as by the enlargement of its cells. Although such enlargement of bone is not admitted by Crowther, Russel, and S. Cooper, M. Richet has proved its existence by admeasurement, after separation of the soft parts. In *primary* osteitis, periosteal effusion is, however, not constant, occurring only as the inflammation approaches the surface; but in *secondary* osteitis, it is the earliest change observed. In the *second* stage, the red colour is concentrated at certain points, little collections of blood taking place. The cells become more and more spacious, and true abscesses are formed within the bone. Sometimes, however, hypertrophy of the inter-cellular parietes leads to a diminution in the size of the cells, and the spongy tissue is resistant instead of yielding. In the *third* stage, the pus which had been infiltrated into the cells destroys the vessels, and the lamellæ, deprived of nutriment, become necrosed. At other times, ulceration, terminating in caries, occurs, and bleeding fungosities, or vegetating granulations, spring from the cells. Sometimes the cartilage is perforated only in places, at the bottom of which bleeding vegetations are seen, an appearance mistaken by Brodie and others for true ulcers.

Arrived at this stage, it is rare for the osteitis to be confined to the articular extremities, and, on cleaving the bone, the entire medullary canal is found to exhibit an intense redness throughout its entire extent, while small sanguineous effusions, and the other phenomena of the early stage of osteitis, are observed at the other extremity of the bone, although externally this exhibits no evidence of the change.

Consecutive Osteitis.—As synovitis may be secondary to an osteitis, so this last may supervene upon a synovitis. An *osteo-periostitis* so produced differs much from an osteitis properly so-called, there not being the enlargement of the cells or the vivid injection of the spongy tissue, with its purulent infiltration. It is the periosteum which undergoes the chief alteration, the bone, at a later period, undergoing hypertrophy even to its centre. It is, however, the cartilaginous surface that chiefly suffers, the synovial fungosities rapidly leading to its perforation and destruction, and entering into immediate relation to the bone. The compact lamellæ become necrosed, and the pus penetrates into the spongy tissue, inducing more or less deep-seated changes. Vegetations are often found within the cells; but beneath the fungosities the intercellular lamellæ are found more resistant than normal, while in primary ulcerative osteitis they are softened. Consecutive osteitis is a less refractory disease than the primary, the articular surfaces sometimes becoming covered with fibrous tissue that allows of some movement.

There is another change in the bone which, although not rare, has not been described, and to which M. Richet considers no better appellation can be at present applied than *fatty degeneration.* A few spoonfuls of a sero-sanguinolent or purulent fluid are found in the cavity of the joint, the synovial membrane not exhibiting changes proportionate to the symptoms observed during life. The articular cartilages are eroded, thinned, and perforated by a great number of minute apertures. On raising them, which is easily done, a large layer of blood, having the colour and consistence of currant jelly, is found interposed between the spongy cells and the thinned compact lamella which is detached with the cartilage. On sawing the bone, in place of finding the cells more or less inflamed and gorged with blood and pus, the section presents a yellow colour, deeper as the centre is approached. The cells are enlarged, and pressure by the finger produces slight crepitation, and expels a quantity of yellowish oily fluid. No trace of the smallest vessel can be found amidst the spongy tissue. The medullary canal is abundantly filled with this yellow fluid. The periosteum is not inflamed, no trace of the vascularity seen in osteitis existing, while the size of the bone is diminished rather than increased.

Changes in the Articular Cartilages.—M. Richet regards cartilage as possessing a very rudimentary organization, coming between fibro-cartilage and the products of epidermic secretion. Its mode of life is, as it were, parasitical, living by absorption of the liquids amidst which it is placed, its component utricles or cells operating osmosis. To the question, whether the articular cartilages are susceptible of inflammation, and of participation in the diseases of surrounding parts, M. Richet replies in the negative. By no experiments can vascularity be induced, and no attempts at reparation are found after old injuries. Amidst the completest change in surrounding parts, they exhibit only some roughening or thinning from commencing absorption. Vascularity, supposed to have been seen on their surface, is really due to the development of new vessels in a pseudo-membrane that covers them. Ossification, adduced as a proof of vitality, is never found in the case of true cartilage; but the eburnation of the bony extremities, after the cartilage has disappeared, has been confounded sometimes with this.

The articular cartilages are, however, liable to various kinds of alterations, resulting from perversion of their nutrition, or from mechanical or chemical causes. Among such is a loss of elasticity, noticed by Delpech, and frequently observed by the author. *Ramollissement*, which appears to be another stage of this loss of elasticity, occurs also pretty frequently, especially in those who have died in advanced years. This change, which has by others been termed *velvety*, has been the object of much research by M. Richet since 1840, and he thinks there is always coincident with it a diminution of synovia, probably due to a lessened nutritive activity in the bones and articular secretions. A total or partial *disappearance of the cartilages* seems to be a third stage of these alterations, which, while they cannot be called normal, can yet hardly be described as pathological, as they are met with in persons whose joints were healthy.

As regards the changes in the cartilages of diseased joints, they are due either to loss of cohesion—*ramollissement*—or are characterized by thinning, inequalities, or erosions. The last especially claim attention, as by some authors they have been termed ulcerations. When the cartilage is found roughened and unequal, this is due either to wearing away or resorption exerted at certain points, to a perversion of nutrition due to disease of the bone, or to the presence of abnormal fluid within the joint. As the cartilages live at the expense of the parts which surround and support them, they become more or less changed, according to the duration and severity of the disease of the part.

When we examine a joint that has suffered from white swelling, originating in osteitis or advanced synovitis, we almost always find the appearance as if the cartilage had been irregularly punched out. Around these spots it is quite normal, not having even lost its cohesion and elasticity, unless effusion or other alteration of the cavity of the joint be present. Generally there is more or less synovitis present; but when this is not the case, a superficial examination might lead to the affection being considered a primary lesion of the cartilage. But if the bone be so cleft that the saw fall in the centre of the erosions, osteitis will be found occupying the articular extremity of the bone, and most intense where the loss of the cartilage is seen. Brodie and other surgeons have contended that such loss of substance is due to a primary affection of the cartilage, which, extending to surrounding parts, gives rise to one of the most painful varieties of white swelling. The facts cited by Brodie are valueless, in consequence of the very superficial manner in which the examination of the joints was conducted. The history of the condition of the joints in the aged, in which, when the cartilage is gone, eburnation takes place, a change inducing little or no pain, is contradictory to the accuracy of Brodie's assignation of severe pain as a sign of cartilaginous disease. Osteitis induces the most dreadful suffering.

Symptoms.—While alluding to the gradual manner in which the disease may come on, M. Richet observes, that in the case of such superficial bones as the tibia and ulna, we may often detect early a little puffiness of the periosteum rounding off the edges of these bones that are naturally so sharp and distinct.

The soft parts may be at first more considerably swollen than the articular extremity, the disease then seeming to have more tendency to attack the diaphysis, or even the opposite articular end, as, e.g., the pain and swelling of the knee in coxalgia. M. Richet, several years since, proffered the explanation of this circumstance by the propagation of the inflammatory action along the medullary canal from one extremity of the bone to the other; and all subsequent observation confirms him in its correctness. Such pains are important in diagnosis, showing that we have to do with an osteitis and not a synovitis.

Diagnosis.—In this section M. Richet chiefly occupies himself in pointing out the distinguishing marks between osteitis and chronic synovitis. The latter may be due to a local cause, although its progress is usually dominated by a general one: but osteitis is almost always referable to a general cause. Synovitis often succeeds rheumatism. Osteitis is usually of scrofulous origin. In synovitis, the various symptoms may appear almost simultaneously; but in osteitis they are more gradual and progressive. In synovitis, there is hypertrophy of the synovial without swelling of the bone, and the softened ligaments allow of considerable and abnormal movements, while consecutive displacements occur frequently and rapidly, without deformity of the articular surfaces. In osteitis, there is appreciable enlargement of the bone, the limited motions are terribly painful, and the displacements, which take place slowly, are due to the flattening down of the deformed articulations.

Treatment.—Upon the general treatment of the diatheses upon which the disease depends, M. Richet has not much to say. He speaks highly of iodine and cod-liver oil in certain cases; but he does not find that the former can be used as a substitute for the latter. He considers that the tonic effects of hydropathy are sufficiently shown to induce medical men to avail themselves of its aid. Sea air and mineral waters are useful in appropriate cases. Vegetable tonics are of little use; and iron, to be even harmless, requires care in its administration.

In the local treatment of *synovitis*, although leeches sometimes give great relief, they seem at others to do harm; and when the relief obtained is not prompt, they should be discontinued, as they enfeeble. M. Richet attaches considerable importance to the prolonged use of local baths. He thinks the large flying-blisters, so much recommended by Velpeau, should not be employed until the subacute stage has been reached. The nitrate of silver ointment is very useful, and sometimes dissipates violent and obstinate pains.

In *pseudo-membranous synovitis*, issues and the actual cautery, used transcurrently, may be resorted to when the ligaments are relaxed, and the bones, consecutively inflamed, are nigh luxation. In the *fungoid* form they are indicated early, and must be employed boldly. In this form, too, compression, combined with immovability, is useful. When the synovial membrane is much distended, it should be opened with a trocar. M. Richet has only sometimes derived benefit from iodine injections in fistulous openings; but finds the fungosities that spring up are well treated by the tincture.

Syphilitic Osteo-Synovitis.—M. Richet remarks upon the silence of authors with respect to the influence of syphilis in relation to white swelling, their attention being confined to scrofula and rheumatism. M. Ricord informs him, that although he has met with certain cases of white swelling, the cause of which has been influenced by syphilis, he has never seen any that seemed to have been directly determined by it. He believes, also, that this disease influences the compact rather than the spongy tissue of bone. Notwithstanding this opinion, M. Richet believes that syphilis may alone determine a synovitis or osteitis, and so constitute an important variety of white swelling. Since his attention has been turned to the subject, he has met with several cases of *syphilitic chronic synovitis* of the knee-joint, and reports three of these in the present essay. The effusion takes place gradually, and is liable to intermissions. The skin is never red or swollen, the tumefaction entirely arising from the amount of effusion, which is sometimes great, and the thickening of the membrane. This thickening may assume the form of indurated plates, which soften and rapidly disappear under the influence of iodide of potassium. The pain is not great, and is worst while at rest. Left to itself, it tends to pass slowly into the fibrous condition, producing partial anchylosis. M. Richet feels certain that several white swellings that resist all treatment are syphilitic, although the detection of this cause is often difficult. As the effused fluid has no tendency to become purulent, or the synovial membrane to become fungoid, the occurrence of consecutive osteitis is rare. It is rather the history of the case, than any peculiarity of local symptoms, that reveals its nature; the treatment often becoming the test of the accuracy of the diagnosis.

Osteitis arising from syphilis is a far more serious affection than synovitis, and, in M. Richet's opinion, it is as common. He furnishes the particulars of three cases. The pain is severe, deep-seated, lancinating, and especially nocturnal. It is propagated along the shaft of the bone, which is swollen and tender. On motion, it is very severe, and sometimes terrible. The articular tumefaction is partly due to enlargement of the bone, and partly to hypertrophy of the synovial membrane. The general symptoms are little marked, but the impoverishment of the blood by the syphilitic poison, and the terrible suffering sometimes induced, may produce emaciation and the straw-coloured skin. There is less tendency to suppurate than in simple osteitis. The pus is viscous, and the sinuses assume the syphilitic aspect. The prognosis is much less serious than in simple or scrofulous osteitis, but more so than in syphilitic synovitis. The preliminary, erratic pains may be mistaken for rheumatism by the most skilful; but in the latter there are febrile symptoms, while the joint is red and swollen from effusion. As compared with simple osteitis, the syphilitic form involves the articular structures earlier, but it does not give rise to local heat and œdema. The pain seems more concentrated in the deep-seated parts, and its nocturnal exacerbation is better marked. In simple osteitis, all the bones constituting the joint may become simultaneously affected; but in the syphilitic form, one bone usually alone suffers, as the femur in the case of the knee-joint.

The essay is illustrated by thirteen fully detailed cases, besides six of syphilitic white swelling, as well as by several good lithographs; and it must be regarded as a valuable contribution to articular pathology.

II. *On the Minute Structure of the Liver, and on the Nature of the Change known as Fatty Liver.* By M. A. Lereboullet.

From this elaborate prize-essay, which occupies many pages, we can only extract the author's summary of his observations on fatty liver.

"1. The fatty degeneration of the liver is due to the accumulation of fat in the biliary cells themselves. 2. Special fatty cells are not formed, as biliary cells would then be found amidst the fatty ones, which is not the case. 3. Nothing authorizes us to admit that fat becomes developed in the interstices external to the cells. 4. The biliary cells may, by the accumulation of fat, acquire double or triple their normal volume, this development of the cells explaining the increased size of the fatty liver. 5. These cells entirely lose their secretory character, and no longer contain biliary granules; the biliary secretion is obstructed, and the contracted gall-bladder contains but little bile. 6. The fatty degeneration induces a decolorized state of the liver, which progresses from the periphery towards the centre of a lobule, giving the organ a spotted and reticulated appearance. 7. The decoloration arises from the development of the fatty cells compressing the portal vesicles, and impeding the circulation in them. 8. In the artificial fattening of geese, the liver only becomes loaded with fat after the other organs of the body, and especially the abdominal viscera, have become saturated with it. 9. The cells of the liver of fattened geese differ from pathological fat cells, inasmuch as the fat that fills the former always retains the form of distinct droplets, accumulated in the cell, to which they give an irregular appearance on distension; while in the pathological cells the fat becomes united into larger and larger drops, until the cell is at last distended by a single one like a balloon. 10. The fatty cells in the goose resemble, as regards the disposition of the fat in the interior, the physiological fatty cells of the fœtus and those of the lower animals. 11. The nuclei of the normal cells, as well as the biliary granules, disappear when the fatty degeneration commences. 12. The degeneration takes place simultaneously throughout the organ, but all the fatty cells do not present the same degree of development. 13. This change of biliary into fatty cells is observed in tuberculosis, cancer, cirrhosis of the liver, &c. 14. The deposition of fat in the cells appears to be closely connected with a diminution of the nutritive process, and consequently of organic combustion, which is the primary condition of that process. When the quantity of oxygen absorbed is less than in the normal state (as in tuberculosis, cancer, and probably all diseases of nutrition); or, when the respiratory elements (fecula, &c.) are taken in too large proportions, the combustion of these substances is incomplete, and the chemical elements which enter into their composition combine so as to form fat, which is deposited in the biliary cells." (tom. xvii. p. 496.)

III. *A Sign of Congenital Syphilis derived from a Special Alteration in the Lungs.* By M. Depaul.

M. Depaul, in the introductory remarks with which he prefaces his account of the new sign, observes that a mother undoubtedly healthy may become infected by an embryo deriving its diseased condition from the father at the time of fecundation. He also regards constitutional syphilis as a much more frequent cause of sterility than it is usually supposed to be, and perhaps more especially when it exists in the man than the woman. When it does not prevent fecundation, it may prove fatal to the infant at various periods of intra-uterine life, or after birth. When death is caused at an early period of pregnancy, no anatomical lesion capable of accounting for this effect can usually be found; but when the disease arrests gestation later, it generally leaves evident traces of its existence. The skin, of all the other organs, is that in which its presence is most commonly manifested, pemphigus being the most characteristic form of disease, as long since pointed out by P. Dubois. In all but two or three of more than forty cases of pemphigus in infants, collected by M. Depaul, constitutional syphilis has been detected in one or both parents. His own observations have not shown him much with regard to the intra-peritoneal lesions described by Simpson as syphilitic; but he has met with cases in which traces of peritonitis co-existed with undoubted syphilitic lesions. He has several times found the fibro plastic deposit in the liver, described by Gubler, the children not usually dying until some time after birth, although contracting the disease *in utero*. He has also seen several examples of abscess of the thymus, regarded by M. P. Dubois, since 1837, as a pathognomonic sign of congenital syphilis.

In 1837, M. Depaul directed the attention of the Académie de Médecine to a change observed in the lungs of children born of syphilitic parents—viz., the dissemination of multiple collections of pus through them; and in the fifteen years that have since elapsed, he has observed at least twenty such cases, the particulars of two of which are here given. Microscopical examination shows that these collections are not tubercular deposits; and M. Depaul believes that it is highly probable that the cases of tubercle in new-born infants given by Billard, Baron, and Husson, were really examples of this affection. It sometimes exhibits itself under the form of [illegible] consisting of infiltrated pus, and at others of true abscesses, surrounded by more or less thickened walls. Formerly, M. Depaul regarded these as the only two forms of the affection, but he has now several times met with another, which may be regarded as its first stage, and which consists in a greyish induration without pus, but attended with a deposit of a considerable quantity of fibro-plastic tissue. Sometimes the lesion occupies very circumscribed spots, but in other cases it is more generalized, invading several lobes. The pulmonary tissue is impermeable to air, even after repeated insufflation. These different degrees of the lesion are not unfrequently met with in the same subject. There are also usually other syphilitic lesions present, as pemphigus, abscess of the thymus, &c.

Various circumstances influence the prognosis in congenital syphilis. When the skin is affected, although the life of the child be seriously menaced, if the nature of the disease be recognised, and the treatment appropriate, it may sometimes be entirely cured. But when organs indispensable for the establishment of extra-uterine life become disorganized, as in this lesion of the lungs, it is obvious that death may ensue from a mere mechanical cause—the air penetrating in insufficient quantity. So promptly, indeed, does death take place, that the practitioner is disarmed; and hence the imperious necessity of combating the syphilis of the parents prior to fecundation, or seeking to mitigate its effects by prompt treatment of the mother during pregnancy. In M. Depaul's opinion, mercurial treatment is sufficiently justified, even if the existence of syphilis can be detected in neither parent, when the above-described change has already been observed in a product of conception.

IV. *On the Etiology of Epilepsy, and the Indications of Treatment furnished by a Study of the Causes.* By M. Moreau.

M. Moreau, as physician to the Bicêtre and different establishments for the treatment of the insane, has seen much of epilepsy. He believes that the very different accounts given by authors of the amount of success attendant upon treatment, arise from different pathological conditions having been confounded together. Resembling each other much in their external manifestations, *true* and *pseudo-epilepsy* may, by hasty observers, be easily confounded. The convulsion, whatever may be its character, and a more or less complete loss of consciousness, do not in themselves constitute epilepsy, properly so called. They may assume an epileptiform mask, and may depend on various morbid conditions, disappearing on the removal of these. The cause is here everything, while the symptoms are of little import.

"Under these same sympathetic forms, a special disease may be also concealed, a deep-seated modification of the nervous dynamism, a special lesion of something unknown but no less real, which is termed a *neurosity*. It is a lesion independent of the various causes which may induce its external manifestation but do not create it, since it existed without and before them, and because, to use the language of the schools, it is *essential*. Here the importance of the cause entirely disappears in presence of that of the symptomatic phenomena. We speak here of exciting not predisposing causes, which, in our opinion, are not separable from the disease itself." (tom. xviii. p. 5.)

It is the confounding essential and sympathetic epilepsy that has given rise, according to M. Moreau, to the vague and uncertain views which prevail respecting the etiology and treatment of the disease. In his view of the disease, it is the predisposing causes of epilepsy that are of fundamental importance, being, so to say, the disease itself; and it is through these alone that the malady is, if anywhere, vulnerable to attack. He considers them at considerable length, and we proceed to reproduce his principal conclusions.

1. *Physiological Predisposing Causes*:—

"(1) *Hereditariness*.—Of all the predisposing causes of epilepsy, the most serious and fertile, the one whose action is most certain and inevitable, attacking the majority of, or even all epileptics, and embosoming, so to say, the secret of the disease, is *hereditariness*—an expression which, in our eyes, comprises the conditions of organization in the double point of view of physiology and pathology of the ascendants and collateral relationships in which the descendants derive their predisposition. Thus comprehended, hereditariness constitutes the essential and truly fundamental part of our work. The principal source of the disease, this it is which furnishes the least vague indications, and those that are most easy of fulfilment for the prevention and cure of the disease." (tom. xviii. p. 17.)

To the term hereditariness, the author gives a far wider acceptation than that generally received—the mere transmission of a similar disease from the ascendants to the descendants. He believes that any abnormal conditions of the nervous system, the precise mode of manifestation of which may differ, may prove efficient; hereditariness, indeed, resulting from such transformation of diseased conditions, playing an important part. Mere simple nervous irritability, slight convulsive movements or tics, have their hereditary influence: and, in fact, nervous disturbances of all kinds, whatever their symptomatic form, whether simple or complex, predispose as much to epilepsy as does epilepsy itself.

Among the predisposing causes we must also range (2) *Drunkenness*. This is well known to be a frequent cause of insanity; and the mutual connexion of all nervous affections, especially of epilepsy and insanity, leads to the expectation of its effect proving alike in both. Again (3), *Phthisis* acts as a predisposing cause, as shown by the number of phthisical persons found among the relatives of epileptics. There seems, indeed, also a remarkable consanguinity between phthisis and other cerebral disorders having an affinity to epilepsy, such as insanity and idiocy.

In support of these views, M. Moreau cites the particulars of 124 cases of epilepsy which have occurred in his own practice. In 44 of these, individuals below the age of puberty, 83 of their relatives (ascending as high as grand-parents, and embracing uncles and aunts as collaterals) furnished 100 pathological conditions. Among 51 male adults, 113 pathological conditions were discovered in 115 relatives; and among 29 epileptic women, 71 such conditions were noted in 57 relatives.

The following are his conclusions upon a review of these cases:—1. The hereditary sources of epilepsy are far more numerous than usually supposed. In these 124 cases, epilepsy itself occurred among the relatives in 30 instances, insanity exhibited itself in one-fifth, hysteria in one-ninth, and paralysis and apoplexy in about the same proportion. In 30 of the cases the influence was traced back to the grand-parents. 2. In the great majority of cases the epilepsy is not the product of any single pathological condition, but of several united, whether occurring in the same relative, or distributed among several. 3. When we observe the relatively large proportion attained by certain conditions, which have not hitherto been supposed to be related to hereditariness—as eccentricity, cerebral disturbances of all kinds, drunkenness and phthisis; and when we observe that these conditions intermingle, alternate with, and replace each other among the ascendants, we feel justified in attributing to them the large part we have. While in these 124 cases, epilepsy manifested itself 30 times, drunkenness did so 24, insanity 26, and phthisis 35 times. 4. Cerebral accidents, paralysis, apoplexy, congestion, &c., were found in 52 instances; paralysis alone occurred in one-twentieth of the cases, and the frequency with which this condition is met with among the epileptic themselves is an additional reason for attributing to it hereditary influence. Forty examples of it were observed among 240 cases of epilepsy at Salpêtrière. 5. These considerations exhibit the powerful special predisposition the majority of epileptics are born with; and it may be predicted that a family having the apparatus of innervation thus injured, must, sooner or later, furnish descendants suffering from epilepsy; and probably there is no other disease that furnishes such strong proofs of its hereditariness. In such persons there is a special condition, an excess of excitability of the nervous system, which perhaps only awaits some insignificant occasion to become transformed into a morbid individuality. 6. We see in this remarkable hereditary predisposition a true neurotic diathesis, which explains the essential character of true epilepsy, and its resistance to curative agents. Epileptiform convulsions may manifest themselves, whether predisposition exists or not; but when the apparatus of innervation is in a normal state, the cause and effects will appear and disappear together. When it is deeply vitiated by hereditary predisposition, it is in vain we remove the occasional cause, for the disease existed in a latent state prior to its advent, and will persist after its cessation.

(4) *Sex.*—It would seem *à priori* that sex should exercise a predisposing influence, and that more women would suffer from the disease than men. Several authors, indeed, have stated this to be the fact; but the records at Bicêtre and Salpêtrière, for a considerable period, show that it prevails much more largely among men. In respect to the influence (5) of *Age*, M. Moreau's experience is in accord with that of other observers. In a total of 995 cases, the disease commenced at birth in 87; before ten years in 306; between ten and twenty, in 364; between twenty and forty, in 170; and between forty and sixty, in 68 cases; the greatest number becoming developed first between ten and twenty years, next between two and ten, and then between twenty and thirty. (6) *Temperament.*—The author's experience goes to show, at least as far as women are concerned, that lymphatic and scrofulous temperaments are most predisposed, the sanguineous coming next.

2. *Pathological Predisposing Causes.*—By those who include sympathetic epilepsy in their consideration, there is no malady or lesion that may not give rise to the disease; but even under the strict limitation imposed by the author, certain diseases may be regarded as predisposing. Among 364 epileptics, convulsions occurred in 79, for a longer or shorter time,

prior to epilepsy manifesting itself. Of 671 epileptic women at Salpêtrière, 61 suffered from some form of hysteria. In most epileptics who suffer from paralysis, this is a co-existent or consecutive affection; but it occurs too often prior to the epilepsy to allow of our denying all causality. Eruptive disease of the scalp and small-pox seem in several cases to have excited some predisposing power. M. Moreau believes an etiological value they do not deserve has been attributed to the pathological changes in the nervous centres, so often observed in epilepsy. Imperfect development or vicious conformation of the cranium is often observed in congenital epilepsy, which is frequently combined with idiocy or imbecility; but on examination of the heads of 500 epileptics in whom the disease became developed at a later period, M. Moreau could detect nothing remarkable. The condition of the system which results from venereal excess or onanism highly favours the operation of slight occasional causes. Like other observers, M. Moreau has met with many cases in which the epilepsy became first developed during sexual intercourse. The chlorotic condition is often observed in female epileptics, and may furnish useful indications of treatment. The nervous condition plays as important a part as regards the individual, as it does in relation to hereditary influence. A remarkable peculiarity of this condition is the tendency the various accidents constituting it exhibit of replacing each other, and undergoing the most varied metamorphoses and unexpected transformations—fully justifying M. Cerise's appellation, *proteiform neuropathy.* Moreover, these proteiform phenomena have not only this disposition to succeed each other, but, what is of far greater consequence, they exhibit a marked tendency to transformation into more serious forms of disease, possessed of more definite, fixed, and complete characters, so as to pass into some of the great neuroses—as hysteria, insanity, or epilepsy.

3. *Predisposing Causes independent of the Individual.*—The author, although convinced that the belief so generally entertained by epileptics, that changes in the moon influence their condition, is a popular prejudice, resolved nevertheless to give the question an attentive examination. He furnishes a detailed table, exhibiting the entire number of attacks 108 epileptics suffered during five years. The total of these amounted to 42,637, of which 16,324 only occurred at the lunar phases, and 26,313 in the intermediate periods. The same tables enable the author to give a similar negative response to the question, whether extremes of temperature exert any influence. The number of attacks which occurred in the different months of the year were remarkably similar. The affection has been sometimes said to be endemic, as in mountainous countries; but it is then frequently associated with idiocy, and may depend on local causes. The operation of endemicity, too, has been confounded with that of hereditary influence.

4. *Occasional Causes.*—So great is the importance of hereditary predisposition, that, in M. Moreau's view, it absorbs all the interest of the case, the occasional or exciting causes being, in its presence, of little importance. With the progress of science, the immense number of those admitted by the older writers have undergone great diminution. On an examination of his own and other observers' statistics, he finds that moral causes seem to have operated in 444 instances as compared with 85 physical; 364 of

these being referable to the influence of fear in some of its varieties. One fact all authors are unanimous in admitting—viz., the great tendency nervous attacks have to be reproduced under the influence of the same occasional causes that first called them forth. M. Moreau believes that the production of congenital epilepsy is not so frequently due to terror on the part of the mother as is generally supposed. He also doubts whether imitation can give rise to true epilepsy, although there can be no doubt of the ease with which the sight of an epileptic paroxysm induces the like in one liable to the disease. As to the influence of physical causes, such as blows or wounds of the head, &c., it is difficult to distinguish this from that of the accompanying mental disturbance. Drunkenness is an exciting, as well as a predisposing cause; and epileptics are, in general, much disposed to the abuse of alcoholic fluids. In respect to epilepsy excited by intestinal worms, the more exact observation of modern times only admits epileptiform convulsions being thus producible, although in a child strongly predisposed, hereditarily, true epilepsy may ensue. It is certain that epileptics are not liable to worms more than other persons.

5. *Indications.*—Although M. Moreau again dwells upon the importance of duly appreciating the potency of hereditary predisposition in our treatment of this disease, we cannot discover that he puts forward any novel suggestions derived from this consideration, as guides in preventive or curative treatment.

Adverting to the *empirical treatment* of the disease, he observes, that every agent capable of exerting a modifying influence on the nervous system has been again and again tried, further experience failing to confirm the existence of virtues at first announced. The only substance the claims of which he examines, is the *oxide of zinc,* which has recently been so prominently brought forward by M. Herpin. By a believer in the almost incurability of true epilepsy like our author, the statements of Herpin (that he cured 26 out of 48 patients, and ameliorated other 10, only 12 resisting all treatment) would naturally be received with utter incredulity. He nevertheless entered upon a series of trials with this agent, the results of which in no wise corroborated these statements; and he attributes their having been made to inexactitude and hasty observation—the diagnosis in some of the cases being founded on hearsay evidence, while in others, the patients had not been long enough watched after their reputed cure.

With respect to the claim set up for epileptic remedies, that they produce some relief and diminution in the violence of the symptoms, M. Moreau observes:

> "Whatever may be thought by many estimable practitioners, we must confess that we altogether reject this statement, for the following reasons:—Every one knows that there are few remedies (or I might say there is no remedy) which, when first employed in the treatment of epilepsy, does not give rise to some apparent advantage. Is this attributable to the action of the substance itself, or merely to the imagination of the patient? We are of opinion that it depends a little on both; for we have seen these substances act in much the same way upon individuals susceptible to the influences of imagination, and those that were not so. It would seem that, in this great perturbation of the neurosity, every agent capable of modifying in any manner the disordered condition of the nervous system, may produce temporary relief. However this may be, it is a fact for me

beyond all dispute, that the improvement thus obtained, the few days of respite gained, are almost always, a little sooner or later, dearly paid for by the prevalence of a worse condition than before. The more sensible the amelioration seems, the more reason have we to fear the violence of the disease when it reappears." (tom. xviii. p. 156.)

Thus far no anti-epileptic has been discovered; but M. Moreau thinks, somewhat inconsistently with the above statement, that, in so exceptional a state, we should try and re-try every substance that may possibly exert a modifying influence on the nervous system. The so-called rational treatment, founded upon the endeavour to operate indirectly upon the nervous system, by remedying faulty general or partial derangements of the entire organism, has met with little more success, if any. Attempts have been made to cut short the disease by attacking a particular symptom, sometimes present—the *aura epileptica.* Various applications have been directed to near the spot where this manifests itself; but the benefit reported by Portal and other old writers to have accrued from these, probably arose from mere epileptiform diseases having been dealt with. It is possible, indeed, that temporary benefit might result, even in true epilepsy, from the effect produced upon the imagination.

Notwithstanding the highly unfavourable prognosis M. Moreau delivers in regard to true epilepsy, he yet admits that a cure does occasionally take place—this result, however, seeming rather due to the operations of nature than to any form of medication employed. Hippocrates pointed out the importance of regimen; and all subsequent authors have echoed or amplified his recommendations. M. Moreau also declares, that if any practical indications are deducible from his study of the causes of epilepsy, these are embodied in the precepts of Hippocrates. In a constitution so deeply tainted by hereditary influence, all has to be remodelled and changed. The indication is to amend, by every means art places at our disposal, the morbid disposition amidst which epileptics are placed; and for the production of modifications and transformations like these we can alone look to *hygiène.* Without dwelling upon the well-known laws of this branch of preventive medicine, M. Moreau adverts to one or two points of importance:

1. An almost indispensable condition is a change of climate. No importance, in this respect, can be attached to any particular climate. The essential thing is to change the one to which the patient has been hitherto accustomed. By this very change, the whole physical and moral habitudes undergo, though slow, yet certain modification, providing the sojourn be sufficiently prolonged. 2. Remove from the patient all that may over-excite his intellectual functions, and develope his sensibility, passions, and affections, while you at the same time engage him in manual labour, or other exercises favourable to the development of muscular energy. Exercise in the open air, and the employments of a country life, where alone the requisite calmness and tranquillity can be found, seem to fulfil these conditions in the best possible manner. Gymnastics, too, have been found at Bicêtre and Salpêtrière to exert a most beneficial effect upon the health of the insane and the epileptic. They should, however, only be employed under medical superintendence. 3. Food. Without doubting the importance of duly attending to the diet

in epilepsy, M. Moreau does not advocate the adoption of too minute regulations. Sobriety and temperance are essential; and all drinks capable of exciting the nervous system—as tea, coffee, and, above all, alcoholic fluids—are to be avoided. The diet should be ample and reparative, but not in excess.

V. *On the Physiological, Obstetrical, and Jurisprudential Relations of the Ergot of Rye.* By M. Millet.

As this essay does not contain much that is novel to the English reader, a brief account of its contents will suffice. Administering large doses (1 to 5 grammes) of this substance to himself and others, M. Millet found dilatation of the pupils to be the most constant and persistent of the phenomena that resulted, being accompanied, when the dose was very large, with much disturbance of vision. This dilatation is, indeed, frequently observable under the ordinary administration of the drug. Cephalalgia is another consequence of large doses, but vertigo is a less constant symptom. It is usually accompanied by singing in the ears, which proves a very pertinacious and distressing one. An irresistible desire for sleep was frequently observed, especially after intense vertigo; and writers note this as one of the most common symptoms in ergotism. M. Millet considers the gastric symptoms that ensue, not as resulting from any direct action of the ergot upon the mucous membrane, but as dependent upon the influence of the cerebro-spinal system, and bearing some analogy to the effects produced by alcoholic drinks and solanaceous plants.

His experiments performed upon poultry led to results analogous to those recorded by Parola, Tessier, and other observers—the general conclusion being that ergot must be ranked among the narcotic poisons. One of the most constant effects was the production of a violaceous colour of the crest and crop, together with an almost gangrenous appearance of the integuments of the abdomen. On trying separately the various principles of the substance, M. Millet found, with Parola, that the *resin* is the poisonous agent—the oil only acting as such when not freed from resin. The lesions found after death are not always well marked, or always the same. Much engorgement of the vessels of the brain is usually observed, and the same condition is found to pervade the osseous system, especially at the articular extremities of the bones. Lesions are often found in the lungs, and especially hepatization at their posterior portion. The cavities of the heart contain black, semi-fluid, sticky blood, and sometimes large polypiform concretions. The venous system is always found gorged with black viscous blood. The alimentary canal sometimes offers slight signs of inflammation. The liver, besides being engorged in many instances, exhibits greenish spots, which penetrate into its tissue.

The obstetrical employment of ergot is most strongly advocated by the author, and with less discrimination than with us is usually thought necessary. Thus, of 32 cases of inertia of the uterus in which he used it, we find that 15 occurred in primiparæ. Indeed, labour impeded by rigidity of the soft parts, is with him an indication for employing it; and he always resorts to it before applying the forceps, as it aids their application by bringing the head lower down. He has also found it useful in several cases of puerperal convulsions, believing, with Levrat and others, that it acts as a true revulsive. In commenced or imminent abortion, the ergot procures a safe and prompt termination. Other observers, however, state that instead of inducing, it prevents abortion; and MM. Taddei and Parola maintain that, when given at an early period, it has a tendency to prolong gestation beyond its normal term. In retained placenta, due to inertia, and in *post-partum* hæmorrhage, the author, like most practitioners, has met with numerous examples of its efficacy, the latter rarely occurring, in his experience, when ergot has been administered during labour. Ergot is very useful in calming the uterine contractions upon which after-pains depend, which speedily yield to small doses of the watery extract. Without denying the possibility of its occurrence, M. Millet has never, in his large experience with this drug, met with an example of injury done to the child; and he believes that the accounts of the English writers must be exaggerated.

In his section on Ergot in Relation to Public Hygiène, the author details the effects produced by the mixture of spurred rye with wholesome flour; and refers to the epidemics of convulsive and gangrenous ergotism that have resulted. As he furnishes no original observations upon this part of his subject, we must refer the reader to his paper for the historical information he has collected.

VI. *Report on the Medical Service at the Thermal Establishments of France during* 1851-2. By M. Patissier.

M. Patissier observes that, both as regards their number and the variety of their chemical constitution, France possesses a richer supply of mineral waters than any other country. The number, 864, furnished by the *Administration des Mines*, is probably below the truth. At 140 of these there are one or more physician-inspectors, appointed by Government, who are required to send in annual reports, which are then transmitted to the *Académie de Médecine* to examine. The authors of the best reports receive medals. The system seems better on paper than in actual practice, for in 1851 and 1852 only 113 of these reports were received. These are examined in detail by M. Patissier. The 200 pages thus occupied may prove interesting to those who wish to ascertain the present condition of the mineral waters in France; but we must content ourselves with adverting to some of the general observations with which the reporter concludes his account of them.

1. *Mineral Waters considered as Prophylactic Agents.*—M. Patissier believes that the hygienic value of these, as compared with their medicinal, has been underrated by the profession. They offer a most valuable resource for the town valetudinarian, who, neither ill nor well, only gets worse under pharmaceutical treatment, whether of a tonic or depressing kind. Under their employment, and that of the attendant hygienic circumstances, old pains disappear, nervous irritability is allayed, nutrition is rendered more active, and, with the increase of strength, courage and hope return. In the melancholic and hypochondriacal the benefit derived

is sometimes remarkable; as is the case also in young persons whose constitution has become enfeebled and their nervous system over-stimulated, by excess of study. In "*lymphatism*"—a diathesis which, though sometimes hereditary, is oftener the product of vitiated air, sedentary habits, and bad food—the advantage of the sea-side, sulphureous, ferruginous, and saline waters, is undoubted. This condition of the organism, without being actual disease, is not health, and is betrayed by a swollen, pale countenance, false *embonpoint*, a feeling of general lassitude, slow pulse, and general indolence of functions. By rendering the imperfect nutrition and languishing vitality more active, and imparting more tone to the tissues—the employment of mineral waters may prevent the establishment of scrofula and phthisis.

2. *Mineral Waters as Curative Agents.*—Taken at their source, mineral waters constitute the best agents for the treatment of chronic disease, when prescribed with due attention to the stage of its progress and the temperament of the patient. Complex in their operation, they are at once stimulant in various degrees and alterative. Their stimulant power is often pre-eminently useful, rendering the chronic disease for an instant acute, and, by rousing the organic movements from their inertia, facilitating the disgorgement of the vessels that are the seat of passive congestion—mineral waters acting in such cases very much as irritating collyria do in chronic ophthalmia. This stimulant action is also exhibited in the increase of the secretions, and in the appearance of various forms of eruption at the surface. The alterative action, though less appreciable to the senses, is no less real, the humours evidently undergoing modification by means of the sulphur, iodine, arsenic, iron, and alkalis contained in the waters. Owing to these active principles existing in such weak proportions, this modifying influence has been denied to them; but alterative medicines seem to be more effectually absorbed in the digestive passages, in proportion as they are given in divided doses, dissolved in liquid. Thus, a much greater effect is produced in chlorosis and anæmia by ferruginous mineral waters, than by pharmaceutical preparations of steel in large doses.

M. Patissier protests against the too indiscriminate employment of these agents, without due consideration of the special adaptation of certain classes of them to different pathological conditions. As a result of his examination of the reports, he finds persons suffering from the *diathesic* affections (as rheumatism, gout, *dartres*, scrofula, and syphilis) resort, as it were, instinctively to mineral waters. The morbid conditions known as *diatheses*, are evidently the product of a special change in the fluids; and upon them the active principles of mineral waters exert great modifying influence. These principles are so well combined with adjuvants, and so completely dissolved, that they readily penetrate into the circulation, impressing upon the entire organism a far more salutary influence than results from the use of most pharmaceutical substances. The amendment takes place slowly, without appreciable crises, and may not become complete until some time after the employment of the waters has ceased.

M. Patissier believes that the doctrine of *metastasis* has undeservedly fallen into discredit, and he finds the reports full of instances of the production of functional disturbances by the metastasis of gouty, rheumatic, or syphilitic principles. These affections often, indeed, assume the mask of neuroses, visceral neuralgia, &c., and the diagnosis may be difficult to establish—the maxim, *naturam morborum ostendit curatio*, often finding its application. Many pathological conditions, to all appearance very serious, and which have resisted the most rational treatment, are dissipated by provoking to the surface the rheumatic, gouty, or syphilitic principles, or reproducing a dartrous eruption inconsiderately repressed.

Rheumatic affections constitute more than a third of the affections met with at the baths. Endocarditis frequently co-exists, and warm baths are found, what *à priori* would not be expected, to almost constantly mitigate the dyspnœa and tumultuous action of the heart. In diseases of the skin, much good or harm may result, accordingly as the mineral waters are selected with due regard to the cause of the affection and amount of irritation, and the perseverance with which they are employed. In scrofula, their use is rarely continued long enough to exert a curative influence on lesions springing from so deep-seated an infection of the economy. It is by no means rare to find syphilis, that had long remained latent, becoming unmasked and amenable to treatment under the influence of mineral waters. They are not in themselves anti-syphilitic; but they are useful adjuvants to specific treatment. It is remarkable that during their employment salivation is rarely produced, even when large doses of mercury are given.

Chronic diseases, as met with at the thermal establishments, are far from presenting themselves in the same simple manner as they are described in the books. Few individuals offer but a single lesion, several usually co-existing or engendering each other, so that the curative indications are sometimes contradictory, and require tact for their seizure.

M. Patissier thinks more attention should be paid to regimen, while taking mineral waters, than is usual; and he believes that much of their efficacy is impaired by luxurious living at *tables d'hôte.* Many persons who thus indulge for several weeks in too copious and too succulent a diet, suffer much after their return home, from gastro-intestinal irritation.

VII. *A Case of Black Coloration of the Face.* By M. Bousquet.

This is a curious case, occurring in the person of a married woman, æt. 21, heretofore in good health. After a violent, resonant, paroxysmal cough had tormented her for several days, but had then yielded, she found her face gradually assuming a darker and darker colour, until it looked as if smeared with a thick solution of indigo. The colour afterwards became still deeper, until her face resembled that of a negro under slight transpiration. By washing, the black layer could be removed, the skin then presenting a bluish-black colour, as the chin of a person having a strong black beard does after shaving. After a while, droplets of black fluid appeared at the surface, and, forming into a compact layer, gave the same appearance as before. Linen coming in contact with the face acquired stains that repeated washing only effaced. Commencing towards the end of August, the coloration had quite disappeared by the middle of December;

and, with the exception of a re-appearance for a few days about a month after, she has since continued quite well. The only symptom noticeable during its persistence was severe cephalalgia.

Besides the papers we have noticed, these volumes contain *éloges* on Hallé, Boyer, and Orfila, from the pen of M. Dubois. There are also reports on the epidemics that occurred in France during 1850, by Professor Levy, and of those which occurred in 1851-2, by M. Gaultier de Clanboy; an interesting one upon the Miliary Sweat, by Jules Guerin; and an account, by M. Bouvier, of the Comparative Mortality produced by the Cholera of 1849 in the different districts of Paris.

John Chatto.

REVIEW IV.

Klinik der Unterleibs-Krankheiten. Von EDUARD HENOCH, Doctor der Medicin und Chirurgie, und Privat-Docenten an der Friedrich-Wilhelms Universität, zu Berlin. Zweiter Band. — *Berlin,* 1854. pp. 336.

A Clinical Treatise on the Diseases of the Abdomen. By EDWARD HENOCH, Doctor of Medicine and Surgery, &c. Second Volume.—*Berlin,* 1854. pp. 336.

WE proceed to present our readers with a brief account of Dr. Henoch's second volume, which fully maintains the position attained by the first, which we reviewed more at length in a former number. It need scarcely be repeated, that the plan he has adopted has been to select some one prominent symptom as the basis of his remarks, and to consider together the several diseases in which it is the most marked phenomenon. The first symptom which he discussed was tumour; and he completes in this volume the diseases which give rise to it, by an account of the diseases of the spleen.

Enlargement of the Spleen—which in early childhood almost always causes projection downwards into the left hypochondrium, but in adults is often concealed behind the thoracic parietes, and is scarcely determinable but by percussion—is liable to be mistaken for a variety of other diseases. Dr. Henoch mentions pleuritic effusions here, but apart from the history of the case, the diagnosis is supported by the normal condition of the intercostal spaces, and the continuance of the respiratory movements of the affected side. Again, an enlarged spleen may press the lower false ribs forwards and outwards, in a manner which only occurs exceptionally in pleuritic exudation. He also enumerates various causes of abdominal tumour, such as enlargement of the left kidney, peritoneal abscess, fæcal accumulation, and retro-peritoneal tumour conjoined with abscess in the subcutaneous cellular tissue.

Displacement of the Spleen may occasion palpable tumour—a condition which, however, we have seen simulated by a kidney pushed forwards and upwards by the habit of inordinately tight lacing.

Mechanical Hyperæmia.—The remarkable structure of the spleen, its elastic covering and trabeculæ, the peculiar arrangement of its blood-vessels, and the provision made for variation in the bulk of the organ as different circumstances—whether local fulness of the portal system, or general plethora—occasion its physiological repletion with blood, all favour the occurrence of enlargement of the organ under a variety of conditions which impede the return of blood from the portal system. Thus it is that passive hyperæmia of the spleen occurs in cirrhosis of the liver, for example, or in various diseases which compress the portal or splenic vein. Where the cause is one which is prolonged or permanent in its duration, more or less exudation of plasma is conjoined, and the consistence of the organ, as well as its redness, is increased, and those changes in the covering of the organ take place—e.g., cartilaginous thickenings, all of which are commonly regarded, with questionable accuracy, as the result of chronic inflammation. The great distension of the vessels, too, may occasion hæmorrhage into the substance of the organ—apoplexy of the spleen—a condition also of much interest, when we keep in mind the observations of Mr. Gray respecting the presence of lacunar spaces within the spleen.

Acute Splenitis, marked by acute pain in the lower left lateral region of the thorax, increased by deep breathing and pressure, fever, short accelerated breathing, dry cough, &c., is liable to be mistaken for acute pleurisy. Two kinds of inquiry are applicable to the diagnosis: on the one hand, assistance is derived from the absence of detrusion of the intercostal spaces, and continuance of respiratory murmur between the position of the spleen and the spine; and, on the other hand, from a recognition of those etiological relations under which splenitis is admitted to arise. It is thus that assistance is derived from the origin of the disease in connexion with intermittent and with various dyscrasic conditions of the blood, such as are present in typhus, cholera-typhus, pyæmia, and in anomalous exanthemic processes, as in dissolution of the blood after erysipelas, scarlatina, miliaria, and rheumatism, in drunkards, and acute tuberculosis. The softness of the tumour in these cases, as well as the tension of the abdominal muscles, may prevent the enlargement being palpable, and, in such a case, it is only discernible on percussion. The spleen may even become ruptured, with a fatal result. As also, in the instance of hepatitis, acute inflammation of the spleen may arise from traumatic causes. The pain in the left shoulder, and other consensual symptoms, may give rise to the splenic disease being overlooked, and to the belief that a case is one of rheumatism. This occurred a few weeks ago, in the instance of a patient about whom we were consulted; and the similarity was increased by the redness and pain in some of the joints which accompanied and preceded the splenitis, but all of which arose in connexion with disorganization of the blood about three weeks after labour. The signs on percussion and palpation clearly determined the splenitis, and the patient died with peritonitis, purulent effusions behind the peritoneum, and with extensive hæmorrhage into the tissue of the bladder and into the cellular tissue anterior to that organ.

As in the instance of hyperæmia of the liver, Dr. Henoch lays much stress upon the frequent connexion of suppression of the catamenia with splenic congestion.

Dr. Henoch recognises two forms of chronic enlargement of the spleen. The most common form is a true *hypertrophy*, a mere increase of the natural elements of the organ. In a large number of cases, however, he considers the enlargement is due to excessive interstitial exudation of plasma, probably of a character unfit for healthy nutrition, and he consequently regards this kind of tumour as the result of chronic splenitis. The course of these chronic splenic tumours is often obscure, and even latent; but, for the most part, they are associated with symptoms. The colour of the skin is usually affected: it is commonly pale and waxy, accompanied by pallor of the mucous membranes of the mouth and eyelids. An additional interest has been attached of late to this symptom, on account of the connexion pointed out by Virchow and Dr. Hughes Bennett between splenic disease and the increase in the white particles of the blood. The coincidence, however, is now known not to be constant, and, independently of the leucæmia, the diminution of the red corpuscles and relative increase in the watery constituents of the blood suffice to account for the anæmia. In other instances, but less frequently, the skin acquires a dull greenish, or greyish-brown tint; and the older physicians mention a blackish yellow tint; in all which cases we have to do with an abnormal formation of pigment, of the presence of which satisfactory proof has been afforded by Meckel, Virchow, Heschl, &c. The occurrence of hæmorrhage is next mentioned as a symptom of splenic disease. These hæmorrhages may be in the form of purpurous spots into the subcutaneous cellular tissue, or may take place from the surface of the mucous membranes. The occurrence of hæmatemesis has been particularly dwelt upon, and Dr. Henoch considers that we should not immediately assume its connexion with a dyscrasic condition of the blood, except in cases where other hæmorrhages are conjoined. It may be due, in fact, to a mechanical cause, namely, the same obstructing cause that produces the splenic enlargement, such as cirrhosis of the liver, or closure of the trunk of the portal vein. When these conditions are absent, it is difficult to explain the hæmorrhage. Nonat relates a case, in which hæmatemesis occurred after a prolonged intermittent, and the blood evacuated, which resembled the lees of wine, contained organic particles like the cells of the spleen. On the subsequent death of the patient, from another disease, a cicatrix of a perforating ulcer of the stomach was discovered adherent to the spleen, and through this ulcer, evidently, the over-filled spleen had emptied itself into the stomach.

In some cases the hæmatemesis appears critical, being accompanied by partial or entire collapse of the spleen, and in cases where the splenic enlargement is connected with menstrual suppressions, the hæmorrhage may be repeated till the catamenia are restored. It is probable that accurate observation in such cases would show as the cause of the hæmorrhage either structural disease of the liver or of the stomach. Where the spleen undergoes no diminution from the hæmorrhage, the prognosis is unfavourable, and the hæmatemesis may be but the prelude of death. Hæmorrhages from the gums, lungs, kidneys, and uterus are proportionally rare, are accompanied by other symptoms of scurvy, and thus, together with the splenic disease, are to be attributed to one and the same dyscrasic condition of the blood.

Dropsical symptoms, lastly, frequently appear sooner or later in the form of œdema of the face and extremities, ascites, œdema of the lungs, &c. The ascites may arise mechanically from pressure upon the great veins of the portal system, but where no mechanical cause can be assigned, the dropsical effusions may be referred either to simultaneous disease of the liver, chronic peritonitis, stopping up of the iliac veins by coagula of blood, morbus Brightii, or heart disease; or else to such an altered condition of the blood as accompanies chlorosis and prolonged intermittents. The etiological relations of splenic tumours sometimes quite fail in assisting our diagnosis; in many instances, however, an endemic origin of the disease is clearly traceable. It may thus arise in the sequel of intermittent; but it not rarely happens that ague has not preceded the development of the tumour, and, under such circumstances, we must assume that the alteration produced upon the blood operates directly upon this organ, together with others associated in preserving the proper composition of that fluid, as the liver and lymphatic glands.

We pass over the sections on splenic abscess, &c., to draw the attention of the reader to the second great division of abdominal diseases proposed by Dr. Henoch, namely, that in which *pain* is the leading and prominent symptom. He commences with the pain which occurs in paroxysms in the region of the stomach; and which is known under the term *cardialgia*. He draws attention to the fact, that the pain which occurs in ulcer and cancer of the stomach not uncommonly presents all the characters of a neuralgic gastrodynia, and, consequently, adopts the clinical view of regarding and treating of these affections as the most frequent and most important causes of this variety of pain. We shall not enter at any great length into the account which he gives of perforating ulcer and cancer of the stomach. In *perforating ulcer* there may be no other symptom present than intense paroxysms of cardialgia, causing the patient to double himself up, to throw himself upon his belly, to press the epigastrium for relief against the edge of a bedstead, a table, &c.; and even spinal pain and tenderness may be present to mislead. Inquiry should, under all such circumstances, be made for the most important symptom in the diagnosis, namely, hæmorrhage from the stomach, whether blood has been vomited, or passed as pitchy stools from the bowels. Where hæmatemesis or melæna have not occurred, the diagnosis of ulcer is very obscure, and must be drawn from the general tendency of a variety of considerations, such as the constancy of pain in the intervals of the paroxysms, and on deep pressure, and from its etiological relations, especially the previous and prolonged operation of the various causes of chronic inflammation. The greater liability of the female sex, established by Crisp and Jaksch, to simple chronic ulcer, and especially of chlorotic girls, aids nothing in the diagnosis, since it is under similar circumstances that pure neuralgic cardialgia is most frequent. In pure cardialgia, Dr. Henoch holds that a cure, either by the efforts of nature or of art, is the customary termination, and refuses his allegiance to the opinion of Andral, that it may terminate in actual structural change, partly because it is contrary to analogy with similar affections in other tissues, and partly because there is no evidence that in the cases where such an occurrence might be supposed to have taken place, the pain was not really a symptom of the presence of ulcer from the first. He considers the various terminations of simple chronic ulcer by adhesion, perforation, communication by erosion with large vessels, with the substance of the spleen, and with the peritoneal sac. His description of the last-mentioned accident is interesting. He draws attention to the different character of the pain experienced in such cases from that which after several attacks of cardialgia had preceded it, patients often describing it as a sense of internal tearing. The immediate cause of the rupture of the peritoneum may be an attack of vomiting, or a full meal, especially when to the latter is added the influence of any external pressure or straining effort. The sudden pain is followed by symptoms of intense peritonitis and a feeling of certainly impending death. He refers to the value of that sign of effusion of gas into the peritoneum which consists in the clear tympanitic sound obtainable on percussion over the region of the liver, and uniformly over the whole surface of the abdomen. We are disposed, however, to regard this sign of perforation, on the whole, as less valuable in respect of the stomach than in respect of the intestines, since the quantity of gas contained in the former at the time of perforation is often very trifling; and, consequently, this sign is more likely to be absent than where the intestines are perforated. Sometimes death occurs, from the severe impression made upon the nervous system, too rapidly for peritonitis to be set up. In other instances the peritonitis may be limited in extent, and terminate in the production of an abscess circumscribed by adhesions. A case is related by Lombard in which this took place in a latent manner, and in which the customary symptoms of perforation only appeared when a laceration of the sac of the abscess caused the discharge of its contents and those of the stomach into the general cavity of the peritoneum.

After a few observations on *Hypertrophy and Induration of the Stomach*, Dr. Henoch passes on to the subject of *Cancer of the Stomach*, entering at length into the symptoms of the disease, as varied by its seat, extent, and concomitants, dilatation of the stomach, perforation, &c., and then discusses the treatment of Chronic Gastritis, Chronic Ulcer and Cancer. We shall not follow him in these details, but pass on to his remarks on the circumstances on which the diagnosis of pure nervous cardialgia is to be founded. He mentions the following:—1. Freedom of the intervals between the paroxysms of pain, absence of dyspepsia, softness and painlessness of the epigastrium, normal nutrition, and the absence of all signs of material disease of the stomach. 2. Alternation with other neuralgiæ, &c. 3. Other anomalous affections of the vagus nerve during the intervals of the paroxysms, especially ravenous appetite, which may even impel the patient to rise in the night to eat. 4. The occurrence of the symptom in the female sex, in which it is often conjoined with symptoms of hysteria or chlorosis, or is associated with various affections of the uterine system. 5. It may arise through a primary irritation of the nervous centres, such as in tabes dorsalis, or powerful mental emotions. 6. Dyscrasic conditions are believed to give origin to the affection, especially arthritis, hæmorrhois, the debility arising from spermatic losses and rheumatism. With respect to the last, Dr. Henoch calls attention to the error which is sometimes made, in mistaking a rheumatic affection of the abdominal aponeurosis for gastralgia. 7. It may appear as a masked ague. The diagnosis between pure and sympathetic neuralgia of the stomach is only to be made by the discovery, on careful inquiry, of some of those conditions out of which the latter may arise.

The subject of cardialgia is followed by the consideration of similar paroxysms of pain whose seat rather indicates the liver and spleen than the stomach: *splenalgia* and *hepatalgia*. *Gall-stones* are the most frequent and important cause of the latter, and are discussed at great length by our author. We shall therefore conclude the review by an analysis of this portion of the volume. After some remarks upon the physical and chemical characters of gall-stones and gall-grit, Dr. Henoch proceeds to the symptoms of the affection. They vary with the part of the biliary passages in which the concretions have formed. *a.* Concretions occur in the small biliary passages within the liver, mostly of a very small size, forming gall-grit or gall-sand, and rarely as large calculi. Sometimes the concretions accumulate here to such an extent (as in a case of Chopart's), as to give rise to a considerable obstacle to making a section of the liver with the knife. Washed down with the stream of bile, they give rise at the most to temporary pains; but should they accumulate, they induce, by virtue of their quantity, or in consequence of a stoppage in the canal of the hepatic or choledic duct, acute pains, jaundice, or even inflammation of the liver. A diagnosis is only possible by discovering the concretions in the stools on washing them carefully on a fine sieve. *b.* Concretions occur even more rarely in the hepatic duct, and are mostly met with where the choledic duct is impermeable. The retention of the bile thus produced not only favours the aggregation of the calculous particles, but may also induce secondary atrophy of the liver. A rare case by M. Voisin is quoted, in which a stone formed within the liver was arrested in its progress through the hepatic duct. *c.* It is in the gall-bladder that concretions are most frequently found, and attain the largest size. If a single calculus is present there, it sometimes fills the bladder, but commonly the calculi are numerous, and often remain in this situation for a long time, without giving rise to any inconvenience. However, they are not always thus harmless. In a case recorded by Abercrombie, for example, severe vomiting had persisted for more than a year, and when the patient died, completely emaciated, no other morbid condition was discovered than a large number of calculi filling the gall-bladder. The pressure of a number of calculi, as in this instance, may also give rise to a sense of weight and tension, and occasionally in these subjects palpation has discovered a sense of crepitation from the mutual friction of the stones. But more than this, the irritation and pressure of the calculi may give rise to chronic catarrh of the mucous membrane of the gall-bladder, or even to partial inflammation, with plastic exudation, suppuration, or ulceration, generally latent at first, but ultimately inducing important morbid symptoms. Thus the irritation of the calculus may give rise to an ulcer, which may perforate the gall-bladder, and occasion, by effusion of bile into the peritoneum, fatal peritonitis. A protective occlusion, however, may take place between the peritoneum at the base of the ulcer and neighbouring parts, and a communication be thus formed with some other cavity, by the formation of a biliary fistula, through which the calculus may be discharged. This communication most frequently takes place with the duodenum or transverse colon; its progress is mostly latent, in which case the first symptom is the passage of a large calculus by stool. The painless occurrence of this process led some of the older physicians to conclude that the passage of a large gall-stone is less painful than that of a smaller concretion. After the passage of the calculus, the fistulous communication may gradually close and cicatrize; but even should it remain open, no great danger to life results—except that, as in a case related by Reynaud, excrementitious matter may pass from the bowel into the gall-bladder, under which circumstances acute inflammation might be set up. The fistulous opening, however, may take place upon the external surface of the body, and then an abscess appears, which bursts, and discharges pus, bile, and gall-stones in variable quantities; and the opening may remain till several calculi have been discharged; or the fistula may close and cicatrize, especially where there has been only a single calculus to evacuate, or after closing it may again break open. *d.* By far most frequently the gall-stones pass along the cystic and choledic ducts into the duodenum. In this passage they may be arrested in the cystic duct. The plaitings of the lining membrane here may arrest even small stones, which may in this situation increase in size until they produce more or less complete obliteration of the canal; under such circumstances the gall-bladder may become atrophied, or be distended with a colourless or pale greenish fluid, but in other cases acute inflammation may result. The passage of the stone along the cystic duct mostly gives rise to the symptoms of hepatic neuralgia—gall-stone colic—which it is unnecessary here to describe, and the intensity of which varies with the length and diameter of the duct, the number and size of the plaitings, the size and character of the stone, and the irritability of the patient. The first symptoms of this movement of the stone may take place suddenly, without any known cause, or after a fall, pressure on the right hypochondrium, or even as a result of a powerful mental impression. A contraction of the gall-bladder operates here, either induced in a reflex manner by the irritation of the calculus, or through other nerves. It frequently occurs a few hours after meals, at a time when the bile is being evacuated from the bladder into the intestine. The stone is not always discharged, and may pass back again into the gall-bladder, or may remain lodged in the cystic duct.

On the arrival of the gall-stone at the comparatively wide choledic duct, the severe symptoms usually abate; but when the calculus is large, severe pain may yet occur before it arrives at the duodenum. The prolonged stay of a large stone in this canal is always accompanied with more or less pronounced jaundice, the development of which enables us to refer with certainty any pain which may yet be present to the transit of the stone through this portion of the biliary passages, since obstruction of the cystic duct of itself cannot give rise to these symptoms. Should there be still room for bile to trickle down, however, between the calculus and the wall of the duct, and the peculiar shape of even a large calculus may permit of this, jaundice may be wanting. Where the bile is absolutely prevented from passing, its retention gives rise to biliary congestion, and simultaneous distention of the gall-bladder. In rare cases the impaction may occur, with acute symptoms, resembling those of perforation of the stomach, and proving fatal after a few days. Sudden severe epigastric pain, with violent vomiting, follow, and, after a time, icterus; the pulse sinks, the skin becomes cold, and the patients die, either suddenly or with gradually increasing collapse. Chronic obturation of the choledic duct by the stone is, however, more frequent, in which case a secondary deposition of gall-stones may occur, which by their irritation may give rise to hepatitis and circumscribed abscess of the liver.

Dr. Henoch next considers the various ways in which the calculi may make their exit from the body. 1. They may get from the duodenum into the stomach, and be evacuated by vomiting. 2. Most frequently they are passed with the stools, often with slight colicky pains, the next day or later, after the cessation of the neuralgia. When a calculus is large, great distress may accompany its evacuation through the rectum. In a case of Déramond's, related by Dufresne, repeated paroxysms of severe colicky pains occurred in various parts of the abdomen, with rigors, severe vomiting, obstinate constipation, and alternating elevation and depression of the abdominal wall. Suddenly the pain changed its seat; it appeared to the patient as if the rectum along its whole length was bored through with a red-hot iron; bloody mucus flowed abundantly, but in spite of straining, no fæces were evacuated. At last, after the noisy expulsion of much fœtid gas, under a paroxysm of fainting, and with much straining, a very large stone passed through the torn and bleeding anus, and fell with noise to the bottom of the night-pan. A similar case is related by Dr. Leo of Carlsbad, in which several large calculi were evacuated in this manner, after producing bloody and mucous stools, tenesmus, and intolerable pain in the rectum. These effects of a single large gall-stone may be produced also by the concretion of several smaller calculi within the canal, or a gall-stone may become the nucleus of an intestinal concretion; and in any of these cases the symptoms of ileus may result. 3. As the easiest mode of evacuation, Dr. Henoch mentions the occurrence of a communication between the gall-bladder and the urinary passages, and the discharge of the calculus by the urethra.

The certainty of the diagnosis of gall-stones is increased by taking into consideration, as in other diseases, the conditions under which it arises. Their occurrence, chiefly after middle age and in the female sex, is first referred to, and next their connexion with obesity, with hereditary tendency, and with the cancerous cachexia. Dr. Henoch regards an atonic condition of the walls of the gall-bladder, which accompanies general atony in the above instances, as having principally to do with the formation of the calculi, by favouring the prolonged retention of bile. In a similar manner he explains their origin in persons of sedentary habits, females, artists, literati, prisoners, &c.; and he believes those cases in which it has been referred to a modified secretion of the liver, with an unusual formation of cholesterine from particular modes of living, to be explicable in a similar manner. Lastly, he draws attention to the long-recognised connexion of this disease with urinary lithiasis.

The volume concludes with an appendix, in which the author discusses the subjects of Dyspepsia and Vomiting.

Edward Ballard.

REVIEW V.

Statistical Reports on the Health of the Navy, for the Years 1837-43, *inclusive.* Part II. *East India Station.* Compiled by Dr. BRYSON, and presented to Parliament, 1853.—Folio, pp. 94.

Two volumes of Reports, by Dr. Wilson, 'On the Health of the Navy during the Seven Years 1830 to 1836,' were presented to Parliament in 1840 and -41. These were followed, in 1849, by a Report by Dr. Bryson, comprising the South American, the North American and West Indian, and the Mediterranean, stations, for the seven subsequent years; and the volume now before us gives the details of the East India Station for the same period. In it several improvements have been introduced, for which much credit is due to the author. The first of these is a change in the classification of the diseases; in the previous Reports, that of Cullen was followed, in consequence of the returns sent to the office having been made out in that form; in the present, a more natural arrangement has been adopted, closely resembling that of the army statistical reports. It is to be regretted that Dr. Bryson did not take the latter classification without any alteration; the points of difference involve little matter of real importance, but are sufficient to prevent a comparison of the relative prevalence of particular classes of diseases in the two services being made, without remodelling one or other of the returns. It is right, however, to add, that he has put it in our power to do so, by giving detailed abstracts of the different diseases. A number of tables which were formerly spread through the volume, have been condensed into one, and the numerous returns from each ship on the station, with which the Appendix was encumbered, have been discontinued; in fact, much mere elementary material has been got rid of, and the work has gained greatly by the change.

Before proceeding to analyse the facts contained in the Report, we are tempted to make the following extract from the introduction, as illustrating well the difficulty of applying statistics, to determine questions as to the influence of treatment in reducing the mortality by disease:

> "In a previous Report, the writer alluded to the medical treatment of disease, as forming an element in vital statistics which could not—or which, at all events, ought not—to be regarded as of minor importance in any inquiry into the causes which influence the rate of mortality—either from particular kinds of disease, or amongst particular classes of men; but this is more especially the case as regards diseases which are suddenly developed and are rapid in their course, and against which, it is supposed, remedial measures, to be effective, must be bold, prompt, and decisive. An attempt was therefore made, in drawing up the present Report, to form a set of tables, showing the results attributable to the different modes of practice in different ships; but in consequence of the difficulty of finding two or more vessels in which the men had, for a sufficient length of time, been placed under circumstances which were precisely similar with respect to the causes which influence health, together with all the necessary information as to their exposure on shore or in boats, their habits and modes of living, and also as regards the condition of the decks and the nature of the ventilation in the several vessels, the attempt was abandoned. But this much has been ascertained—that the exhibition of any of the more popular remedies on the heroic plan was not attended with the

most favourable results; for though, when so given, they might appear to succeed in one case, there were others in which they failed." (p. v.)

The seven years included in the Report now under consideration may be divided into three of peace and four of war; for although, during the last year, active hostilities had ceased, the greater part of the force remained at Hong Kong, where "they contracted diseases nearly equal in virulence to those from which they suffered in the Canton and Yang-tse-Kiang rivers." During the first period, the vessels were chiefly employed in the Bay of Bengal, the Arabian Sea, and the Indian Ocean, north and south of the equator, from about the 70th to the 160th degree of east longitude, their duties being the protection of the mercantile shipping and the suppression of piracy, more especially off the coasts of Borneo, Malacca, Sumatra, Java, and Amboyna. In April, 1840, the fleet assembled at Singapore, and sailed for the outer waters of the Canton river, and from this period till towards the close of 1842 it was employed in active operations along the shores or up the rivers of the Chinese empire. In 1843, the greater part of the force remained stationary at Hong Kong. In 1840, -41, and -42, the men were much exposed in the boats, and were frequently landed to assist the troops in the attacks upon the various forts, and in both duties were continually within the influence of the pestilential emanations from the lagoons and swamps with which the shores abound.

Dr. Bryson has given a brief summary of the various operations in which the fleet was thus engaged, and the circumstances which appeared to exercise an influence upon their health during this period. The results, as evidenced by the amount of sickness, mortality, and invaliding, are shown in the following table:

Aggregate strength:	Ratio per 1000 of mean strength.		
27,570.	Treated.	Died.	Invalided.
1837	1634	21·4	37·1
1838	1317	22·7	20·
1839	1496	21·	22·
1840	1763	33·3	18·
1841	1821	59·9	32·8
1842	2243	51·5	23·3
1843	2125	51·8	37·2
Total	1923	44·7	27·9

If the period be divided as above suggested, we find the ratio treated during the first three years to be 1460, and during the last four, 2042 per thousand, annually, while the deaths amounted to 21·8 and 50·6 per 1000; the sickness consequently was increased in the proportion of 3 to 2, and the mortality nearly in that of 5 to 2. With a view to ascertain the diseases to which this increase was chiefly attributable, we have compiled the following table, showing the ratio per 1000 of mean strength treated for each class of diseases, and the deaths by each during the two periods:

——	Annual ratio per 1000 of mean strength.						
Period	3 years: 1837—39.		4 years: 1840—43.		7 years; 1837—43.		
Aggregate strength	5650		21,920		27,570		
	Treated	Died.	Treated.	Died.	Treated.	Died.	Invalided.
Fevers	147	2·1	501	7·9	429	6·7	2·3
Eruptive fevers	...	...	2	·3	1	·3	...
Diseases of brain, nerves, &c.	10	1·4	8	1·7	8	1·6	1·5
„ respiratory organs	217	2·5	241	2·2	236	3·1	2·5
„ heart and bloodvessels	12	·7	10	·1	11	·3	·7
„ stomach and bowels	272	5·3	538	23·6	484	19·8	8·6
„ liver, &c.	18	·7	11	·7	13	·7	1·1
„ kidneys, bladder, and genitals	71	·5	54	·3	57	·3	1·3
„ joints, muscles, bones, &c.	58	·7	73	·3	69	·4	1·7
„ senses, inflammatory affections of the eyes, ears, &c.	24	...	12	...	14	...	·4
„ skin, glandular system, and cellular tissue	375	·4	337	1·0	345	·8	3·3
Wounds, accidents, injuries, &c.	202	2·5	198	2·4	199	2·4	3·0
Diseases, &c., not classed above	54	...	57	·7	57	·6	1·5
Drowned	...	4·5	...	3·6	...	3·8	...
Cause of death unknown	...	·5	...	4·8	...	3·9	...
Total	1460	21·8	2042	50·6	1923	44·7	27·9

An inspection of this table will show that the great increase, both in sickness and mortality, took place in fevers, and diseases of the stomach and bowels. The proportion of deaths, also, from causes not known, was much higher, arising, in most instances, from having occurred among men detached from their ships, and, in a few, from the returns being incomplete, in consequence of the death of the surgeon. The Report strikingly illustrates how much more formidable to an army or a fleet disease is than the sword. During the three years of active hostile operations, only 29 men fell by the hand of the enemy, being in the proportion of $1\frac{9}{10}$ per 1000 of the strength; while during the same period, 748, or $48\frac{3}{10}$ per 1000, perished from other causes, chiefly diseases produced by climate:—"Thus showing," as Dr. Bryson remarks, "what few will venture to question, that generally, in continuous wars, more men fall victims to disease than are killed by wounds inflicted in the strife of battle."

Fevers were more than trebled both in amount and mortality by the Chinese war, and were contracted chiefly in the Canton and Yang-tse-Kiang rivers, and at Hong Kong. In 1841, on the Canton river—

> "Large detachments of men were frequently landed, remaining sometimes for several nights in succession exposed to the noxious exhalations from paddy fields, from marshes, and from the slimy deposit on the banks of the river, sleeping occasionally on the bare ground, under trees, or at best under some temporary shelter hurriedly put up for the purpose, and indulging in the intemperate use of spirits of the worst description, which they stealthily purchased, or procured by other means, from the natives. These were indeed causes amply sufficient to account for the fearful loss of health and life which followed." (p. 38.)

In commenting on the returns for 1843, Dr. Bryson remarks:

> "Like the preceding year, the details for the present furnish the most ample proof, were it required, of the unhealthy nature of the Chinese coasts, from the equator up to the thirty-third degree of north latitude; although beyond that, even up to the most northern limits of the Yellow Sea, there is no reason to suppose that it is more healthy. Everywhere a humid atmosphere overhangs a marshy soil, teeming with a profuse and vigorous vegetation, stimulated to exuberant growth by strong and offensive manures—conditions which, within the tropics, generally, if not universally, prove destructive to the health of men coming from regions north of the fiftieth parallel of latitude." (p. 68.)

The limited extent over which the cause of these paroxysmal fevers operates is illustrated in the case of the *Agincourt*, at Hong Kong. The most severe cases of remittent were contracted by a party of Marines from this ship, who mounted guard over the

> "Stores on West Point, which were surrounded by swamps and deserted paddy fields, bearing a rich but rank crop of wild vegetables. . . . . Every man of the guard who slept on shore was attacked, whereas there did not occur at that period a single case of a similar nature amongst the rest of the crew." (p. 69.)

A similar fact is also noticed by the surgeon of the *Childers*, as regards the Canton river.

> "The malaria appears to have been very limited in its range; for we ascertained that the merchant vessels two or three miles further down did not suffer nearly so much from disease as those which lay higher up the river; consequently, some of the former (latter?) tripped their anchors, and dropped down the river, to take advantage of the atmosphere below, which was considered to be less charged with marsh miasm." (p. 76.)

The fevers have been divided into two great classes—Continued and Remittent, and Intermittent. The first were classed together, as Dr. Bryson informs us in his previous Report, "in consequence of the frequency with which these two forms of fever merge into each other, as well as from the irregular manner in which they have been originally arranged; there being no possibility of separating the one from the other with even an approach to accuracy." This is somewhat different from the arrangement usually followed, which is to combine the intermittent and remittent, keeping the continued type as a separate class. Thus Dr. Wilson, in his 'Medical Notes on China,' describes the remittent fever as constantly passing into the intermittent form; and Dr. Geddes, in his 'Clinical Illustrations of the Diseases of India,' says, that "Apparently the produce of the same cause, showing similar symptoms, and removed by the same means, there seems no difference between these two forms of disease but that of severity." Dr. Bryson himself observes that the fevers, "with the exception of a few doubtful cases, were of a periodic character, remitting in the first instance, and in the end intermitting." According to the Table in the Report, there were in the seven years 3994 cases and 123 deaths of continued and remittent fever, and 7831 cases, with 62 deaths, of intermittent; so that the latter, though twice as numerous, gave rise to only half as many deaths as the former types.

During the first three years, 1 in 69 of the cases of fever proved fatal; and during the last four, 1 in 64. In the seven years, 1830-36, the proportion was 1 in 59; so that although during the period now under review this class of diseases was much more prevalent and fatal, their intensity does not appear to have been increased.

There were two diseases with which the fevers in the Chinese seas were frequently complicated, and which tended greatly to embarrass the surgeon in his treatment; these were dysentery and scurvy.

> "The treatment of these cases must have been extremely difficult and perplexing; . . . . for the remedial measures which were suitable for the one disease were contra-indicated in the other. Lemon juice and a wholesome nourishing diet, consisting of fresh meat, vegetables, and fruit, were required to remove the scorbutic diathesis; astringents, absorbents, mercury, and opium, with a strictly farinaceous diet, to cure the bowel complaints; quinine and arsenical solution, to break the chain of febrile action. The remedies and diet required to cure the scurvy were contra-indicated in dysentery; while those required for dysentery were frequently inadmissible in ague. For the cure of scurvy, succulent vegetables and ripe sub-acid fruits are essentially necessary; but in dysentery or diarrhœa they are the most pernicious of any kind of diet." (p. 72.)

Dr. Bryson states the remittent fever of China to be, in all its essential characters,

> "Identical with that which prevails at all times and seasons on the coasts of Africa and America within the tropics, but in no instance did it present the true characteristics of the yellow fever peculiar to those regions. Yellowness of the skin was not an unfrequent symptom, but veritable black vomit does not appear to have been once observed. Moreover, in the fatal cases, the disease did not destroy its victim so rapidly as does the latter malady; while the proportion of deaths to the number of attacks was widely different. In the fevers which occurred on the Chinese coast, the proportion of deaths to the number of attacks did not exceed one in thirty-three; whereas in epidemic yellow fever it is seldom less than one in four or five." (p. 88.)

It does not appear in any instance to have been communicated by contagion.

Eruptive Fevers.—The admissions by this class were 38 cases of small-pox, of which 7 died. From this high rate of mortality, it seems probable that a large proportion of the cases were not protected by vaccination. In consequence of the great difficulty of preventing small-pox spreading on shipboard, by complete seclusion of the cases, Dr. Bryson recommends that no person should be admitted into the service till he has been vaccinated, or if entered, that he should be at once sent to a naval hospital, there to remain till he had passed through the vaccine disease; and if it could not be induced in any instance, the person "might be rejected, on the ground of his liability to introduce variola among a healthy ship's company."

Diseases of the Brain.—Apoplexy has been the most fatal disease of this class, 16 deaths having occurred out of 31 cases. Notwithstanding the frequent exposure of the men to the excessive heat, only 10 were admitted with coup-de-soleil, and of these, 8 recovered. The most prevalent disease was epilepsy, of which there were 85 cases, being rather more than 3 per 1000 of the strength; and of these, 25 were invalided, and 1 died. We have before had occasion to remark on the prevalence of this disease in the navy, where it is generally considerably higher than in the army serving on the same station. Delirium tremens was much more prevalent and fatal than during the previous seven years, owing, doubtless, to the facility with which the men procured abundance of sham-soo, a spirituous liquor of the vilest quality.

In the remarks upon this class of diseases in 1842, there is a mistake of an extraordinary kind—a case of pulmonary apoplexy included among the diseases of the brain.

Diseases of the Respiratory Organs.—As compared with the period formerly reported on, there is an increase both in the admissions and deaths by this class. The proportion is very slightly higher in the cases of hæmoptysis and phthisis, but that of the deaths is greater by about a third; a result probably attributable to fewer having been invalided, from the want of opportunities of sending them home. This seems to bear strongly on the question of the alleged influence of marshes in preventing or even arresting phthisis. Had such been really the case, instead of an increase we should have expected a total cessation of this disease, for it would hardly be possible to find a place in which marsh miasm was more abundant, or its influence upon the persons exposed to it more unmistakably evidenced.

Dr. Bryson makes an observation with reference to the prevalence of catarrh, which tends to show that it does not arise so much from low temperature, or even from sudden alternations, as from some peculiar condition of the atmosphere:

> "In the *Wellesley*, while in the Persian Gulf, catarrh prevailed during the months of April, May, and June, when the range of the thermometer extended sometimes from 64° to 85° in one day; consequently, it might be supposed that the disease depended on these sudden alternations of temperature; but the cases were even more numerous in July, in Trincomalee harbour, when, with fine dry weather, and a south-westerly monsoon blowing, it ranged from 78° to 90° only." (p. 20.)

Diseases of the Stomach and Bowels furnished a higher proportion of cases and deaths than any other class, amounting to one-fourth of the number treated, and to nearly half the total mortality. The principal diseases were diarrhœa, dysentery, and cholera; the two former, though at all times a source of considerable inefficiency and loss on this station, were doubled in frequency, and increased fourfold in mortality, during the second period. On the coast of China, dysentery and diarrhœa appeared on almost every occasion to be associated with the remittent or intermittent fevers; sometimes following, at others co-existing, and at others alternating with them. The only exception to this was in the small squadron which entered the Yellow Sea in 1840, and in which dysentery prevailed for several weeks without any corresponding eruption of fever, or increase even in the number of aguish paroxysms. This co-existence of the diseases does not appear to have been observed in the other divisions of the station.

It seems to have been a general opinion among the navy medical officers, that these diseases were, like fevers, the product of marsh miasm. From this conclusion, however, Dr. Bryson dissents, because on the west coast of Africa, and in the estuaries of the larger rivers of America, where marshes abound, dysentery is by no means constantly, or even usually, found co-existent or alternating with remittent fever. He rather favours the hypothesis of its origin from infusorial animalcules of a poisonous nature:

> "Notwithstanding what has been said," he observes, "as to the improbability that there is contained in the water of large rivers a sufficiency of decayed, or dead organic or inorganic matter, in whatever form, to account for the production of acute dysentery, still bearing in mind the general presence of intestinal worms in a large proportion of the cases, and the recovery of the patients—still free from organic disease—after their expulsion; and likewise bearing in mind the difference in the nature of the soil and products of the low lands of China, as compared with the soil and products of other marshy countries—it seems to the writer that, after all, we must look to the swamp, and river water *drained from the swamps*, for the chief offending cause, in which it seems more reasonable to suppose it may exist, in the form of some unknown tribe of infusorial animalcules of a poisonous nature, or in the form of minute organic germs either of the animal or vegetable world, rather than in the form of mineral salts, or the *débris* of organic bodies, as has been generally supposed. It is also to be observed, that the season of the year in which dysentery was most prevalent, was precisely that in which the reproductive powers both of the animal and vegetable world are most vigorously called into action. It has been already noticed that the disease occasionally made its appearance before there was any communication with the land, and before any of the water had been used; but in these instances it was generally slight, and more of a diarrhœal than a dysenteric nature—similar, in fact, to that which sometimes follows a sudden change from hot to cold weather within the tropics; and were it not that it would look like pushing the question to the very verge of probability, it might even be supposed that the rich and putrid manures which the Chinese so abundantly spread upon their grounds, or the disorganized matter produced by the decay of rice crops, either singly or combined, may form the kind of soil or swamp which is best adapted for the development of these minute organisms, which, if not in their natural, at all events in their metamorphosed forms, prove so detrimental to the lining membrane of the alimentary canal." (p. 89.)

We must confess, however, that we think the evidence in favour of malaria being the common, though probably not the sole cause of dysentery on the Chinese coasts, appears to be much the stronger; while it cannot be doubted that the quality of the water exerted a very injurious influence, both in exciting and maintaining the disease. Intestinal worms were present in a large proportion of the cases of dysentery and diarrhœa. They were passed in considerable numbers, sometimes naturally, at others from medicine, and after death were found in all parts of the intestinal canal.

During the seven years included in the Report, 3269 cases of dysentery occurred, of which 366 proved fatal, being 1 in 9; and of diarrhœa, 55 died out of 7368, being 1 in 134. Dr. Bryson remarks, that

> "The total deaths, however, give but a faint idea of the real mortality resulting from these affections; for were it possible to trace out men who were invalided and discharged from the service, when their respective vessels returned to England, but who, from ill health, could not rejoin it, together with the men who did re-enter but proceeded to other stations, it would be found that the figures do not express a moiety of the mortality. . . . . . They are maladies which, once fairly engrafted on the system, never leave it till life itself becomes extinct." (p. 74.)

And in another place, he says:

> "There may be diseases of a more fatal character than dysentery, but there are few which entail so great an amount of suffering; for when once it has passed into the chronic form, it slowly, but not the less surely, continues, by a most loathsome process, to exhaust the vital energies, until death relieves the patient of an existence rendered almost intolerable by pain, debility, and the offensive nature of the discharges." (p. 40.)

Every conceivable mode of treatment was tried in this disease, but with very indifferent success unless accompanied with change of climate. The following summary, by the surgeon of the hospital ship, *Minden*, must, we fear, too truly represent the general result of the treatment of dysentery, when carried on in the very midst of those causes which originally produced the disease:

> "He was of opinion that no one remedy was better than another, so far as their curative effects were concerned; he had seen all the astringents, both mineral and vegetable, mercury, externally and internally, with many other medicines, tried, without any benefit; but there were some which were useful as a means of relieving the more urgent and distressing symptoms, and, as it were, smoothing the path to the grave. Amongst these he mentions a well-regulated farinaceous diet, opium suppositories, anodynes, astringent injections, minute doses of calomel in combination with opium, absorbents, buchu, cascarilla, resinous astringents, and the application of leeches to the rectum when tenesmus was distressing, or over the course of the colon when there was deep-seated pain. It is melancholy to reflect how little these maladies in their more advanced stages, and also in their more acute forms, appear to have been under the control of medicine. When taken early, however, and while the symptoms were still those of simple diarrhœa, they seem to have been easily arrested by appropriate remedies: but when they were complicated with intermitting fever, or when they began to assume a chronic form, or when relapses were frequent, the only rational means of arresting their fatal tendency appear to have been the immediate removal of the patients to a colder climate." (p. 77.)

The value of change as a curative measure is strikingly illustrated by the rapidity with which the crews of the various vessels improved in health almost immediately after quitting the station, and the speedy disappearance of diseases of the bowels among them, except in cases where the dysentery had become chronic, and the men's constitutions been greatly, and in many instances permanently, impaired by tropical disease.

> "Unquestionably the health, and probably the lives, of many men would, for a time at least, be preserved, if it were possible to remove them from such stations as China and the east and west coasts of Africa, into a more healthy climate, as soon as they became affected either with chronic dysentery or ague. It would even be an advantage to the service, in so far as its efficiency is concerned, to send such men home, because the space they occupy is wanted for other purposes; they are in the way, and may occasionally create despondency and discontent, where hilarity and cheerfulness would otherwise prevail. There may have been times when invaliding was carried too far; but even setting aside the question as one of humanity, there can be no greater mistake committed than to attempt to lessen the number of invalids sent annually from these stations, by retaining men upon them who are worn out by repeated attacks of these intractable maladies." (p. 55.)

It is stated in the Report that the natives of China boil the water they intend for internal use, and afterwards add a little alum to it. As they suffer but little from dysentery, it might be worth while to try this as a prophylactic on any station where the disease is very prevalent, and the quality of the water, as in China, very indifferent.

Cholera did not prevail to any great extent, except during the last two years included in the Report, in which 294 cases and 80 deaths occurred, being in the ratio of 21·4 and 5·8 per thousand of the strength. In 1842, the disease was confined chiefly to the ships at Chiang Kiang Foo, Woosung, and Nanking, in the Chinese division of the station; and at Bombay and Calcutta.

> "On a careful perusal of the medical reports from the squadron, it appears that in every vessel employed in the Yang-tse-Kiang, from Woosung to Nanking, between the middle of July and the middle of October, cholera or choleraic diarrhœa broke out; while not a case of either form of the disease appeared in vessels employed on other parts of this division of the station, with the exception of those which arrived at Chusan from the Yang-tse-Kiang." (p. 59.)

In 1843, Amoy, Hong Kong, Chusan, and Manilla are the places at which the disease is stated to have appeared in the ships.

Of the treatment it is stated that

> "There were not two medical officers in the squadron that agreed as to the same remedial measures; while, as a general rule, each considered his own, even although every second or third case proved fatal, to have been attended with some good effect." (p. 59.)

The reports, however, prove the advantage of removal from the locality in which the disease broke out. Thus, in 1843, it is recorded that after the *Agincourt* left Manilla on the 28th of April,

> "Although attacks continued to occur up to the 12th of May, as soon as the vessel was well clear of the atmosphere of the port, they gradually became of less frequent occurrence, and assumed a milder form." (p. 81.)

And of the *Plover* it is stated "that as soon as the disease made its appearance, she went to sea, after which no more cases occurred."

The remaining classes of diseases appear to call for but few remarks. Among the diseases of the senses are recorded 37 cases of hemeralopia; of these 30 occurred on board the *Winchester*, in 1838, on her homeward voyage, and are considered by Dr. Bryson to have been one of the early manifestations of scorbutus.

> "Whether the men had not been supplied with vegetables and fresh meat, or whether the rations on which they were victualled were bad in quality, it is now impossible to ascertain from anything that has been stated in the medical reports; but that a change of diet speedily removed the disease is sufficiently apparent. During the eight days that the vessel remained at the Cape of Good Hope, where the crew were supplied with fresh beef and vegetables, the whole of the cases rapidly recovered, and the patients were discharged to their several duties; but on resuming the use of sea rations it again made its appearance, and continued to resist the various modes of treatment which were adopted until the men were supplied with fresh provisions after their arrival in England, when it speedily began to decline." (p. 16.)

To these remarks Dr. Bryson adds a judicious caution:

> "As this affection in the naval service, and probably amongst other bodies of men, has been mistaken for that kind of night blindness which depends on hyperæmia, or on causes of a sthenic character, it is important to remark that it will not generally yield to remedial measures which include bleeding and blistering, with the exhibition of purgatives, and the preparations of mercury; but when several cases occur about the same time, as was the case in the *Winchester*, it will readily yield to a more generous diet; and in these instances a careful inspection of the men will generally lead to the detection of a glairy or a spongy state of the gums in some portion of the crew." (p. 16.)

Scurvy appears to have prevailed to any extent only in two of the years under review. In 1839, 55 cases occurred among the ship's company of the *Alligator*, disembarked at Port Essington, Australia, to form a settlement. After a residence of four months, during which they lived chiefly on the provisions brought with them from England, scurvy broke out, and by the end of the second quarter they were compelled to sail for Sydney. An abundant supply of fresh meat, vegetables, and fruit was procured there, and the disease rapidly disappeared. In 1842 again, 25 cases occurred in the squadron on the coast of China, and there was a scorbutic taint observed among the crews of several ships. "The appearance of this disease in the fleet seems to have been due to the scarcity of fresh provisions while it was in the Yang-tse-Kiang."

It is worthy of note that the number treated for wounds, accidents, and injuries, and the deaths by these and by drowning, were rather lower during the period of active service than in the three years of peace. The difference, however, may be more apparent than real, as the proportion of deaths whereof the cause was not reported was very much higher, and in all probability consisted chiefly of such cases.

The influence of season in the production of fevers and bowel complaints was most marked. During the four years the fleet was chiefly employed on the coast of China, the number treated in each quarter was as follows:

	1st quarter.	2nd quarter.	3rd quarter.	4th quarter.
Fevers	960 ...	2633 ...	4912 ...	2489
Dysentery and diarrhœa.	987 ...	2025 ...	4477 ...	2154

Thus it appears that both these classes were twice as prevalent in the third as in any other quarter, while in the first quarter the numbers fall much below the average of the year.

In reference to this subject Dr. Bryson remarks:

> "There is at least one important fact deducible from the preceding details, which may be of essential service to the officers in command of any European force employed on the coast of China, namely, a knowledge of the period of the year when dysentery and fever prevail with the greatest severity. It may be safely assumed as an axiom, that no expedition up any of the rivers or on shore, even on the adjacent islands of Chusan, can be undertaken between June and October, without the risk of incurring a large amount of sickness; whereas during the colder months of winter the risk is reduced to a mere nullity as regards endemial disease, or indeed any other disease besides those peculiar to regions lying within the temperate zone." (p. 92.)

We have repeatedly had occasion to remark how little influence season appears to exert on rheumatic affections, and these observations are quite borne out by the present Report, the numbers treated having been 445 in the first quarter, 456 in the second, 505 in the third, and 458 in the fourth.

We have already noticed the improvements introduced into these Reports by Dr. Bryson, but there appears still room for amelioration. To render Table No. 1 complete, the addition of the aggregate strength for the year is required; and the value of Table No. 2 would be materially enhanced by the addition of the class of the ships, and the average strength of their companies.

The remarks on the treatment of the diseases in the various ships would also bear considerable curtailment. Although in general very good, they are of too departmental a character; addressed to the officer whose treatment is commented upon, they might prove useful, but are scarcely suited to a Report of a statistical character, or calculated to benefit the general reader.

While we feel it our duty to offer these suggestions, we must bear testimony to the zeal and ability with which Dr. Bryson has executed his task; and when it is considered that the Report was prepared at such times as he could snatch from his onerous duties as professional assistant to the Director-general of the Navy Medical Department, it must be admitted to be a work which redounds greatly to his credit, and a monument of industry of which the department has just reason to be proud.

REVIEW VI.

The Pathology and Treatment of Leucorrhœa. By W. TYLER SMITH, M.D., Member of the Royal College of Physicians, Physician Accoucheur to St. Mary's Hospital, &c. &c.—*London*, 1855. 8vo, pp. 217.

ONE would naturally imagine, on first seeing an octavo volume, containing no less than 217 pages, on "Leucorrhœa," that the author of such a work on such a subject must indeed be the victim of a *cacoëthes scribendi* but little suited to command attention from the members of so practical a profession as our own. But on looking into Dr. Tyler Smith's work, for the purpose of reviewing it, and not without certain misgivings as to the pleasure or profit of the undertaking, we are glad to find that it possesses much more interest than its title would imply, and that it tends to remove obscurity from a difficult subject. Indeed, we are much mistaken if it be not the means of bringing fresh interest to bear upon many subjects connected with uterine pathology; and it is not unreasonable to expect improvements in practice as the result. Dr. Tyler Smith has employed the term "leucorrhœa" in a general sense, and under it includes most of the diseases of which it is a prominent symptom, believing that it groups together a large number of disordered conditions more conveniently, than any other now in use. In the work to which we refer, he has given, in the first instance, a description of the minute anatomy of the vagina, and of the os and cervix uteri, and the secretions of these parts in their normal states; the microscopical pathology of the same mucous surfaces, and their morbid secretions in leucorrhœal disorders follow; and the treatment of this class of affections forms the conclusion. It will be our endeavour to present our readers with the views of Dr. Tyler Smith with such condensation as appears compatible with clearness of description; avoiding much reference to what has been done by other pathologists towards the advancement of this department of medicine, and leaving the greater part of our author's statements and reasoning to be obtained by a careful perusal of the work itself.

The chief points in the anatomy of the organs concerned in the phenomena of leucorrhœa are, the differences between the mucous membrane of the vagina and that of the cavity of the cervix uteri; and the situation of the mucous glands.

"The mucous membrane of the vagina is studded with large papillæ, or villi, which are very numerous in the lower part of the canal, but diminish in number on passing upwards towards the os uteri. The whole of the vaginal mucous membrane is covered by a layer of pavement epithelium, which is thicker in the upper part of the vagina than at the orifice. *The coating of epithelium, and the sub-epithelial papillæ, are the parts of the vagina most largely concerned in vaginal leucorrhœa.* The mucous membrane of the os and cervix uteri may be divided into two tracts, one comprising the surface of the os uteri and external portion of the cervix, the other being the mucous lining of the canal, or cavity of the cervix. The layer of epithelium found in the former situation is tesselated or squamous, and arranged so as to form a membrane of considerable thickness; it closely resembles the epithelial covering of the vagina, with which it is continuous. Immediately beneath the layer of epithelium, the basement membrane is found, covering numerous villi, or papillæ, which stud the whole surface of the mucous membrane. The thick layer of epithelium and the villi, with their looped vessels, appear to be the principal anatomical features of the mucous membrane of the os uteri and external portion of the cervix. On passing within the os uteri, a small tract of smooth surface is generally found between the margin of the lips of the os uteri and the commencement of the penniform rugæ; and here the mucous membrane appears more delicate and vascular than the mucous membrane of the external portion of the os uteri. The mucous membrane immediately within the os uteri is found to be composed of cylinder epithelium arranged upon villi (somewhat after the manner of the epithelium covering the villi of the intestinal canal), of basement membrane, and of sub-mucous tissue. The villi in this situation are three or four times larger than the villi of the external portion of the os uteri. The os and cervix uteri are covered with epithelium, which may be abraded, and it contains papillæ, or villi, which, when denuded or hypertrophied, may present all the appearances of granulations, and have probably been mistaken for them. The large vascular loops contained in the villi of the os uteri, and particularly of the lower part of the canal of the cervix, also afford the explanation of the sanguineous discharges from the os and cervix uteri, so frequently met with in cases of leucorrhœa."

Our author takes objection to the descriptions hitherto given of the canal of the cervix uteri, and considers the most careful of them to be imperfect. It will probably be interesting to our readers to be made acquainted with Dr. Tyler Smith's views of this important subject, and we quote his description in his own words:

"*Rugæ of the Cervical Canal.*—When the cavity of the cervix belonging to a virgin uterus is laid open by a longitudinal incision, so as to expose the whole of the cervical canal, the internal surface is generally found to contain four columns of rugæ, or folds of mucous membrane, the rugæ being arranged in an oblique, curved, or transverse direction. Between these columns, four longitudinal grooves or ridges are usually seen Of these, the two in the median line, anteriorly and posteriorly, are the most distinct. The other longitudinal markings are situated, one on each side, between the anterior and posterior walls of the cervix, beginning below at the angles dividing the anterior and posterior lips of the os uteri. The canal of the uterus is flattened in shape, and two of the rugous columns are arranged on the anterior surface, corresponding to the anterior lip, and the other two upon the posterior lip; the posterior half of the cervix being the larger of the two, and containing the greater number of rugæ. The sulcus or division between the posterior rugous columns, is also generally more strongly marked than the sulcus dividing the anterior rugous columns. The rugæ of each column, as seen by the eye alone, vary from about ten to fifteen in number. In the intervals between the columns, numerous small longitudinal folds may be seen, but these are less distinct than the transverse rugæ. In the healthy state, the transverse rugæ, with the fossæ between them, are covered with a viscid and transparent mucus, and when this is brushed away, a reticulated appearance, caused by numbers of secondary rugæ, is visible in the mucous membrane beneath. Besides the four rugous columns and the furrows between them, which are found in the well-developed cervix, other rugæ of irregular shape are seen, particularly at the upper and lower portions of the cervix, where the regular, transverse, or oblique rugæ become indistinct. After pregnancy and child-bearing, these arrangements of the mucous membrane become, to some extent, confused and irregular, though the follicular structure remains. This is not to be wondered at when we consider the changes which occur from the development of the cervix in pregnancy, and the great dilatation of this part of the uterus during the passage of the child in parturition. Probably, it is owing to the great extent of the reduplication of the mucous membrane of the cervix that laceration of the mucous surface of the cervix does not occur more frequently during labour.

"*Glandular Follicles of the Cervical Canal.*—If we take a section of a virgin cervix uteri, containing one of the longitudinal columns only, and magnify it nine diameters, we obtain a clear insight into the glandular structure of the cervical canal. The transverse ridges now stand out with great prominence. Besides the primary rugæ, each fossa is seen to be subdivided by smaller rugæ, from which curved septa, still more minute, take their origin, dividing the principal fossa into a great number of crypts, arranged like a fine piece of net-work. In each of the fossæ between the primary rugæ, as many as from forty to fifty crypts or laminæ may be seen. A cervix of moderate size would show between the transverse rugæ of the four columns alone, with this low magnifying power, from two to three thousand follicular pits. But, besides the fossæ between the rugæ, the spaces between the rugous columns and the longitudinal sulci themselves, are all seen to be covered by multitudes of mucous follicles. Small plicæ are everywhere visible, and these are evidently only a repetition of the columnar rugæ, on a lesser scale. This is particularly the case with respect to the larger extremities of the transverse rugæ, all of which are closely studded with mucous pits.

"If a portion of the cervical mucous membrane be magnified still further to the extent of eighteen diameters, so as to take only two or three of the primary ridges and fossæ into the field, it will be seen that the rugæ themselves, and even the secondary septa, are covered in the greater part of their length with mucous follicles. The crypts in the furrows are still further divided and sub-divided, so as to double or treble the number of follicles and laminæ seen with the lower power. In a portion of the cervix, comprising only three rugæ, and their two interspaces, upwards of five hundred mucous follicles were easily counted, so that it is within the limits of moderation to say that a well-developed virgin cervix uteri must contain at least ten thousand mucous follicles.

"When a longitudinal section is made through the middle of one of the rugous columns, and viewed laterally, the fossæ are found to extend obliquely and deeply into the substance of the cervix, sometimes to the extent of the sixth of an inch or more; and occasionally mucous openings pass into the centre of the walls of the cervix, and may be seen filled with the tenacious mucus proper to the cervical canal.

"Besides the anatomical arrangements already described, the superficial surface of this part of the mucous membrane of the cervical canal is further increased by the presence of villi similar to those found in the lower part of the cervix. These villi extend to the glandular surface of the canal, and are found in considerable numbers on the larger rugæ, and other parts of the mucous membrane in this situation. Thus the entire organization and disposal of the mucous membrane lining the canal of the cervix uteri is such as to afford a very large extent of glandular surface for the purposes of secretion. In effect, the cervix uteri is an open gland, and it performs all the functions fulfilled by glands in other situations.

"I may here refer to a point which should not be lost sight of, bearing, as it does, upon the pathology and treatment of leucorrhœa, and some other disorders of the os and cervix uteri—namely, the great similarity which exists between the skin and the mucous membrane of the vagina, and of the external portion of the os and cervix uteri. The resemblances of the mucous membrane in these situations are certainly much nearer to the cutaneous structures than to the mucous membranes of more internal parts. This is particularly the case with respect to the dense epithelial layer of the vagina and os uteri; and the villi of the os uteri are perhaps more nearly allied to the papillæ of the skin than to the villi of the intestinal mucous membrane. The surface of the vagina, and the external portion of the os and cervix, like that of the skin, is constantly acid; while within the cervical canal the surface is as constantly alkaline. These analogies are strongly confirmed by what is observed of the pathological lesions to which these parts are liable, and by the effects of therapeutical applications. Several of the common skin affections are closely imitated on the vaginal surface and the vaginal portion of the cervix uteri, and give way to treatment adapted for genuine skin diseases.

"The epithelium found upon the follicular surface of the canal of the cervix is cylindrical or dentated, like the epithelium just within the os. It is also ciliated low down in the cervix, but not at its very lowest part, and the ciliated character is continued into the cavity of the fundus uteri. The villi found in the upper portion of the cervix are covered by dentated epithelium, just as is the case with the villi of the lowest part of the cervix. Mixed with the epithelium of the follicular surface of the cervix, a considerable number of caudate corpuscles are frequently found, each having a distinct central nucleus. These are probably nothing more than altered epithelial particles. The epithelium of the os uteri and external portion of the cervix is, like that of the vagina, constantly squamous; the epithelium just within the os uteri is cylindrical but not ciliated. The situation in which cilia are first found in ascending the utero-vaginal tract varies a little in different subjects; but I believe it will be found that the transition from squamous to dentated epithelium constantly occurs just at the margin of the os uteri.

"It will be seen that from the nymphæ to the entrance of the fundus uteri, the glandular structures are arranged at two principal stations—namely, at the ostium vaginæ, or the cervix vaginæ, as it might be called, and at the cervix uteri. There is no apparatus for any considerable mucous secretion in the space between these two points." (pp. 21—30.)

We have thus given in detail Dr. Tyler Smith's anatomical description of the parts concerned in the production of leucorrhœa, for the purpose of enabling our readers to understand and judge of the superstructure founded upon it. No doubt can be entertained as to the importance of the arrangement of structures thus described, especially in connexion with the diseases to which these parts are liable; particularly the glandular apparatus at either end of the vaginal canal, and the differences between the mucous membrane of the vagina, cervix, and os uteri, and that of the cavity of the cervix and fundus uteri. We will now proceed to consider some of the practical points elucidated by, or dependent upon, these anatomical peculiarities. The secretions of the vagina and of the os and cervix uteri and the cervical canal, in the healthy state, differ in their nature and appearance according to the anatomical peculiarities before described. The sebaceous follicles, or fat-glands, of the vulva and external parts of generation secrete an oily matter, to defend the vulva from friction, and to preserve the surface from the irritation of the uterine and vaginal secretions, and of the urine. The secretion of the mucous glands of the ostium vaginæ is said to be connected with the sexual function, and to be increased under excitement; and, like other vaginal secretions, it has an acid reaction. The mucus of the vaginal canal is only secreted in sufficient quantity to keep the mucous surface in a state of lubrication; it lies upon the mucous membrane as a milky fluid, containing quantities of small curdly points or masses, and consists of a transparent or semi-transparent plasma, containing an abundance of scaly epithelium and its *débris*. It is distinctly acid, and its curdled appearance is attributable to the effect of the acid in coagulating the albumen of the mucus.

In the healthy state of the uterine organs, the mucous crypts and the canal of the cervix are generally found filled with a clear, viscid mucus, so as to entirely block up the passage from the vagina to the cavity of the fundus. At each catamenial period the whole of the tenacious plug of mucus must be washed away by the menstrual fluid; but in a few days after the completion of the period, the mucous plug is again formed. This cervical mucus is alkaline, and our author considers its use to be twofold. In the first place, it closes the cervix uteri, and defends the cavity of the fundus from external agencies as completely as though it were a shut sac. In the second place, it appears to afford a suitable medium for the passage of the spermatozoa through the cervix uteri into the uterine cavity. During pregnancy, the plug of mucus, when it is once formed, continues for the most part unremoved up to the commencement of labour, at which period it is discharged from the cervix, in consequence of the incipient dilatation of the os uteri, and the secretion of a quantity of mucus having a more fluid character than the plug of pregnancy itself; and the mucus which lubricates the vagina during the act of parturition, instead of being, as has usually been supposed, a vaginal secretion, is chiefly the product of the glands of the cervical canal, and is almost identical with the secretion of the glandular structures of the cervix uteri in the unimpregnated state, or during pregnancy. The following remarks upon the discrepancies of opinion amongst authors respecting the nature and sources of the utero-vaginal secretions are of practical import:

> "Nothing has contributed to perpetuate these discrepancies so much as the different chemical conditions of the secretions of the vagina and the cervical canal. However long the alkaline cervical secretion remains within the cervical canal, and removed from contact with the acid of the vagina, it is generally transparent or semi-transparent. It evidently owes its transparency to the alkali it contains. But the acid vaginal secretion cannot remain upon the surface of the vagina without becoming curdled and opaque. This as evidently depends on the vaginal acid. The acid of the vaginal mucus has, however, the same effect on the clear viscid mucus of the cervix, whenever it comes in contact with it, an effect which may be imitated out of the body. The importance of this fact has not been perceived in the examinations of the utero-vaginal discharges. As the proper mucus of the vagina is, when in any quantity, curdy or creamy in its appearance, it has been thought that, whenever a discharge of this kind has been found in the vagina, it must have been formed there. It is so unlike the transparent tenacious mucus of the cervix, that the cervical canal has not been suspected as the source of such discharges. The truth is, however, that whenever any of the clear cervical mucus passes into the vagina, the acid secretion assimilates it to the vaginal secretion so exactly, that without the microscope it would be impossible to perceive any difference between this and the strictly vaginal mucus. Hence a fertile source of mistake, and a tendency to attribute the cervical secretion, when found in the vagina, to the vagina itself." (p. 49.)

Based upon the anatomical and physiological peculiarities of different portions of the utero-vaginal canal, our author describes two principal forms of leucorrhœa. The first, and the most frequent and important, is the mucous variety, consisting chiefly of mucous corpuscles and plasma, and secreted chiefly by the follicular canal of the cervix. The second is the epithelial variety, in which the discharge is vaginal, or is secreted by the vaginal portion of the os and cervix, and consists for the most part of scaly epithelium and its *débris.*

"The following are the elements found in the discharges in vaginal or epithelial leucorrhœa of different degrees of severity:

1. Acid plasma.
2. Scaly epithelium.
3. Pus corpuscles.
4. Blood globules.
5. Fatty matter.

"The following are the elements found in the different forms of cervical or mucous leucorrhœa:

1. Alkaline plasma.
2. Mucous corpuscles.
3. Altered cylinder epithelium.
4. Pus corpuscles.
5. Blood globules.
6. Fatty particles."

It is said that it is easy to pronounce, without microscopical examination, in ordinary cases, whether the discharge be vaginal or cervical.

"The transparent gelatinous-looking discharge from the cervical canal, and the same discharge rendered white and soapy, or coagulated into masses by the vaginal acid, is easily distinguished from the white, milky, or creamy discharge from the vagina. The only mistake likely to occur is when the cervical discharge is so curded and broken down in the vagina by the action of the acid as to resemble the vaginal discharge. But these points are readily determined by an examination of the os and cervix uteri and the vagina. The flaky discharge in epithelial disorganization is unmistakeable." (p. 62.)

Under the head of the sequelæ of leucorrhœa, are comprised inflammation, abrasion, ulceration, induration, and hypertrophy of the os and cervix uteri, and abrasion and superficial ulceration of the vagina. Time and space will not admit of our entering fully into these subjects, but our author contends that in the majority of cases in which leucorrhœa is present, in combination with non-malignant disease of the os and cervix, the morbidly active condition of the cervical glands is the primary and essential disorder; that it is extremely rare for cervical leucorrhœa to exist without inducing disorder of the os uteri; and, on the other hand, that disorder of the os uteri very rarely occurs without exciting leucorrhœa, and the leucorrhœa thus excited is almost sure to aggravate the original disorder. These are views which are borne out by daily experience of cases of leucorrhœa, and may, we think, be entertained without much doubt of their correctness.

In another chapter, the relations between secondary syphilis and leucorrhœa are considered, and we strongly recommend a perusal of it to all who desire to understand this difficult and painfully interesting subject. Dr. Tyler Smith believes that in almost all cases in which leucorrhœa and disease of the os and cervix uteri are present in women suffering from constitutional syphilis, the uterine symptoms are a genuine manifestation of the constitutional or secondary disorder; and that syphilitic leucorrhœa is, in the female subject, almost as common a manifestation of secondary syphilis as sore throat. It is a lamentable reflection, that where a person has had syphilis before marriage, and is liable to occasional outbreaks of secondary or tertiary disorders, he may entail disease upon his offspring; and that disease thus imparted to the ovum may be still further conveyed to the mother by means of the fœtal circulation, thus endangering the life of both. Frequent abortions, and general ill health, are the consequences often affecting the wife of a person who had previously suffered from syphilis; and the death of the ovum, or a sickly unhealthy child, are other not less disastrous results. How desirable ought it to be considered to enlighten the general public on a point of such vital import; and that in contracting for marriage, not only should pecuniary arrangements, and other worldly matters likely to contribute to the happiness of individuals about to enter upon the duties of matrimonial life, be carefully and judiciously entertained, but also the far more important consideration whether the party, who may have been led into the irregularities unfortunately so common to youth, be so entirely free from any taint of syphilis as to secure his future wife from danger, and his children from broken and unhealthy constitutions. No doubt it is a subject of great delicacy, requiring cautious management; but it ought to actuate the breast of every man who knows himself to have been the subject of venereal disease, to ascertain by every available means, before marriage, that all taint has been removed. Many may enter into that state in ignorance of the possible consequences; it is our duty to make such consequences known, and then it will remain with the conscience of every man thus instructed to do his best to prevent suffering, disease, and death, to those most nearly connected with him, from some remaining effects of his own previous indiscretions.

Before passing on to the treatment of the affections included by our author under the term Leucorrhœa, we may just refer to a subject which has received various explanations, namely, the anatomy and pathology of the ovula Nabothi. They have generally been considered as obstructed follicles, but Dr. Tyler Smith is of an entirely different opinion. He observes, they occur very frequently upon the vaginal portion of the cervix uteri, outside the os, where, although it has been generally considered plenty of mucous follicles exist, the microscope fails to detect any follicular structure; and these small cysts or vesicles seldom appear except in diseased states of the os and cervix uteri. He concludes that under the designation of ovules of Naboth, several dissimilar conditions—such as specific and simple eruptions of the os uteri, cysts or vesicles developed upon the mucous membrane, and possibly obstructed follicles—have all been grouped together.

There is nothing more certain in medical practice than the fact, that those who attempt to cure local symptoms on the principle of considering them as manifestations simply of local disease, are very often disappointed in their expectations of effecting a cure; and so, in leucorrhœa, the constitutional as well as the local causes must be taken into account, if we hope to be successful. Our author very properly insists upon this necessary connexion between local and general causes: amongst the latter he enumerates plethora, debility, prolonged lactation, the strumous habit, skin diseases, influence of climate; and amongst the former, rectal irritation, vesical and urethral irritation, vaginal and uterine irritation, gestation, abortion and labour, &c. He argues strongly against the views of many, that almost all the conditions upon which leucorrhœa depends may be referred to *inflammation of the os and cervix uteri.* That such is not really the case, daily experience convinces us; and we feel sure that more careful and extended observations will satisfy those who think that almost every case they examine presents ulcerations of the os and cervix uteri, that many such are of a nature entirely different from inflammation and ulceration, and are simply epithelial abrasions; and, moreover, that the remedies for inflammation are not the most suitable for their cure. We have long suspected that the frequency and importance of inflammations and ulcerations of the os and cervix have been much overrated, and that the discrepancies between different observers upon this point have had their origin in a mistaken diagnosis. We entirely agree with Dr. Tyler Smith, that "the term *epithelial abrasion* should, in the great majority of the cases, take the place of *ulceration;* and that the words *irritation* or *relaxation* should generally take the place which has been assigned to *inflammation.*"

With regard to the treatment of leucorrhœa generally, it does not appear that advancement has been made at all in proportion to the increased employment of local investigations and applications. Before the minds of physicians were so much engaged about the use of the speculum, and the appearances it unfolded to their sight, they seemed to take a more general and impartial view of the disease, and treated it upon those general principles, constitutionally and locally, which in most diseases are the surest guide to recovery. And although it must in fairness be acknowledged, that by the speculum we have been enabled to discover and treat successfully many conditions which might have escaped our notice otherwise, it cannot, at the same time, be denied that the instrument has been subjected to great abuse—that it has been employed often more as a matter of course than of necessity—that it has sometimes been used for the purpose, it would almost seem, of impressing a patient with a greater notion of the capability and accuracy of the doctor, as compared with others who may have thought it unnecessary—and last, though not least, that its general application has engendered in the minds of some patients a morbid desire to know what it reveals, and had a tendency to abate that delicate sensitiveness on the part of females which ought to protect them from every unnecessary infringement of their natural modesty and strictly moral feelings. It may be thought we are straining a point in thus combating an indiscriminate use of the speculum; but we do so under a firm persuasion that it is very often used without occasion, that its abuse is gradually becoming more general and frequent, and that when so employed it has a tendency to undermine those bulwarks which are the protection of the weaker sex against improper and indecent interference with their purity of mind and body. Against the genuine and necessary employment of the speculum we should be the last to object, and, with that qualification, would rather rejoice in having been supplied with so useful a means of distinguishing and treating many conditions of disease.

There are few cases of leucorrhœa that do not require both constitutional and local treatment, and an experienced physician will meet with but little difficulty in determining the nature of the former. The value of iron is indisputable, but our author speaks highly of a particular form of it of which we ourselves have at present had no experience. It is a combination called iron alum, of which he mentions two kinds; one being a double sulphate of the protoxide of iron and potash; and the other a double sulphate of the protoxide of iron and ammonia. The first he calls potash iron alum; the last, ammonia iron alum; and says:

> "I have prescribed the iron alum with ammonia, which I now prefer, in most cases, to the similar salt with potash, because of its greater solubility, in doses of from three to six grains, in infusion of calumba, or in simple water, with some warm tincture, three times a-day. It is similar in its action to the sesquichloride of iron; but while it is equal to, or perhaps more effective than, this medicine as an astringent, it is less stimulating, more easily assimilated, and seldom causes any nausea or headache. It generally produces a slight tendency to constipation, which may be obviated by an occasional aperient." (p. 194.)

With regard to the local treatment of leucorrhœa, we have found, from no little experience, that, provided suitable constitutional means have been put into operation, there are many cases in which nothing more than daily washings-out of the vagina, and the strictest attention to rest and cleanliness, will be required. Our habit is almost invariably, in the first instance, to advise the use of tepid or cold water daily; not simply by injecting it into the vagina, and allowing it to come away as it may, but by a thorough washing of the whole vagina and os uteri for several minutes at a time, by means of an enema syringe, fitted with a female end, so as to clear away every particle of discharge that may be lying in the vagina, or around the os and cervix uteri. This will have the effect of allowing granulations or excoriations to heal, instead of being irritated by the uterine or vaginal secretions; it will cause the relaxed vagina to contract, and afford a better support to the uterus; and altogether contribute very much indeed to the comfort of the patient. In obstinate cases, after these washings, a syringe full of astringent lotion—alum, lead, zinc, or nitrate of silver—is injected, and allowed to remain as long as convenient; and it is only when these simple, but generally effectual, means fail, that we resort to the speculum, and direct local applications to the os uteri.

There are many very sensible observations, in the work we are considering, upon the local treatment of leucorrhœal diseases, and particularly with respect to the use of caustics. For these, we must refer our readers to the book itself, having already carried our analytical remarks to a greater length than we intended. In conclusion, we wish to congratulate Dr. Tyler Smith on his praiseworthy efforts to elucidate the subject he has selected; and to express to our readers the belief, that a careful perusal of his book will repay them for their trouble, by enlightening them on some interesting anatomical and physiological questions, by confirming them, probably, in some judicious, though hitherto undecided views of treatment, and by proving to them the value, especially in a conjectural science like ours, of patient investigation, inductive reasoning, and conclusions adopted, not hastily, but after due consideration, and with matured knowledge.

E. Copeman.

REVIEW VII.

1. *Beiträge zur Kenntniss der Urinabsonderung bei Gesunden. Inaugural Abhandlung.* Von AUG. WINTER.—*Giessen*, 1852.

Contributions to our Knowledge of the Urine in Health. By A. WINTER.

2. *Beiträge zur Kenntniss der Urinabsonderung bei gesunden, schwangern und kranken Personen. Inaugural Abhandlung.* Von FRIEDR. MOSLER.—*Giessen*, 1853.

Contributions to our Knowledge of the Urine in Health, Pregnancy, and Disease. By. F. MOSLER.

3. *Ueber Ausscheidung der Chlor-Verbindungen durch den Harn. Inaugural Abhandlung.* Von A. HEGAR.—*Giessen*, 1852.

On the Excretion of Chlorides by the Urine. By A. HEGAR.

4. *Die Ausscheidung der Schwefelsäure durch den Harn. Inaugural Abhandlung.* Von G. GRUNER.—*Giessen*, 1852.

The Excretion of Sulphuric Acid by the Urine. By G. GRUNER.

5. *Klinische Untersuchungen über den Stoffwechsel, bei gesunden und kranken Menschen überhaupt, und den durch den Urin insbesondere.* Von Prof. J. VOGEL. (Arch. d. Vereins f. gemeinschaftl. Arbeiten. Band i. Heft 1.)—*Göttingen*, 1853.

Clinical Investigations regarding the Metamorphosis of Tissue in Healthy and Diseased Persons generally, and especially on that which is effected through the Urinary Secretions. By J. VOGEL.

6. *Studien zur Urologie.* Von Dr. F. FR. BENEKE. (Archiv des Vereins für gemeinschaftliche Arbeiten. Band i. Hefte 3 and 4.)—*Göttingen*, 1853.

Urological Studies. By Dr. BENEKE.

ALTHOUGH little more than two years have elapsed since we availed ourselves of the opportunity afforded by the publication of Verdeil and Robin's 'Traité de Chimie Anatomique et Physiologique,' to notice at some length the chemistry of the urine,* we find that treatises, inaugural theses, and memoirs, either on the subject generally or on some of its various subdivisions, are accumulating so abundantly around us, that it seems expedient, without further delay, to place before our readers the results of these recent investigations; and this we propose doing in the form of a supplement to the review, to which we have already alluded.

We shall commence with the consideration of the *physical characters* of the urine:—its *quantity*, *specific gravity*, and *colour;* we shall then examine its *chemical constituents;* and shall conclude with a few remarks on the *physiological conditions* influencing this secretion.†

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* See vol. xi. pp. 358—378 of this Review.

† The decimal system presents so many advantages to the chemist, that we shall, as a general rule, retain the nomenclature of *grammes*, *cubic centimetres*, &c., employed in the memoirs which we now propose to analyze; we shall, however, when necessary, add the corresponding English weights and measures. The gramme is nearly 15·5 grains; the kilogramme nearly 2·2 pounds; and 30 c. c. are nearly equal to 1 ounce.

The *quantity* of the urine has been especially investigated by Vogel (in whose memoir are included the results obtained by his former students, Winter, Hegar, and Gruner), by Beneke, and by Mosler. The conclusions at which Vogel arrives, in reference to this point, may be summed up in a few sentences. (His observations are entirely restricted to healthy young men.) If we observe, for any length of time, the quantity of urine secreted by any individual, we shall find that the hourly and daily amounts will vary extraordinarily at different times. The *daily* urine of a man generally ranges from 1000 to 3000 c. c.; it may, however, fall short of, or exceed, these limits; thus, for example, the following fluctuations were observed in three men:

In the first, in 29 days, the minimum was 1080, and the maximum, 3340 c. c.
" second " 20 " " 1520, " " 3090 "
" third " 6 " " 910, " " 2920 "

The *hourly* quantity varies between 20 and 200 c. c.; but after copious drinking, 600 or even 800 c. c. may be excreted in an hour.

The causes of these variations may be internal (that is to say, may depend upon the organism itself) or external; transitory or persistent; simple or complicated. The most marked augmenting influences are those of abundant drinking, and the administration of diuretics; whilst amongst the leading diminishing influences, we must place abundant sweating, purging and vomiting, and the deprivation of fluids.

The *mean daily* urine of a healthy young man amounts to 1635 c. c., or about 54 ounces. This was the *general* mean of the mean numbers in nine cases, and is considerably higher than that given by most other observers, as Becquerel, Lehmann, &c. Beneke, whose memoir has been published subsequently to Vogel's, gives us a mean of twenty-three days' urine in the months of September and October, the number 1250 c. c. (the observations being made upon himself during his residence in London). Scherer, in one of his admirable 'Reports on the Progress of Physiological Chemistry,' suggests that beer-drinking may account for the high numbers obtained by Vogel in his observations on German students.

The *mean hourly* quantity, in the above cases, was 68 c. c.; for each kilogramme (about 2·2lbs.) of bodily weight there being 1 c. c. of urine excreted hourly, and for each 100 centimetres of height, 40 c. c. of urine.

With regard to the period of the day, it appears that the horary excretion is least during the night (the mean being 58 c. c.), that it rises in the morning (the mean being 69 c. c.), and that it reaches its highest point in the afternoon or evening (about 77 c. c.).

A considerable amount of interesting matter 'On the Quantity of the Urine, and the Causes by which it is Affected,' may be found in Beneke's memoirs, quoted at the head of this article, pp. 417—425, and 573—594.

We must now touch very briefly upon the quantity of urine in *diseases.* The quantity of the urine seldom *exceeds,* for a permanence, the normal mean to any great extent; diabetes mellitus, and the so-called diabetes insipidus, being almost the only cases in which this occurs.

In a very great majority of the cases of disease, the quantity is *diminished.* The following is Vogel's classification of these cases in a condensed form:

I. There may be a *moderate* diminution, occasioned by the scanty hospital diet which is commonly prescribed, by the removal of various external influences which augment the metamorphosis of tissue and the urinary secretion in healthy persons, and possibly also by a slight impeding action of the disease on the metamorphosis of tissue. The mean daily amount in these cases varies between 800 and 1500 c. c. To this class belong most chronic disorders, and probably many sub-acute diseases, if accompanied with little or no fever.

II. The diminution may be *more considerable*, the daily quantity falling for a longer or shorter period below 800 c. c. This occurs during the height of almost all febrile diseases, at the period when chronic diseases approximate to their fatal termination, in the more advanced stages of dropsy, and when very copious watery evacuations of some other kind occur during various diseases. Copious illustrations of these cases are given in Vogel's memoir.

The *specific gravity* of the urine is a subject to which both Vogel and Beneke have devoted considerable attention. Vogel observes, that if we would determine it with accuracy, we should employ two areometers, one marking from 1000 to 1018, and another from 1015 to 1035, and so graduated that we can read off to a quarter of a degree; and repeats the caution, that the experiment should be made on urine at a mean temperature.

The conclusions to be drawn from the specific gravity regarding the solid constituents of the urine are, of course, only approximate. Vogel, however, believes that Trapp's formula* gives results in which the error cannot exceed one-tenth in healthy, and one-fifth in morbid urine. Trapp's formula may be thus given:—If Δ represents the excess of the specific gravity of urine above that of water ($= 1000$), the amount of the solid constituents in 1000 parts of that fluid will be represented by 2Δ. Since in a state of health the urea is the main ingredient of the solid constituents (amounting on an average to at least as much as all the other constituents), we may thus form an approximate estimate of the quantity of this substance that may be present, as we shall show in a future page. According to Vogel, a healthy man may, by very copious water-drinking, reduce the specific gravity of his urine to 1000·5; and conversely, by abstaining from fluids, and by taking such violent exercise as to induce free perspiration, he may raise it to a very high number, 1033, or even more. The general mean specific gravity of the urine in the cases already referred to was $1020\frac{2}{9}$, and the mean quantity of solid constituents in 1000 parts (according to Trapp's formula) $40\frac{5}{9}$; hence, in twenty-four hours the average quantity of water is 1600 grammes, and that of solid constituents, 67 grammes. With regard to the influence of the bodily weight and height, it appears:

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* The writer of this review pointed out many years ago that no formula of this nature could give trustworthy results, even with healthy urine. His memoir, 'On the Specific Gravity of the Urine in Health and Disease, especially in Diabetes and Granular Degeneration of the Kidneys,' was published in the 'Lancet' for 1844, vol. i. p. 369. In a subsequent number of his 'Archiv,' Vogel contends strongly for the general accuracy of Trapp's formula, except in cases of diabetes, in opposition to an attack made upon it by Häser, who recommends a formula *of his own*, which consists in substituting $2\frac{1}{3}$ for 2, in Trapp's formula. A reference to the above-mentioned article in the 'Lancet,' will show that Prof. Häser's formula is not quite original.

(1st) That a man, for every kilogramme's weight, excretes on an average in one hour 103 grammes of urine, in which are 99 of water, and 4·1 of solid constituents; and (2nd) that a man, for every centimetre, excretes 39 grammes of urine, in which are 37·5 of water and 1·5 of solid constituents.

The amount of the excretion and its specific gravity at different periods of the day were noticed by Vogel. (The morning extends from 6 A.M. to 12 noon; the afternoon to 9 or 10 o'clock; and the night includes the remaining hours.) The following are his results:

Night: specific gravity, 1020·5; water, 58 grammes; solid constituents, 2·5 grammes, hourly.

Morning: specific gravity, 1088·0; water, 67 grammes; solid constituents, 2·5 grammes, hourly.

Afternoon: specific gravity, 1021·0; water, 78 grammes; solid constituents, 3·2 grammes, hourly.

From these numbers it appears that dinner (the principal meal) exerts a decided influence on the discharge of the solid constituents. The abundant ingestion of fluids exerts an unquestionable influence over the excretion of water by the kidneys, but so far from promoting, seems to impede, the discharge of solid constituents. We have not space to record the experiments by which Vogel was led to this conclusion; we must content ourselves with observing, that at his suggestion twelve medical students nobly devoted themselves one afternoon to copious beer-drinking in the cause of science, and that the mean hourly quantity of solid constituents which they discharged was very far below the average. This result is quite in accordance with the experiments of Bidder and Schmidt,* who found that an excessive circulation of water diminishes the metamorphosis of tissue; the question is, however, as we shall presently see, still undecided.

The following are the most important conclusions at which Vogel has arrived with regard to the specific gravity of the urine in disease.

In most *chronic diseases* the specific gravity of the urine exhibits the same irregular variations as in health. Upon the whole, the specific gravity is somewhat below the normal standard; and, at the same time, the daily amount of solid constituents is diminished: hence it follows that the amount of water is less affected than that of solids.

Exceptions to this rule are presented by two classes of chronic diseases, in which the urinary secretion is very much increased.

1. There may be a very abundant urine, containing a large amount of solid constituents, and accompanied by impaired nutrition, emaciation, &c., as diabetes mellitus and diabetes insipidus.

2. Or there may be a very abundant urine with a low specific gravity, and a comparatively small amount of solid constituents. Here it is only the water that is carried away in excess from the body; and hence there is no emaciation or hectic, and the process often seems to be even a salu-

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* Die Verdauungssäfte und der Stoffwechsel, p. 343.

tary effort of nature, tending to the removal of dropsical effusions. To this form Vogel gives the name of *Hydruria*, and he appends the particulars of cases, showing how completely hydruria differs from diabetes insipidus.

In *acute febrile diseases*, the specific gravity rises in the same proportion as the quantity of urine diminishes; and the amount of solid constituents, although less than in health, is not less than in chronic diseases; the quantity of water is, however, considerably diminished. Vogel adds, that as such patients usually live almost exclusively on fluids, they are in much the same position as persons in a state of inanition, the excretion of the solid constituents taking place at the expense of their tissues, which are not replaced by food.

His observations on the urine in *dropsies* are so important, that we translate them without abridgment.

> "It is well known that during their duration the urine is scanty, and that when the quantity of that excretion considerably increases, the dropsy diminishes or disappears. In the increased diuresis of dropsical patients, the urine presents a very low specific gravity, and contains a very large quantity of water, with a comparatively small amount of solid constituents. Here, then, we have *hydruria;* and the following observations will show how it may be favourable to the removal of dropsy.
>
> "The blood and the various fluids which permeate the parenchyma of the organs are perpetually reacting on each other by endosmosis; the thinner and less concentrated fluid has a tendency to pass to the more concentrated one, through the membranes separating them, and to be absorbed by it. If a dropsical effusion be present, it is resorbed by passing by endosmosis into the blood—that is to say, if we put out of consideration the action of the lymphatics. *Cæteris paribus*, the facility with which this occurs is proportional to the concentration of the blood. A hydræmic crasis—that is to say, a very thin, watery condition of the blood, independently of mechanical impediments in the venous circulation, is not merely the cause of most dropsies, but is also the most important impediment to their cure. The hydræmia is, however, relieved by hydruria, the blood thus becoming more concentrated, and the dropsy being thus cured. Hence we further see why a dropsy is the more readily removed by simple diuresis, the more it depends on a mere hydræmia; and why dropsies which are not caused by hydræmia, but are dependent on a mechanical disturbance of the venous circulation, cannot be cured by diuretics and other similarly acting agents.
>
> "In the urine of dropsical persons with hydruria, there is another circumstance to be considered, which influences the specific gravity and the amount of solid constituents, and which I shall now briefly notice.
>
> "Schmidt[*] has discovered the important fact, that in the cases where the amount of albumen in the blood is diminished, a certain quantity of chloride of sodium is taken up to replace the missing albumen. I have found that this excess of salt in the blood is again removed by the kidneys with the hydruria which relieves the hydræmia; the urine then contains a very large amount of chlorides, which renders its specific gravity, as well as the quantity of its solid constituents, much higher than we should otherwise have been led to expect." (pp. 135, 136.)

We shall return to this subject in our remarks on the excretion of the chlorides. If any of our readers wish for a fuller consideration of the value of the specific gravity of the urine, we must refer them to Beneke's

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[*] Charakteristik der epidemischen Cholera. 1850.

'Urological Studies' (quoted at the head of this article), pp. 425—436, and pp. 594—600.

The conclusion of Vogel's elaborate memoir is devoted to the *colour* of the urine. To establish a definite terminology, he lays down the following scale of colours. From long clinical experience, he believes that all shades of colour presented by the urine may be reduced to nine, which may be arranged in the following groups:

> "The *first group* embraces the yellowish urines, which may be
>
> 1. Pale yellow (like gamboge dissolved in much water).
> 2. Bright yellow (like gamboge dissolved in less water); or
> 3. Yellow (like gamboge dissolved in very little water).
>
> "The *second group* embraces the reddish urines, which may be
>
> 4. Reddish-yellow (like gamboge with a little carmine).
> 5. Yellowish-red (like gamboge with more carmine); or
> 6. Red (like carmine with a little gamboge).
>
> "The *third group* embraces the brown (dark) urines.
>
> "The red colour passes through the brown into almost a black colour. This group may be imitated by gamboge, carmine, and prussian blue, in various proportions. The individual tints are—
>
> 7. Brownish-red—a little brown mixed with the red.
> 8. Reddish-brown—more brown than the preceding.
> 9. Brownish-black—almost black, with, however, a tinge of brownish-red."
>
> (pp. 138, 139.)

We have not space for his description of the manner in which he estimates the amount of urine-pigment by the depth of the colour, and must content ourselves with his principal conclusions.

In healthy persons, the colour usually ranges from reddish-yellow to bright yellow; pale yellow, or even a still paler urine, only occurring after abundant drinking. In persons labouring under disease, the urine presents many variations of colour.

Very pale urine (from a pale yellow to a colourless fluid) may of course occur, after copious drinking, in cases of disease, just as in cases of health; it also occurs in hysterical and nervous affections, in many cases of anæmia, and during convalescence after severe diseases. In almost all febrile diseases the urine is yellowish-red, or even red, and contains, not only relatively, but absolutely, more pigment than healthy urine. During convalescence the quantity of pigment diminishes, and frequently falls below the normal standard. In cases of pneumonia, pleurisy, and gastric, rheumatic, or hectic fever, we can generally tell by the mere colour of the urine whether or not the fever has abated.

Brown and brownish-black urines are comparatively rare. These colours are observed in many cases of typhus, putrid fever, Bright's disease, and scurvy—in short, wherever there is an excessive disintegration of the blood-corpuscles. It is worthy of notice, that the darkest (most nearly black) urine which Vogel ever saw, was passed by a man who had inhaled arseniuretted hydrogen.

In concluding this part of our subject, we may observe, that Vogel holds the opinion (and advances several arguments in favour of it), that the urine-pigment is formed from the hæmatin, and is produced by a decomposition of the blood-corpuscles; and hence, according to him, the amount of pigment in the urine affords a very ready and simple means of determining the rate of disintegration of the corpuscles in a given time.

We now proceed to the consideration of the individual constituents which are held in solution in the urine, and commence with the most important of them—the *urea.*

Most of our readers are doubtless aware that the *origin* of urea is at present a *quæstio vexata* amongst chemists and physiologists; one party —including the names of Liebig, Bischoff,[*] and others—holding that the urea is solely a product of the metamorphosis of the nitrogenous tissues; while the other party—which ranks amongst its supporters Lehmann, Frerichs, and (more especially) Bidder and Schmidt[†]—maintain that the formation of the urea is dependent upon two factors, one of which is *variable*—namely, the amount of assimilated histo-plastic or albuminous food, while the other is *constant*—namely, the necessary consumption of the albuminous tissue when the animal is fasting.

As the works of the leading antagonists—namely, Bidder and Schmidt on the one hand, and Bischoff on the other—have been already reviewed in this journal,[‡] we will content ourselves with observing that Lehmann, who must have been fully acquainted with Bischoff's treatise when he published his 'Handbuch' a year subsequently, has in no way modified the views which he had previously expressed; for he concludes his observations on the formation of urea with the following sentence:

> "Finally, it is not at all probable that the augmentation of urea in the urine, after the abundant use of gelatigenous substances, should occur so rapidly, if the nitrogenous materials in the blood were not directly consumed, and their nitrogen united with certain other elements to form urea. Its formation must, therefore, be regarded as occurring mainly in the blood, and, as it would appear from the phenomenon which has just been mentioned, its source must be sought, not merely in the consumption of the nitrogenous tissues, but also, in part, in the food that has been taken." (p. 48.)

As Liebig's *method of determining the quantity* of urea (the principles of which were described in the Chemical Report in the last number of this Review) will, probably, be generally adopted for the future, it may be expedient to say a word or two regarding its claims to accuracy. Liebig himself found the differences in the amount of urea obtained by his own method and that of Ragsky very trifling; and he adds that—"The numerous experiments made by professors Dr. Vogel and Dr. Bischoff, at whose instigation this method has been devised, leave no doubt as to its applicability and correctness." Heintz[§] regards it as equal in accuracy to any previous method (including, of course, his own). Kletzinsky[||] seems, however, to have put the method to the most severe test. In his memoir, 'On the Comparison of the Values of the Different Methods of Determining the Quantity of Urea,' he gives the results of three methods—namely, Liebig's, Ragsky's, and that by extraction with alcohol, as applied to five specimens of his own urine. We give his conclusions in the briefest possible terms:

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[*] Der Harnstoff als Maass des Stoffwechsels. 1853.

[†] Die Verdauungssäfte und der Stoffwechsel, p. 386. 1852.

[‡] See vol. xiii. pp. 384—405.

[§] Lehrbuch der Zoochemie, p. 1058.

[||] Heller's Archiv für Chem. und Mikrosk., p. 252. Jahrgang, 1853.

The number of milligrammes of urea in 10 c. c. of urine were—

	By Liebig's method.	By Ragsky's method.	By extraction with alcohol.
1	610	596	603
2	585	575	581
3	605	591	600
4	570	562	564
5	595	587	589

The same urines were then treated with a solution of sugar of lead, to which a few drops of acetic acid had been added, and after the removal of any excess of lead by sulphuretted hydrogen, they were submitted to the previous processes for the urea, with the following results:

1	590	589	594
2	570	571	572
3	590	585	592
4	560	561	561
5	585	586	584

It follows from these experiments, that, by first precipitating with the salt of lead, we eliminate one or more nitrogenous matters which act as sources of error in the determination of the urea in all three methods. These errors being removed, it is most satisfactory to observe the very close accordance between the first and second columns. Kletzinsky shows that, while we should never omit this correction, even for normal urine, Liebig's method, adopted without this precaution, gives thoroughly deceptive results for many kinds of morbid urine.

The *normal quantity* of urea secreted in twenty-four hours has next to be considered—a subject which has been recently taken up by several chemists. We may especially notice the investigations of Scherer, Rummel, and Bischoff, on this point. Scherer[*] analysed the urine of a girl (A), aged 3½ years, weighing twenty-nine pounds;[†] of a boy (B), aged 7 years, weighing forty pounds; of a man (C), aged 22 years, weighing 112 pounds; and of a man (D), aged 38 years, weighing 125 pounds; and Rummel,[‡] who has carried on a similar series of observations, has examined the urine of a boy (*a*), aged 3 years, weighing 24¼ pounds; of a boy (*b*), aged 4 years, and weighing 25¾ pounds; of a girl (*c*) aged 5 years, and weighing 30 pounds; of a youth (*d*), aged 18 years, and weighing 105 pounds; of a man (*e*), aged 31 years, and weighing 136 pounds; and of an old man (*f*.), aged 65 years, and weighing 104 pounds. We arrange their results in a single table, classifying the cases according to age. To save repetition in a future part of the article, we give the amount of water, inorganic salts, &c.

Bischoff's observations are given separately in a subsequent table in p. 80.

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[*] Verhandl. der Phys.-Med. Gesellsch. zu Würzburg, vol. iii. p. 180. 1852.

[†] The Bavarian pound here employed is nearly 1·23 lb. avoirdupois; its actual value being 0·5604 of a kilogramme.

[‡] Verhandl. der Phys.-Med. Gesellsch. zu Würzburg, vol. v. p. 116.

	a. 3 years.	A. 3½ years.	*b.* 4 years.	*c.* 5 years.	B. 7 years.	*d.* 18 years.	C. 22 years.	*e.* 31 years.	D. 38 years.	*f.* 65 years.
1000 parts contained—										
Water	966·39	965·39	955·89	941·23	969·91	967·37	965·32	967·95	959·53	977·39
Solid constituents	33·61	34·61	44·11	58·77	30·09	32·63	34·68	32·05	40·47	22·61
Urea	15·34	17·19	20·25	26·11	16·98	14·61	12·52	16·44	16·93	7·82
Other organic matters	3·57	2·87	5·51	9·90	3·61	4·60	11·21	6·41	11·66	9·54
Fixed salts	14·70	14·55	18·35	22·76	9·50	13·42	10·95	9·20	11·88	5·25
In twenty-four hours there were discharged, in grammes—										
Water	874·26	728·87	758·04	680·98	1044·60	2472·43	2081·430	2343·41	1689·773	2396·63
Solid constituents	29·74	26·13	33·96	41·02	32·40	81·57	74·970	76·59	71·227	54·37
Urea	13·57	12·98	15·59	18·22	18·29	36·52	27·008	39·28	29·824	19·17
Other organic matters	3·17	2·17	4·25	6·92	3·88	11·50	24·335	15·33	20·484	12·31
Fixed salts	13·00	10·98	14·12	15·88	10·23	33·55	23·627	21·98	20·919	12·89
For every pound weight of the body there were excreted in twenty-four hours, in grammes—										
Water	36·050	25·133	29·430	22·660	26·115	23·540	18·584	17·010	13·518	23·140
Solid constituents	1·226	0·901	1·318	1·367	0·81	0·776	0·669	0·563	0·569	0·523
Urea	0·526	0·447	0·605	0·607	0·457	0·347	0·241	0·288	0·238	0·184
Other organic matters	0·130	0·075	0·165	0·230	0·097	0·109	0·217	0·112	0·164	0·214
Fixed salts	0·536	0·378	0·548	0·529	0·256	0·319	0·211	0·161	0·167	0·124

[illegible] and taking [illegible] C. and e, [illegible] we may conclude that a man [illegible] grains of urea in twenty-four hours.

[illegible]* observations on the quantity of urea are deserving of special notice. [illegible] a series of comparative [illegible] aged [illegible] years and weighing [illegible] pounds, whose [illegible] of March (A and B), aged [illegible] years and weighing [illegible] pounds, whose urine he analysed [illegible] aged [illegible] years and weighing [illegible] pounds, whose urine was analysed in the same days [illegible] man [illegible] aged [illegible] years, weighing rather [illegible] aged 45 years, [illegible]. In the two [illegible] the urine was daily analysed from the [illegible] of March to the [illegible] of April.

	A.	B.	C.	D.	E.
Specific gravity	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	1662·70
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	37·70
Percentage of urea in urine	[illegible]	[illegible]	[illegible]	[illegible]	2·07
[illegible] of urea for [illegible] pound of [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	0·175
[illegible] in 24 hours	[illegible]	[illegible]	[illegible]	9·13	14·50

Before [illegible] the various [illegible] influencing the amount of urea, we may [illegible] to the view held by M[illegible]† that the excess of the specific gravity over 1.000 corresponds very nearly to the [illegible] in 1000 parts of urine: the above table of [illegible] analyses seems to support M[illegible]'s opinion; and certain experiments by [illegible] quoted by [illegible]‡ on the action of senega and colchicum, likewise corroborate it. The observations of Chantert and Lehmann, on the other hand, seem to indicate that even in healthy urine there is no necessary connexion between these numbers.

We showed in a previous page (see p. 74) that certain experiments recorded by Vogel confirmed the view held by Schmidt, "that a free use of water diminishes the metamorphosis of tissue," and consequently the amount of urea. Bischoff's observations, however, made partly upon himself and partly upon dogs, led him to the conclusion that abundant water drinking decidedly augments the excretion of urea. Till we more clearly understand the action which water exerts on the metamorphosis of tissue (and notwithstanding the admirable researches of Schmidt and Bischoff, much still remains to be cleared up on this subject), we cannot regard the views of either party as established; we are, however, inclined to believe that Bischoff's view is the nearer to the truth.

According to Bischoff, the quantity and quality of the *food*, and especially its amount of nitrogen, exert a far greater influence on the formation and decomposition of the nitrogenous constituents of the body, and, consequently, on the amount of urea excreted in a given time, than had previously been supposed; the limit to the increase being apparently the power of the individual to dissolve and digest nitrogenous food: thus, for instance, one of the dogs on which he experimented, when taking 4000 grammes of beef (freed from fat and bone), discharged 190 grammes of urea daily, and when living on 500 grammes of potatoes and 250 grammes of fat, excreted not more than from 6 to 8 grammes.

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* Der Harnstoff als Maass des Stoffwechsel.

† Comptes Rendus, vol xxvi. p. 120.

‡ Archiv des Vereins für gemeinsch. Arbeiten, vol. i. p. 426, note.

These results are, to a certain degree, confirmed by the experiments of Siegmund[*] on rabbits. He found (1) that the daily quantity of urea gradually diminishes with the diminution of the food (which in this case consisted of cabbage); (2) that the per-centage of urea in the urine increased with the diminution of the food; and (3) that the amount of urea in relation to one gramme of food varies inversely with the quantity of food.

There are two additional points established by Bischoff's experiments which should be noticed here; namely (1), that the use of fat materially retards the disintegration of the nitrogenous constituents of the body, and therefore diminishes the formation and excretion of urea; and (2) that the use of gelatine as food increases the amount of urea to a great extent. Bischoff, however, believes that this additional urea is merely a product of the decomposition of the gelatine in the blood, and does not suppose that the gelatine had contributed to form any tissue of the body.

The influence of "accessory foods" on the urinary secretion, and especially on the excretion of urea, has been fully considered by Dr. T. K. Chambers in the fourteenth volume of this Review. We need only remark that, according to the experiments of Böcker, *alcohol*[†] and *tea*[‡] (especially the latter) diminish the daily excretion of urea; while Dr. Julius Lehmann[§] has shown that *coffee* (and especially the empyreumatic aromatic substance contained in it) has a similar action. According to Böcker,[‖] *beer* neither increases nor diminishes the quantity of urea. (In justice to Heller we should observe, that while the memoirs of Böcker and Lehmann on tea and coffee were published in 1853, the following passage occurs in his memoir "On the Urea" published in the first number of his 'Archiv' for 1852: "It is now a long time since I remarked that after the moderately free use of coffee, tea, and alcoholic drinks, the quantity of urea in the urine is diminished.")

The influence of *common salt* in augmenting the excretion of urea was distinctly shown by Bischoff's experiments. A dog was fed for one week on a pound of beef daily, during which time its weight remained constant; it was then fed for twelve days on the same food, with the addition of a daily allowance of 50 grammes of a saturated solution of salt. During the first period the daily amount of urea was 23·50, and during the second period 28·34 grammes.

It has been long known that strong bodily *exercise* increases the quantity of urea.[¶] Dr. Hammond[**] has recently published in the 'American

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[*] Ann. der Chem. und Pharm., vol. lxxx. p. 112.

[†] See vol. xiv. p. 398, of this Review.

[‡] Archiv des Vereins für gemeinsch. Arbeiten, vol. i. p. 213.

[§] Ann. der Chem. und Pharm., vol. lxxxvii. pp. 205, 275.

[‖] Archiv des Vereins für gemeinsch. Arbeiten, vol. i. p. 544.

[¶] See Lehmann's Physiological Chemistry, vol. i. p. 168.

[**] Quoted in Monthly Journal of Medical Science. March, 1855.

Journal of Medical Science' some excellent experiments on this subject instituted on himself. The following table exhibits the results of three days' analyses. His food and drink were the same in all cases. The first day he took his ordinary exercise; on the second day he "walked briskly eight and a half miles over a hilly country, rode ten miles on horseback, and pitched quoits for two and a half hours;" on the third day, after rising from bed, he "immediately laid down on a sofa, and remained there continuously the ensuing twenty-four hours, with the exception of rising four times to urinate."

	Quantity of urine.		Specific gravity.	Quantity of urea.	Quantity of uric acid.
	oz.	dr.		grains.	grains.
Moderate exercise . . .	31	2	1021	682·1	13·7
Increased exercise . . .	34	1	1024	865·0	8·2
No exercise	24	7	1018	457·0	24·9

The table printed on p. 79 distinctly shows that for a definite unit of bodily weight nearly twice as much urea is excreted in twenty-four hours by a child as by an adult, and by an adult as by an aged person.

Little is definitely known regarding the power of *remedial agents* in modifying the amount of urea. Millon and Laveran[*] believed that after the use of small doses of tartrate of potash and soda they found a diminution of the uric acid and an augmentation of the urea. Böcker,[†] in a somewhat dreary article of sixty pages, "On the Physiological Action of Phosphoric Acid and Phosphate of Soda," arrives at the conclusions (1) that phosphoric acid in a state of great concentration (100 drops to 250 grammes of water) augments the quantity of urea, but that when much diluted it neither increases nor diminishes it; and (2) that phosphate of soda always diminishes the amount of urea. Siegmund, after making the observations on rabbits to which we have already referred, proceeded to notice the effect of diuretics upon them in relation to the excretion of urea. A rabbit which, when indulging freely on cabbage, discharged 1·599 grammes of urea daily, took cubebs (in addition to its cabbage) for nine days (38 grammes having been administered in varying doses during this period); under the influence of this drug the average daily quantity of urea rose to 2·9 grammes. A repetition of the experiment, under which the animal sunk, yielded a similar result. A second rabbit, which on its ordinary diet discharged 3·248 grammes of urea, was dosed with the ethereal extract of cantharides, under which treatment the daily quantity of urea rose to 5·471 grammes. Both animals lost much weight during the experiments. Some experiments on the action of digitaline[‡] on the same animals (rabbits) gave no very definite results. The action of common salt has been already noticed. The only other medicinal agent which, so far as we yet know, decidedly increases the amount of urea, is potash. This is most decisively proved by the experiments of Dr. Parkes.[§] Whether liquor potassæ (which was the preparation employed) is more powerful than other alkaline medicines in hastening the metamorphosis of tissue, can only be told by further investigations, and it is to be hoped, for the cause of science, that Dr. Parkes will extend his experiments, which cannot fail to throw much light upon many points not only of therapeutics but of physiology.

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[*] Ann. de Chim. et de Phys., troisième série, vol. xii. p. 139.

[†] Prak. Vierteljahrschrift, vol. xxiv. pp. 117—176. 1854.

[‡] Arch. für path. Anat., vol. vi.

[§] See vol. xiv. pp. 498—506, of this Review.

The last point we shall notice in connexion with urea is the effect which *diseases* exert on its excretion. We shall probably have much more information on this point in a few years, now that Liebig's method has so much facilitated the determination of this constituent. The only memoirs on this subject requiring special mention are that of Heller,* "On the Augmentation and Diminution of the Urea in Diseases," and that of Dr. A. Vogel† (of Munich), entitled 'Urinary Investigations according to Liebig's New Method,' whose most important conclusions, drawn from no less than 182 analyses, have been already published in this 'Review.'‡ Heller does not give the particulars either of the cases or of his analyses. The following are the results at which he arrives:

1. Whenever the urea is increased, the brown urine-pigment, the sulphates, and the alkaline phosphates (and often the uric acid), are also increased.

2. The greatest augmentation of the urea was observed in meningitis. (In several cases the whole urine solidified, in a few minutes, into a crystalline magma, on the addition of concentrated nitric acid.) In pneumonia and pleuritis, in acute tuberculosis, and in rheumatism, especially if endocarditis be simultaneously present, the urine is very rich in urea during the stage of exudation, but diminishes during resorption. In the beginning of typhus there is an augmentation of urea, but not so great an increase as in the above-mentioned diseases. Heller has likewise met with cases of azoturia, but unfortunately gives no particulars regarding them.

3. Excluding those cases in which the urea diminishes from decomposition into carbonate of ammonia, there is a primary diminution of this constituent in most renal diseases, and in the chronic neuroses.

Dr. Parkes' Gulstonian Lectures "On Pyrexia," published during the spring of the present year in the 'Medical Times,' may also be consulted with advantage on this subject.

The organic acids of the urine, especially *uric acid*, stand next in physiological importance to the urea.

Uric acid, for the most part in combination with soda, is a normal constituent of human urine, in which it averages about $0{\cdot}1\frac{0}{0}$; the daily quantity excreted by an adult ranging, according to Lehmann, from 0·5 to 0·9 of a gramme, or from 8 to 14 grains.

There is much in connexion with the physiological relations, both of uric and *hippuric acid*, that is far from being so clear as could be wished. Heller,§ in a memoir "On Uric and Hippuric Acids," advances certain views which will probably be novel to many of our readers. After alluding to two opinions which have met with considerable support, namely, (a.) that diet exerts less influence on the amount of uric acid than on that of urea, and (β.) that the quantities of uric acid and urea are always in an inverse ratio to one another, he states that his own experiments and observations lead him to regard both as untenable.

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* Arch. für Chem. und Mikrosk., p. 15. 1852.
† See vol. xiv. p. 530.
‡ Zeitschrift für rat. Med., new series, vol. iv. p. 362.
§ Arch. für Chem. und Mikrosk., p. 20. 1852.

Regarding the first of these views, he holds that the influence of different kinds of food on the uric acid is very considerable, but that it is slow in exhibiting itself; if, however, the diet be sufficiently prolonged, the influence is obvious. In conjunction with a friend he performed the following experiments:—After determining their normal quantity of uric acid, Heller lived for a week on wheat- and rye-bread, and his friend solely on rye-bread (of which they took a sufficiency), and water was their only drink. The uric acid soon began to diminish, and to be replaced by hippuric acid, while the quantity of urea was not materially affected. At the end of the week there was a large quantity of hippuric, with a mere trace of uric acid, in Heller's urine; while in that of his friend the hippuric acid completely replaced the uric acid, not even a trace remaining. During the next week, when they were living on a mixed diet (including flesh), the process was reversed: the hippuric acid vanishing, and the uric acid returning more rapidly than it had disappeared. It thus appears (in opposition to the well-known experiments of Lehmann) that an exclusively vegetable diet causes the abundant formation of hippuric acid, and the simultaneous disappearance of the uric acid. Heller seems to regard these results as completely overthrowing the second as well as the first of the above opinions; we do not see very clearly, however, how he makes out his case, unless under the general term "uric acid" he includes both uric and hippuric acids. His view, that the uric and hippuric acids stand in an inverse relation to one another, is probably correct, being supported not only by the experiments we have just quoted, but by certain physiological facts—as, for instance, the presence of uric acid in the urine of the *herbivora* while sucking, and that of hippuric acid in its place when these animals resort to a vegetable diet.

We have no space for noticing Heller's remarks on the augmentation and diminution of the uric acid in the urine in diseased conditions of the system.

He does not regard the occurrence of hippuric acid in the urine as a diagnostic sign of any value; and believes that it may always be traced to the diet.

Formic acid appears to occur in small quantity in healthy urine.* *Valerianic* acid (in combination, probably, with ammonia) has been shown occasionally to exist in morbid urine.† *Butyric* acid is also sometimes present in minute quantity. Although *lactic* acid does not occur in healthy human urine, it very soon presents itself when the oxidation in the blood is to any extent impeded; hence it occurs (according to Lehmann) whenever fever is present, and in various disturbances of the digestive and respiratory organs.

Creatine and *creatinine* appear to be integral constituents of the urine, but we are not aware that any attempts have been made to determine them quantitatively, or to ascertain the circumstances influencing their augmentation or diminution in the urinary secretion.

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* Campbell: Chemical Gazette, vol. xi p. 312; vol. xii. p. 53.

† See Report on Physiological Chemistry, in vol. xv. p. 534, of this Review.

The substances occurring under the vague term *extractive matters* next claim our attention. Considering the small quantities in which uric and hippuric acids, the pigment, mucus, and the far smaller quantities in which the organic bases mentioned in our last paragraph occur in the urine, we may, at all events for the purpose of comparison, regard the extractive matters as equivalent to the "*other organic matters*" in the Table in p. 79. We have nothing to add in relation to the amount in which these extractive matters occur at different ages, beyond what may be at once seen from that Table, except to notice an error into which Lehmann has fallen, both in his larger work and in his 'Handbuch.'* In referring to Scherer's experiments (Rummel's were not then published), he misplaces the figures, and consequently arrives at a precisely opposite result to that of Scherer himself. There seems, from our table, to be not the slightest connexion between the age and the relative amount of extractive matters. In the urine of a madman who was starving himself to death, the extractive matters, &c., in twenty-four hours, amounted to 10·59, while the urea had sunk to 9·48 grammes.

In diseases the extractive matters are commonly increased in the urine, but accurate observations on this subject are still required.

We now proceed to the consideration of the *inorganic salts* of the urine—namely, the chlorides, sulphates, and phosphates—with which traces of iron, silica, and fluoride of calcium are frequently associated.

The excretion of the *chlorides* by the urine has been specially studied by Hegar (in the thesis whose title stands at the head of this article), and by Wundt,† in a memoir "On the Amount of Chloride of Sodium in the Urine." We shall confine our observations to the investigations of Hegar, which were carried on under the superintendence of Liebig and Vogel. The subjects of experiment were seven men, whose ages ranged from twenty to twenty-five, and one man of thirty-eight.

The mean quantity of chlorine in the urine of twenty-four hours was 10·46 grammes, the maximum being 13·92, and the minimum 7·38 grammes.

The morning urine of one hour contained on an average 0·48 of a gramme, and for every 100 kilogrammes of weight there were 0·73 of a gramme in one hour. The mid-day urine of one hour contained on an average 0·57 of a gramme, and for every 100 kilogrammes of weight there were 0·84 of a gramme in one hour. The urine during the night was the poorest in chlorine, there being only 0·28 of a gramme discharged in one hour, and for every 100 kilogrammes of weight only 0·39 of a gramme.

The following are the most important conclusions at which Hegar arrived:—1. The amount of chlorine excreted in twenty-four hours is very variable in different individuals, and depends partly on the food, but partly also on the manner of life, and on the constitution. 2. It stands in no definite relation to the weight or height of the individual. 3. It attains its maximum in the afternoon, although not immediately after dinner, falls to its minimum in the night, and rises again in the morning. 4. Bodily exercise tends to augment the excretion of chlorine, as also does copious water-drinking, but in this case the augmentation is succeeded by a decided diminution. 5. Indisposition appears to diminish the excretion very rapidly. 6. In the normal state the excretion of chlorine never appears to be perfectly suspended. When no chlorides are taken with the food and drink, the chlorine must be obtained from the blood or tissues. 7. If more than the ordinary quantity of chlorides be taken, the excretion very soon rises, but shortly afterwards falls to the normal level. Only a small portion of the excess of chlorides is removed by the urine, even when there is no augmentation of the intestinal discharges; the chlorine must, therefore, have been carried off by some other course. 8. In what connexion the excretion of chlorine stands to the excretion of urea and uric acid, and in what connexion it stands with the respiratory process by which the non-nitrogenous food is removed from the system, are unknown; but that such a connexion exists seems supported by the facts that chloride of sodium is decomposed in the body, and that it forms compounds with grape-sugar and with urea, and that there is an instinctive propensity universally exhibited by man to take salt with every kind of diet, and especially with amylaceous food.

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* Physiological Chemistry, published by the Cavendish Society, vol. iii. p. 536; and Handbuch der phys. Chem., p. 184.

† Journ. für prak. Chem., vol. lix. p. 354.

We must refer to the Table in p. 80 for Bischoff's determinations of the quantity of chloride of sodium passed in five cases in the urine of twenty-four hours. From these numbers the quantity of chlorine can readily be calculated.

In all diseases in which a copious exudation is separated from the blood, there seems to be a striking diminution of the chlorides. Heller seems to have been the first who noticed this fact, and it has since been confirmed (especially in cases of pneumonia) by Redtenbacher, Bennett, Beale, Parkes, and others. In acute rheumatism, capillary bronchitis, and typhus, as well as in pneumonia, urine is often passed which is so free from chlorides that the addition of nitrate of silver scarcely causes any turbidity. Dr. A. Vogel's conclusions regarding the variations of the chlorides in different diseases have been given in vol. xiv., p. 530 of this Review.

For our knowledge regarding the excretion of the *sulphates* by the urine, we are chiefly indebted to Gruner, whose researches are given in the thesis whose title stands at the head of this review, and to Dr. Parkes.* As the excellent memoirs of Dr. Parkes are accessible to all our readers, we shall confine our attention to the results obtained by Gruner from experiments on seven healthy men and seven patients.

1. From the mean observations on the seven healthy men, it appeared that a man weighing 60 kilogrammes, or rather more than 13 stone, discharged on an average 2·094 grammes of sulphuric acid in the urine of twenty-four hours.† Taking, for convenience, 100 kilogrammes as the standard of comparison, there are excreted for this weight of the body (which, however, man seldom attains to) 3·19 grammes of sulphuric acid, the extremes being 2·04 and 3·73 grammes. The constitution appears to exert a greater influence on the excretion of sulphuric acid than

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* British and Foreign Medico-Chirurgical Review, vol. xi. p. 258; vol. xiii. p. 248; and vol. xiv. p. 498.

† This is about 32 grains. Parkes regards 24 grains as the average; while Becquerel puts the number as low as about 17 grains.

external conditions. 2. There is no definite ratio between the excretion of sulphuric acid and either the weight or height of the individual, or the specific gravity of the urine. 3. Food influences the variations which occur in the amount of the sulphuric acid excreted at different periods of the day. The excretion always rises in the afternoon (when digestion is going on), sinks a little during the night, and attains its minimum in the forenoon; the ordinary exercise commonly taken in the forenoon having apparently no influence in augmenting the excretion of sulphuric acid. 4. But extraordinary muscular exertion and mental excitement appear to augment the excretion of this acid. 5. Fasting does not diminish the excretion of sulphuric acid, at all events during the first twenty-four hours. 6. Whether the excretion can, normally, ever be entirely suspended, is not shown by these experiments; it may, however, sink to a barely recognisable minimum. 7. During copious water-drinking there is for the first few hours, probably, an augmentation, which is soon followed by a diminution, of the sulphuric acid. 8. An augmented ingestion of sulphuric acid, in the form of sulphate of soda, causes an increased excretion. Whether the sulphuric acid passes as such directly into the urine, or whether it first takes a share in the metamorphosis of tissue, is uncertain; probably the latter, since the augmentation does not appear for some hours after the salt has been taken. Between the eighteenth and the twenty-fourth hour almost the whole quantity which was taken is again excreted. 9. It may be assumed with certainty that the sulphuric acid which appears in the urine is a result of the metamorphosis of the proteine bodies (which contain on an average 1% of sulphur), and that at all events a considerable part of the sulphur which is burned in the system, reappears in the urine as sulphuric acid.

In justice to a distinguished British chemist, Dr. Bence Jones,* we should remark, that he has in several points anticipated the observations of Gruner, who, however, duly recognises his claims.

Dr. Parkes, in the memoirs to which we have already alluded, has shown, that when liquor potassæ is taken into an empty stomach, it passes unneutralized into the circulation, and in from thirty to ninety minutes, an increased flow of slightly-acid urine occurs, which contains the whole of the potash, and, amongst other things, a relatively large proportion of sulphuric acid. The liquor potassæ exerts an oxidizing effect on all albuminous compounds, either in the blood or in the tissues; and its sulphur, under the form of sulphuric acid, unites with the potash, and is given off by the kidneys.

Various observations have been made on the influence which different morbid processes exert on the excretion of sulphates. Heller some years ago maintained that in acute diseases, especially of the respiratory organs, the sulphates were considerably increased; but observations made by Lehmann upon the urine of two patients with pneumonia, and one with pleurisy, and by Gruner in a case of pleuro-pneumonia, are far from confirming Heller's view. According to Parkes, the sulphuric acid is greatly augmented in rheumatic fever during the existence of the severe symptoms; while, on the other hand, in a case of acute rheumatism recorded by Gruner (page 25 of his 'Thesis'), the reverse was the case. Moreover, it appears from Gruner's observations, that chronic rheumatic pains decidedly diminish the sulphates. Heller further maintained that they were diminished in chlorosis, and the neuroses; but both Lehmann and Gruner arrive at opposite conclusions to him in relation to chlorosis; and Dr. Bence Jones* has shown that they are actually increased in acute chorea and in delirium tremens.

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* On Animal Chemistry in its Application to Stomach and Renal Diseases, pp. 71—76. 1850.

The quantity of *phosphoric acid* and *phosphates* in the urine has been independently investigated by several chemists, amongst whom we may especially notice Breed,† Winter, and Mosler. The phosphoric acid exists partly as *acid phosphate of soda*, and partly as *earthy phosphates*. As far as we are aware, Breed was the first who determined the amount of phosphoric acid daily excreted under different physiological conditions. The following are his most important conclusions. From 24 analyses of the urine of four persons, he found that the mean daily quantity of urine was 1610 c. c., while that of phosphoric acid was 3·732 grammes. When a person drank an excess of three pints of water daily, the mean quantities of urine and phosphoric acid were 2086 c. c. and 4·228 grammes; when, on the other hand, he drank but half his usual amount of fluid, the quantities were 988 c. c. and 4·015 grammes. Breed likewise ascertained that the urine secreted during the waking hours contained (both absolutely and relatively) more phosphoric acid than that secreted during sleep; and that the urine secreted after dinner contained more phosphoric acid than the fluid secreted before that meal. Winter found, from experiments on three young men, that for every kilogramme's weight of the body, the mean daily quantity of phosphoric acid is 0·064 of a gramme. Assuming that 67 kilogrammes, or about 14½ stone, is the ordinary weight of an adult man, the daily quantity of phosphoric acid is 4·288 grammes, the limits being 6·432 and 2·881 grammes respectively. Winter likewise found that the secretion of phosphoric acid attains its maximum in the afternoon (while digestion is going on), that it sinks during the night, and still more in the forenoon. Mosler's experiments were made (1) on himself (his age being 22, and his weight 134 Bavarian pounds); (2) upon three other healthy persons, one a woman; (3) on a woman in the later stages of pregnancy; and (4) on certain patients. The following are the most important conclusions derived from the observations on his own urine.

As a mean of ten experiments, made at different periods of the year, he found that he excreted daily 3·209 grammes of phosphoric acid, of which 2·422 were combined with alkalies, and 0·841 with earths—a ratio of about 3 : 1. With regard to the daily variations, he found that the largest quantity was secreted in the evening, when his mental faculties were most actively engaged, and before the process of digestion had terminated; next in order stood the mid-day and nightly urine; while the least amount was always found in the morning secretion.

The phosphoric acid in combination with the earths, is to that in combination with the alkalies as 1 : 2 in the morning urine, as 1 : 4 in the mid-day urine, and as 1 : 3 in the nightly urine. Great mental exertion increases the quantity of phosphoric acid generally by about one-half; the portion in combination with alkalies being increased about one-fourth, while that which is in combination with earths is increased three-fold. An abundant animal diet increases the secretion of phosphoric acid to nearly double the normal amount, the earthy phosphates being increased in a greater proportion than the alkaline phosphates; and conversely during fasting, the secretion of phosphoric acid falls to about one-half; if, however, water or cider be taken, the diminution is less marked. For details regarding the second, third, and fourth heads, we must refer to the original 'Thesis.'

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* Medico-Chirurgical Transactions, vol. xxxiv. pp. 180—192. 1851.

† Ann. der Chem. und Pharm., vol. lxxviii. p. 153; and Silliman's Journal, second series, vol. xiii. p. 224.

Other determinations of the daily amount of phosphoric acid in the urine have been given by Beneke* and by Böcker.† None of the German chemists whose labours we have been reviewing in the present article, have added materially to the facts elicited by Dr. Bence Jones,‡ regarding the influence of disease on the amount of phosphates.

In connexion with the occurrence of the alkaline phosphates, we may allude to the cause of the *acid reaction* of normal urine. No new light has been thrown upon this point, and we may conclude with Lehmann,§ that in many cases Liebig's view, that the acidity is due to the acid phosphate of soda, is correct, but that often, however, some free organic acid, or some other acid salt which reddens litmus paper, must be present in addition to the acid phosphate of soda; while, in morbid urine, the acid reaction often depends on the presence of hippuric and lactic acids, no less than on that of the acid phosphate of soda. Winter found that an adult of average bodily weight (67 kilogrammes, or 14½ stone) discharged, in the twenty-four hours, as much free acid as would correspond with 2·304 grammes of oxalic acid. The various causes influencing the degree of acidity of the urine are discussed at considerable length by Beneke‖ in his Urological Studies.

Our remarks upon the *physiological conditions* influencing the urinary secretions will be very brief; indeed, we have already anticipated, in our observations on the urea, much that might with equal propriety have found a place under this head.

It is generally held that "the urine of *women* is richer in water, and poorer in salts and urea, than that of men."¶ If this be the case, it is much more probably due to their using less nitrogenous food and taking less exercise than men, than to differences dependent upon *sex*. A reference to the Table in p. 99 will at all events show that the analyses therein recorded are very far from supporting the view which is ordinarily held.

Mosler has made a few observations on the influence of *pregnancy* in its later stages (during the last three months) on the urine. The mean bodily weight being 128 (Bavarian?) pounds, 12 experiments gave 1468 c.c. as the mean daily quantity of urine, the limits being 1980 and 1200 c.c. The reaction was in 3 cases alkaline, in 4 neutral, and in 5 acid. The colouring matter amounted to nearly double the quantity that occurred in his own urine. Both the alkaline and the earthy phosphates were present in less quantity than in his own urine. The following table gives the results of six analyses of the urine in twenty-four hours, in so far as the urea, chlorine, and phosphoric and sulphuric acids are concerned:

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* Archiv des Vereins für gemeinsch. Arbeiten, vol. i. p. 600.

† Prag. Vierteljahrschrift für 1854, vol. iv. p. 138.

‡ Animal Chemistry, p. 87. 1850.

§ Physiological Chemistry, vol. ii. p. 405; vol. iii. p. 540.

‖ Archiv des Vereins für gemeinsch. Arbeiten, vol. i. pp. 436—450.

¶ Lehmann's Handbuch der Physiologischen Chemie, p. 193. 1854.

	Mean.		Maximum.		Minimum.
Urea	26·193	...	30·260	...	20·800
Chlorine	7·930	...	8·600	...	7·200
Phosphoric acid . .	2·422	...	3·328	...	1·440
Sulphuric acid . .	1·250	...	1·608	...	1·080

The specific gravity never exceeded 1011, which, we may observe, was the superior limit assigned by Becquerel. A very simple calculation will show that the daily amount of urea for each pound weight of the body approximates closely to the figures given in our Table for persons in the prime of life (from twenty-two to thirty-eight years).

The effect of *age* on the excretion of the most important urinary constituents is so clearly seen in the table just referred to, that we shall not further allude to the point. From certain observations of Lehmann's, it would appear that the urine of young children contains relatively more hippuric acid, and far less phosphate of lime, than the urine in youth or more advanced age.

From scattered observations in the preceding pages, it appears obvious that *digestion* exerts a marked influence on the excretion of the urinary constituents. We have seen that the maximum quantity of urine is secreted a few hours after the principal meal has been taken; and at no other period of the day is so much chlorine, sulphuric acid, or phosphoric acid given off by the kidneys as in the afternoon.

We have already cursorily alluded to the influence of mental and bodily *exercise* upon certain of the constituents of the urine. From the experiments of Simon* and Lehmann,† it follows that great bodily exercise causes a diminution of the water (we are of course speaking of the urine of twenty-four hours), an augmentation of the urea, phosphates, and sulphates, and a diminution of the uric acid and extractive matters. Hammond's experiments, quoted in p. 81, confirm this statement in so far as the augmentation of urea and diminution of uric acid are concerned, but are opposed to it in reference to the amount of water; Rummel's‡ analyses, on the other hand, give the following results, which are diametrically opposed to those of Lehmann, in reference to the amount of urea and extractive matters:

Urine.	After strong exercise.		After rest.
Inorganic salts	22·7%	...	19·4%
Urea	31·1%	...	60·7%
Extractive matters, uric acid, and mucus .	46·2%	...	19·9%

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* Animal Chemistry, vol. ii. p. 168.

† Physiological Chemistry, vol. i. p. 163.

‡ Verhandl. der Phys.-Med. Gesellsch. zu Würzburg, vol. v. p. 119.

while Mosler arrives at yet another conclusion, namely, "that strong bodily exercise (gymnastics) does not directly augment the urea and chlorine, as has been generally supposed." A further investigation of this important point is obviously requisite.

Mosler is, as far as we know, the only experimentalist who has recently attempted to determine the effect of continuous mental labour on the urinary secretion. As, however, he commenced his investigation by eating a very hearty supper, which must have materially interfered with the correctness of his conclusions, we deem it unnecessary to give his results.

For information regarding the effect of *sleep*, of various kinds of *baths*, &c., upon the urinary secretion, we must refer to the "Report on the Progress of Animal Chemistry" in a subsequent page of the present number of this Review.

George E. Day.

Review VIII.

Lehrbuch der Geschichte der Medicin. Von Dr. H. Haeser, Professor zu Greifswald. Erster Band. *Geschichte der Medicin.* Zweite völlig umgearbeitete Auflage.—*Jena*, 1853.

The History of Medicine. By Professor H. Haeser. Vol. I. Second Edition.

The history of medicine is a branch of medical science the culture and development of which could hardly be thought of until the rich materials contained in the works of the old masters were rendered accessible, after a long interval of neglect. It is, of necessity, a product of modern times. The zeal with which the philologists of the fifteenth century applied themselves to the study of the classics, seized also upon the physicians of this period. As early as the year 1538, the oath (ὅρκος) of Hippocrates was translated by Thomas Linacre, of Canterbury, to whose indefatigable exertions we owe the foundation of chairs at Oxford and Cambridge, for teaching the doctrines of Hippocrates and Galen. He likewise conceived the idea of forming a medical college—the present College of Physicians—in London. The example of Linacre aroused in Germany and France a zeal for the study and publication of the classical writers on medicine. Of the numerous works published at this time, suffice it to mention the valuable edition and translation of the works of Hippocrates, by Foës (1595). The seventeenth century proved less favourable to the study of the history of medicine, since almost all eminent men of this period applied themselves with great vigour to the renewed, and hitherto much neglected, studies of anatomy and physiology. In the eighteenth century, medical men returned with the more ardour to the historical studies, and from this time the history of medicine has grown in importance, and has exercised great influence over the development of medical science.

At the head of the medical historiographers of the eighteenth century we must place Daniel Le Clerc, of Geneva, and John Freind, of Northampton. The work of the former, entitled 'Histoire de la Médecine,' contains the history of medicine only to the time of Galen. Freind's work, which treats of the history of medical science from the time of Galen to the beginning of the sixteenth century, may be considered as the continuation of Le Clerc's.

Of all countries, Germany is the soil on which the study of the history of medicine has been most sedulously cultivated during modern times. We might, indeed, expect, from the leaning of the German mind, that such a subject would be peculiarly congenial to it. The first work in the literature of medicine, comprising the whole history of our science, appeared in Germany, in five volumes, under the title, 'Versuch einer Pragmatischen Geschichte der Arzneikunde.' By K. Sprengel; continued by B. Ebele. The follower of Sprengel, worthy in every respect of his great master, was J. F. Hecker, professor of medicine at the University of Berlin. His 'Geschichte der Heilkunde,' unfortunately not finished, treats only of the medicine of antiquity.

The materials for a complete history are so vast in amount, that Dr. Haeser, whose text-book we are about to notice, justly observes, that the idea of a comprehensive history of medicine can only be realized by the united efforts and labours of a medical society. By these remarks, Dr. Haeser has at once indicated the limits and purport of his own work, of which we have given the title at the head of this article. The author does not pretend to offer a complete history of medicine, but, as he modestly states, desires to lay before the eyes of the reader the most important epochs of the development of our art. A glance at the book will at once convince the reader that the author has not only fulfilled his promise, but has done much more; for this work will prove to be, at the same time, an excellent text-book for the professor, as well as a rich mine of information for the student and medical practitioner who are desirous of information in the history of any branch of medical science. The readiness with which the value of this work was acknowledged on the Continent may be judged from the circumstance, that as early as the year 1847, two years after the appearance of the first edition, a second was called for. Political commotions on the Continent, however, which kept all matters but those of a political nature in the background, prevented the author from publishing the second edition before 1853. However, this delay is the less to be regretted, as it furnished the author with the leisure for thoroughly revising and improving the first edition. On comparing the two editions, this improvement will be seen especially in the sections on Hippocrates, Aristotle, Galen, Celsus, which are entirely re-written, and in the sections on the social position of the physicians at the time of the Roman emperors. But above all, the improvement will be evident in the section treating of the history of medicine in the middle ages (which has been hitherto so much neglected), as well as in that of the last three centuries. As to the method and form of division adopted by the author, it may be stated, that the book does not give an uninterrupted narrative of the history of medicine; but the whole is divided into periods, sections, paragraphs, and notes, in which the different branches of medical science receive a separate consideration.

The first volume contains, in 898 pages, a succinct history of medicine, from the remotest times to the present. The second volume (which, as far as we know, has not yet been published) will contain the history of epidemic diseases. With regard to the contents of the former, they are divided into four periods: the first, extending from the very beginnings of medicine to Hippocrates (400 B.C.), is called the theurgical-empirical epoch; the second, from Hippocrates to Galen (200 B.C.), the author terms the epoch of artistical treatment; the third embraces the time from Galen to the reformation of the medicine in the sixteenth century (dialectic epoch); the fourth extends from the sixteenth century to our time (the scientific epoch). Our author has not only given a general view of the history of medicine as a whole in each of these periods, but, by arranging his subject under appropriate heads, has been enabled to describe separately the development of medicine in individual nations; thus we are enabled to see at a glance the distinguishing features characterizing the medical literature of the Indians, Chinese, Egyptians, Jews, Greeks, Romans, Arabs, Italians, French, English, Germans, &c.

The great strides made in science have ever been due to the surpassing intellect of a few great minds, who have shown the way to their own and to many succeeding generations. Appreciating this fact, the author has bestowed special care in bringing to light the merits and labours of the men to whom medical science is pre-eminently indebted. With what care the great masters of our art are treated in this work, may be illustrated by a single example. Sections 36—56 treat of Hippocrates in the following order:—after a biographical sketch, his writings and their divisions are considered; the general influence of Hippocrates is discussed, and then follow analytical accounts of his anatomy, physiology, ætiology, pathogeny, semeiotics, therapeutics, materia medica, surgery, ophthalmology, and midwifery. The author has bestowed the same care upon the treatises on Galen, Celsus, and the other great masters. But although so much attention is paid to the ancient and mediæval times, by far the greatest labour and care have been bestowed on the history of medicine of the last three centuries. The history of individual branches of medical and chirurgical science—namely, general and special pathology, therapeutics, anatomy, physiology, chemistry, natural philosophy, physics, materia medica, and forensic medicine—constitutes another sub-division of the work. An account is given of the various methods of treatment, and their gradual development, including even the history of homœopathy and hydropathy.

The references given in the shape of foot-notes are very copious; but the plan and intention of the book indicated certain limits in this respect. Those who are desirous of having a complete biography of medical science, must refer to special works on this subject, as those of Haller and Choulant. A full and complete index is appended.

The author deserves great praise for his earnest and successful endeavour to mould the vast materials into a simple, clear, and intelligible form. This merit is the greater as the subject is in itself one that offers many difficulties not embarrassing the political historian. It is, moreover, a peculiar merit in a German, who is not forced by habit and custom into that terse and clear mode of expressing his thoughts which may be regarded as a characteristic of the better writers of this country.

We trust, that even the brief announcement of this valuable work which we are able to lay before our readers may attract their attention to it. Nor can we avoid expressing the hope, that a translator may be found to render Dr. Haeser's book, which treats of an important branch of medical science, but too little regarded in England, accessible to those who are unacquainted with the German language.

P. Gerber.

Review IX.

1. *Beiträge zur Vergleichenden Pathologischen Anatomie der Gelenkkrankheiten.* Von Dr. E. Gurlt, Praktischem-Arzte und Assistenz-Arzte der Chirurgisch-Augenärztlichen Klinik der Universität zu Berlin. 1853.

Contributions to the Comparative Pathological Anatomy of Diseases of the Joints. By Dr. E. Gurlt.

2. *Traité des Tumeurs Blanches des Articulations.* Par Dr. J. Crocq. *Bruxelles*, 1854.

Treatise on White Swellings of the Joints. By Dr. Crocq.

(Continued from No. 29, p. 70.)

In our last paper on the subject of diseases of the joints, we devoted our chief attention to the views of the German surgeon whose name stands first at the head of the present article. We now proceed to examine the views of our Belgian confrère.

Dr. J. Crocq has recently published an interesting work upon those diseases of joints formerly comprehended under the head of "White Swellings," a term still commonly used in France.

"White swelling," *tumeur blanche*, is indeed so far unobjectionable, that by expressing no special point in pathology, it cannot convey an erroneous impression; but it is wanting in precision, and groups together all chronic enlargements of joints unattended by redness or discoloration of the skin. It strikes us as the more remarkable that Dr. Crocq should have given such a title to his work, because he is obviously acquainted, from personal observations, with the variety of forms which this class of articular affections may assume. He describes with faithfulness and accuracy the structural changes in the synovial membrane, the ligaments, the cartilages, and the bones; he attaches to all their due and proper importance, but still preserves a name the employment of which, in other hands, might tend to discourage accurate diagnosis.

Chapter II. contains a list of synonyms of *Tumeurs Blanches en général.* We here find classed together synovitis, articular osteitis, arthrocacæ, spina ventosa, scrofulous caries, articular fungus, consecutive and spontaneous luxation. Now the synovitis, illustrating one form of white swelling, is that better known as hydrops articuli, increase in the synovial secretion, without pain or marked inflammatory symptoms, but attended by softening and elongation of the ligaments, œdema and weakness of the limb. How different is that disease from articular osteitis, the principal lesions of which are thus arranged by Dr. Crocq:

1. Increase of vascularity in the bone, indicated by red, brown, or violet discoloration.

2. Dilatation of the vascular canals in the compact tissue; the feeble adherence on separation of the periosteum, its thickening, softening, and augmentation of vascularity.

3. Enlargement of the cellules in the spongy texture.

4. The presence of a cellulo-vascular structure, and of pus in the dilated cellules, under the cartilages, and under the periosteum.

5. The complete disappearance of the osseous tissue, replaced entirely by cellulo-vascular tissue.

6. The formation of white or yellow sparingly-vascular deposits, sometimes of the same consistence as the surrounding tissues, sometimes harder and more eburnated.

7. The augmentation of volume in the bone by deposit of osseous matter upon the surface.

8. The formation of caverns, resulting from the complete destruction of the osseous lamellæ by the production of pus and fungous growths.

9. Brownish discoloration, fungous growths, and suppuration in medullary cavity.

10. Necrosis and separation of entire osseous lamellæ, raised by the fungous growths.

To render the list of osseous affections constituting white swelling complete, he adds,

11. Tubercle in bone.

12. Cancer in bone.

It needs no detail to show how spina ventosa—an obscure affection, probably expansion of the walls of a bone, by the deposit of pus, the result of active inflammation—differs from scrofulous caries, or ulceration of bone; and how the separation of the articular cartilage differs from tuberculous deposit; nor how the latter, again, presents wide points of difference from articular fungus, or the light-brown degeneration of the synovial membrane. We must, therefore, express a preference for the nomenclature commonly used in this country, or for that of Gurlt, who has greatly contributed, by his laborious collection and arrangement of pathological observations, to clear up much that was doubtful or only partially known, and to put diseases upon their proper bases.

In the section devoted to pathological anatomy, Dr. Crocq gives a chapter to the examination of the contents of the articular cavities. These contents do not only consist of altered synovia and of pus, as is commonly said; the most remarkable is a solid element, which undergoes transformation into a tissue of new formation, called by the author cellulo-vascular. He describes with care and accuracy the different varieties of this substance—viz., false membranes, fungosities, lardaceous tissue, cellular and osseous tissues. The description shows how the different elements pass one into the other. Finally, the author confirms the fact of the direct formation of tubercle in the interior of a joint.

The changes which occur in cartilage he subdivides as follows:

1. Hypertrophy; which he himself has not witnessed, although he admits the possibility of its occurrence. He believes rather in a swelling by *imbibition*, by which process cartilages are, in his opinion, nourished. A case of hypertrophy has been related and published by Redfern.*

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* On Anormal Nutrition in Articular Cartilages.

2. The wearing away of cartilage, *l'usure*, is seen in old subjects or in joints long kept in a state of immobility. It is characterized by asperities, inequalities, grooves, depressions, &c., which take off from the cartilage its smooth aspect.

3. Thinning of the cartilage, atrophy, general or partial, seen in joints long disused, and kept in a state of perfect rest.

4. Decortication, or separation of the cartilage from the head of the bone by granulations or fungous growths. The cartilage loses its white colour, and passes into the state described by Delpech as "*perte d'élasticité.*" Crocq, however, properly considers this but a form of ramollissement, or softening. The microscopical changes, in an histological point of view, are—infiltration of a serous fluid; enlargement of the cartilage cellules, by which they acquire five to six times their normal size. They may remain oval or round in form, when they constitute gelatiniform ramollissement; or they may become elongated, their neighbouring parts touching in consequence of their swelling; they then become blended, and constitute fibres, the nuclei disappearing; here we have fibrillar ramollissement, or fibrous degeneration. Sometimes the cellules are found infiltrated by fat.

5. Erosion is loss of substance, determined by any cause which interferes with the materials of nutrition in cartilage. Sometimes it comprehends the whole thickness down to the bone, sometimes only a part. These erosions are constantly covered by a layer of exudation, generally cellulo-vascular tissue, sometimes tubercle. There is never injection of blood into the cartilage; although the author believes that, under some morbid conditions, vessels may be prolonged into grooves found in this structure. This disease, known as ulceration of cartilage, commences, he adds, either on the surface or between the cartilage and bone. In the latter case, a layer of cellulo-vascular substance covers the articular surface of the bone, raising the cartilaginous covering often unequally into eminences, and ultimately perforating it in various situations. The museum of the Royal College of Surgeons possesses a specimen in which this *riddled* condition of the articular cartilages is well seen in the knee-joint. Dr. Crocq objects to the term *ulceration*, because it implies vascularization, of which the cartilages are not susceptible.

Fibro-cartilages offer lesions analogous to those of diarthrodial cartilages. Nevertheless, there is a great difference, the former being nourished by vessels directly entering them. Therefore, we may have pus and cellulo-vascular tissue produced in the middle of their substance. Similar changes may take place in the epiphysiary cartilages, which are also vascular.

We now proceed to examine specially some of the principal morbid changes affecting the joints of the lower extremity.

Diseases of the Hip Joint constitute the largest class of articular affections, and are of especial importance in relation to the changes which they secondarily produce in the vertebral column and trunk.

Congenital Deficiency of the Ligamentum Teres, mentioned by Gurlt among the "abnormities" of the hip, is of more than doubtful occurrence, except in a few of the cases, where there is a congenital absence of the head and neck of the femur. Various morbid processes will cause the softening and gradual removal of this ligament, such as rheumatic inflammation, by which the articular cartilage is absorbed, the head of the bone eburnated, and the capsule thickened. Such specimens, commonly seen in any dissecting-room, generally exhibit either partial softening of the ligamentum teres, or its removal, so complete that even the depression on the head of the femur, to which it is attached, is smoothed over, and nearly obliterated. The ligament is generally deficient on both sides. To rheumatic disease must, we think, be attributed the cases of "congenital deficiency" related by Genga,[*] Nicholai,[†] Sandifort,[‡] and J. Palletta;[§] and this opinion is corroborated by the fact that the subjects were all aged, or at least adults, in whom the occurrence of previous disease was highly probable.

We mistrust much of the evidence brought forward by Gurlt to prove congenital atrophy of the head of the femur. At birth the skeleton is usually well formed, and arrest of development takes place subsequently, and during growth, in this or that limb, consequent upon want of exercise, from muscular paralysis, club-foot, or some such cause. Thus Sandifort's[‖] case of "congenital atrophy" of the neck of the femur, is that of "a young skoliotic person afflicted with varus;" and that of Prochaska[¶] is obviously one of absorption of the neck of the femur, consequent upon rheumatic disease, for there was eburnation of the surfaces of the bones, osteophytes around the joint, and absence of the ligamentum teres. It is to be remarked, that in many of these cases the head and neck of the bone acquire an abnormal relation to the shaft, so that the whole limb undergoes a twist, the foot being usually directed more outwards than natural. These changes should rather be spoken of in connexion with the diseases of which they illustrate a stage, than in a special chapter. The general effect produced by congenital paralysis of only one set of muscles—such as the tibialis anticus and the extensors of the foot, causing talipes equinus; or of the flexor muscles, causing talipes calcaneus, &c.—have received of late years much attention. The museum of St. Bartholomew's Hospital contains a specimen well illustrating these points. The case must have originally been one of talipes equinus, the toes being pointed downwards, and the articulating surfaces altered in direction: all the bones are smaller than natural, and the neck of the femur would be described as atrophied. The specimen, however, is one of arrest of development proceeding from known and recognised causes.

Hydrarthrosis of the Hip Joint.—A rare affection, correctly referred by Gurlt to inflammation of the synovial membrane. To the cases related by Stanley[**] are added those of Parise,[††] Lesauvage,[‡‡] and Jolly,[§§] by which we learn that the fibrous capsule of the joint becomes softened and elongated, the ligamentum teres split into three strings, and partially destroyed; and thus the fluid may accumulate in sufficient quantity to drive the head of the femur from the acetabulum, and produce a complete dislocation. When the healthy capsule is distended by artificially injected fluid, the thigh is flexed, abducted, and rotated outwards: when the bone is dislocated, it is drawn upwards and backwards upon the ilium. Cases of purulent deposits in the hip are also mentioned, as related by Zuccarini[*] and M'Dowel.[†]

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[*] Genga: Anat. Chir. in Rom. 1678.

[†] Nicholai Decas: Observ. illustrium Anatomicarum. Argentorati, 1725.

[‡] Ed. Sandifort: Observ. Anatomico-Patholog., Libri 4. Lugd. Bat. 1771.

[§] J. B. Palletta: Exercitationes Pathol., Pars i. 68.

[‖] E. Sandifort, jun.: Animad. de Vitiis Congenitis. Lugd. Bat. 1836.

[¶] Prochaska: Disquisitio Anatomico-Physiologica Organis. Corp. Hum. Vien. 1812.

[**] Medico-Chirurgical Transactions, vol. xxiv.

[††] Archiv. Gén. de Méd., troisième série, tom. xiv. 1849.

[‡‡] Ibid., deuxième série, tom. ix.

[§§] Dissert. sur les Hydropsies Synoviales. 1829.

Inflammation of the Hip Joint.—We concur with Gurlt in his remark, that while this disease is the most common among the affections of joints, its pathogeny, and the anatomico-pathological changes belonging to it, have not yet been traced through their successive stages with sufficient accuracy; authors have rather occupied themselves with representing forms of disease as borrowed from theory, with which the facts proved by pathological anatomy do not always agree. Inflammatory disease of the hip joint does not always commence in the same way; while in many cases the inflammation of the synovial membrane goes hand in hand with a similar process in the spongy substance of the bones forming the joint, there are other cases in which the latter morbid changes are either absent or exist in a very slight degree. And this we believe to constitute a most important point of difference; for when the head and neck of the femur have been extensively diseased, ulcerated, and changed both in form and structure, those processes which end in osseous anchylosis will not readily go on. On the other hand, the ulcerative absorption of cartilage from the opposed surfaces of not unhealthy granulating bones, may be said to constitute a stage towards repair. To found a system of treatment, in cases of strumous "hip-disease," upon the idea that the morbid process was simply synovitis, would lead to error; for it is well known that the reduction of the strength, by the abstraction of blood, and other antiphlogistic measures, combined with the imposition of a system of perfect rest, will in time produce such changes in the skeleton as may in themselves lead to the destruction or injury of the joints. In the museums of St. Bartholomew's, of Guy's, of St. Thomas's, and other hospitals, there are specimens showing so perfect an anchylosis between the ossa femoris and the os innominatum, the bones retaining their perfect outline, that the cancellous texture, as proved by the sawn surface, is continuous; while others exhibit destructive processes both in the femur and the acetabulum which must have precluded all possibility of cure in such a way.

Premising that there are several varieties of inflammation of the hip joint, we proceed to investigate the morbid changes so well known as occurring in young subjects, and termed strumous. We notice very early softening of the ligamentum teres: Fricke[‡] dissected the diseased hip joint of a child, aged 4, in whom he found the ligament thinned, and its synovial investment softened and swelled; further observations show that this process goes on until the fibrous structure is broken up, and only a few shreds remain,—usually attached to the head of the femur,—but these ultimately disappear by constant friction. These changes are accompanied generally by synovitis, shown by the red and swelled condition of the synovial fringes, and of the gland of Havers, the latter becoming also remarkably hypertrophied. The sawn bones, however, at the same period, exhibit in their spongy texture an exudation, which may lead to many metamorphoses. It may cause alteration of form in the articular extremity of the femur, and become hardened and ossified; but more generally it assumes a purulent character, the pus eventually becoming thick and caseous, and forming a substance which has been frequently mistaken for tubercle. During these processes the articular cartilages may remain unchanged, and suffer even at a later stage only partial destruction. In young subjects the epiphyses are separated from the head of the femur,[*] or the great trochanter from the diaphysis, the cavity of the joint remaining but slightly affected. The limb may, under these circumstances, become really shortened from dislocation, although this occurrence is by no means so common as heretofore supposed, such deformity being generally due to obliquity of the pelvis, or, when real, to absorption of the head and neck of the femur, and widening of the acetabulum. That this disease commences rather in the bones than in the soft parts, may be at least inferred in those cases where there is a remarkable difference in the degree to which the ulceration has proceeded in the acetabulum and femur.[†] Sometimes the former is so much destroyed that perforation is made into the pelvis, where pus may enter—as shown by a preparation in St. Thomas's museum—make an opening into the rectum, and discharge itself per anum. In young subjects, the incompletely ossified acetabulum may become resolved into its three primary constituent pieces of ilium, ischium, and pubes, and the opening undergo further enlargement from ulceration; through this the head of the femur has been, by some accidental movement or pressure, driven in and firmly wedged.[‡] In the progress of the disease the thickened capsule becomes distended by pus, which ultimately, making its way externally, forms consecutive abscesses, either on the anterior or the posterior part of the limb. It would exceed the limits of our review to trace the different courses which the pus pursues. It may find its way through the bone to the iliac fossa, or enter the bursa of the psoas muscle. M'Dowel[§] has seen an iliac abscess communicate with the small intestine, and with the external iliac artery; and R. Adams has known the fluid to make its way into the vena cava inferior.

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[*] Deutsche Klinik, No. 7. 1852.

[†] E. M'Dowel: Dublin Journal of Medicine, vol. iii.

[‡] Annal. der Chirurg. Abtheilung des allgem. Krankenhaus zu Hamburg, Band ii. 1833.

In cases of consecutive dislocation, the femur is usually thrown on the dorsum ilii, but Portal[‖] and Burtz[¶] have each related a case in which the head of the bone lay in the obturator foramen. Gurlt has failed to find any record of a dislocation upon the ischiatic notch or the ramus of the pubes.

We will now proceed to review the principles of treatment laid down by Dr. Crocq for the different stages of this disease, which, we cannot help thinking, corresponds with his *tumeur blanche*, although he denies the scrofulous origin of the latter. White swellings, he observes, are cases of exudative arthritis, with a tendency to the production of pus and of cellulo-vascular tissue. There are four distinct elements or stages: 1. that of abnormal afflux of blood; 2. of plastic exudation; 3. of organization of the cellulo-vascular tissue; 4. of the production of pus.

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[*] Museum St. Bartholomew's Hospital, Series I. No. 221.

[†] Ferrall: Dublin Journal of Medical Science, vol. xvii.

[‡] Museum Royal College of Surgeons of England, No. 940.

[§] Dublin Journal of Medical Science, vol. iv. 1834.

[‖] Observat. sur la Nature et sur le Traitement du Rachitisme. 1797.

[¶] Rust's Magazin, Band xlv. 1835.

At the commencement the two first elements exist alone, namely, the afflux of blood and the exudation. The indication is to prevent the continuation of these phenomena. The first condition required is the complete immobility of the articulation; by this we annihilate one of the causes which tend to make it a centre of fluxion. But there is another measure which may be combined with the preceding; namely, compression, by which the surrounding soft parts are supported, and the vessels are compressed. We believe that these two points, immobility and compression, merit in their more complete and scientific application greater general attention than they receive in this country. At a later stage, when the exudation has become organized as cellulo-vascular tissue, the indications consist in repressing the afflux of blood, and preventing further exudation; and in favouring the transformation of the cellulo-vascular into cellulo-fibrous tissue. The first condition has been already met by immobility and compression. As to the second, the result to be obtained is cicatrisation, for it consists in the passage of the fungous growth into fibrous tissue. The author meets these difficulties by the same means, namely, a system of immobility and compression. Of the form of bandage we will speak hereafter.

Four important elements yet remain to be considered—pain, position, displacements or luxations, spasmodic contraction of the muscles. Compression quiets the action of the muscles. The apparatus for rendering the limb motionless must, by extension, prevent displacement; these means alone will assuage pain. The principles have long been recognised. Brodie and Desault recommend immobility; Malgaigne and Bonnet both apply it with success. Compression was employed in olden times by A. Paré, and in our days by Scott and Velpeau. The combination of the two has been especially insisted upon by MM. Seutin, Lavacherie, A. Fredericq, Burggraeve, &c. Before proceeding to a description of the apparatus employed by Dr. Crocq, we will consider other points of general treatment.

The following have been recommended in the treatment of these affections:—Local bleeding, narcotics, astringents and the application of cold, resolutives, revulsives. Local bleedings have been strongly recommended by Latta and Lisfranc. But what is their mode of action? By the abstraction of blood? evidently not. It is by directing the current of blood to another part, and by acting as a derivative. Thus, local bleedings, to be useful, should be often repeated; otherwise they become hurtful. They should be employed only on the robust Dr. Crocq does not attach much value to cataplasms and warm fomentations. He believes them to be injurious by favouring the afflux of blood. In this we do not agree. They may be incapable of effecting any important change for the better, but they possess no power of doing harm. He advocates a plan which we can strongly recommend for the relief of pain, which often destroys rest and impairs digestion, namely, the local application of narcotics. Medicated poultices, containing laudanum, &c., are not sufficiently active, but a pomade of chloroform, commencing at half an ounce to the ounce of lard, has acted beneficially, especially in nervous subjects. As the effects of chloroform are transient, 5 to 10 grains of the acetate of morphia or of the cyanide of potassium may be added to the same dose. Dr. Crocq prefers the following pomade:—Ext. belladon. alcohol., ʒij—℥j; axung. recent., ℥j; ol. olivæ, ʒss. Should this fail, half a grain to a grain of acetate of morphia may be added.

Revulsive remedies, which act on the skin and, as Broussais observed, call the irritation from one point to another, are divided into four classes. 1. Rubefacients; 2. blisters; 3. caustics; 4. seton. Of the two first no particular notice is required, excepting that Crocq warns the surgeon against the practice recommended by Velpeau, of enveloping the whole joint in a blister. He relates the case of a man aged 49, admitted June 29th, 1848, into the Hospital St. Jean, under the care of M. André Uyttenhoeven, with considerable effusion into the synovial membrane of the right knee, of eight weeks' duration. The limb was kept quiet, and a pomade of nitrate of silver applied without any result. The application of a blister was followed by shivering and general disturbance. A puncture made into the joint gave egress to thin synovial and purulent fluid, but ulceration of the cartilages came on, and after a prolonged illness, the patient recovered with a deformed and partially dislocated limb. We do not, however, think that this case quite proves the point which it was intended to illustrate. Blisters should be applied only in chronic cases, or after the vessels of a part in a state of active inflammation have been relieved by local depletion; and in the case here related no such preliminary measure had been adopted, the man, from all accounts, being healthy and strong. It shows, however, how constantly destruction of the articular cartilages supervenes upon the puncture of a joint.

Mr. Horner has lately recommended the continued application of potters' clay, $\frac{3}{4}$; guano, $\frac{1}{4}$. The skin soon reddens; in a few days phlyctenulæ appear, then a real vesication. These effects are due to the urate of ammonia found in the guano.

The most effectual remedy of this class, however, is undoubtedly cauterisation, which Crocq arranges under six heads. 1. Inherent cautery; 2. transcurrent cautery; 3. moxa; 4. caustic potash; 5. Vienna paste; 6. chloride of zinc.

The inherent cautery is effected by a metallic instrument, of various forms, heated to a white heat, and allowed to remain a sufficiently long time to act deeply. If it be the prismatic cautery of Rust, the instrument, firmly pressed against the skin, must be made to pass slowly over an extent of two to four inches. The joint should be thickly covered with soft parts.

The transcurrent cauterisation is effected by a knife or spear-shaped instrument. It should be heated to a white heat, and made to pass over the skin without resting, so as to produce a simple seam or streak. These streaks should be rather less than an inch from one another, and in the long axis of the limb. The heated iron may be made to pass several times over the same line, so as to give it a deep yellow hue. It is applicable to joints placed immediately under the skin, such as the knee, the elbow; while the preceding is preferable for thickly-covered joints, such as the shoulder, the hip, the spine. The pain may be mitigated, or indeed altogether removed, by the employment, for half an hour to an hour, of pomade of chloroform, or, for a shorter period, of pounded ice; but it is better that the patient should, when possible, suffer pain.

The rules for the employment of revulsives are, then, as follows. They must be applied at a proper distance from the diseased synovial membrane. The more acute the inflammation, the further off they must be made to act. Slight cauteries must be applied to joints superficially seated. There is no need to promote suppuration. The cauterised lines need not be above one-half or three-quarters of an inch wide. A first application should be followed by others. Violent revulsives should be avoided in infants, and in nervous and irritable subjects. In such cases, rubefacients of nitrate of silver ointment, and such like remedies, are best borne. Of the moxa and seton no special remarks are needed. With these remedies, compression and immobility should always be combined. The cases requiring specially the addition of revulsives are chronic articular diseases, and those of the hip accompanied by muscular spasm.

In the selection of an apparatus, it should be borne in mind that the position of the bones which will subsequently give to the patient the most useful limb, is not always that one most conducive to the arrest of the disease. Let us take for an example the articulations of the inferior extremity. The ligaments, while they remain, are so formed, that in the extended state of the limb, the bones are firmly pressed against one another; and the patient would therefore suffer greatly, were their articular extremities inflamed, and the cartilaginous coverings loosened. The directions given by Dr. Crocq are better suited for joints where anchylosis is going on, or where the destruction of the surrounding parts has been considerable. He recommends that the articular surfaces should touch one another in as many points as possible, and the cavity offer as little capacity as possible. He prefers the amovo-inamovible apparatus of M. Seutin, which is applied as follows; all osseous and tendinous elevations are protected by wadding, and the whole limb is surrounded by a roller, commencing at the affected part, or at the extremity. This bandage is covered by starch upon its surface, care being taken to avoid spreading it over the elevations, where it would press unpleasantly when it became hard. Two or three layers are thus applied. Then pasteboard splints are formed for the limbs; they are softened in water, coated on both sides by starch, supplied by wadding pads, and torn, rather than cut, into shape. A starched roller holds them in place.

This apparatus is, however, better suited to the knee and elbow than to the hip, which may require more perfect means of extension, such as are offered by the long wooden splint and the perinæal band. Dr. Crocq has related some cases, and we have seen others, in which the effects of a well-applied apparatus have been remarkable in relieving pain, controlling muscular spasm, and diminishing the general swelling.

Dr. Crocq does not approve of the system of frequent purging, as practised by M. Guerin, nor of that of sweating, as recommended by Bonnet. Mercury and iodine appear to him, as internal remedies, among the most valuable. The former is preferable in acute and sub-acute diseases attended by great pain; the latter in affections of more chronic character. He strongly disapproves of local bleeding in scrofulous or weakly subjects. He recommends the opening of abscesses by the lancet when they are small, by a subcutaneous incision when they are of larger extent. He uses afterwards medicated injections, especially those containing iodine.

Several interesting chapters are devoted to the complication of white swelling; the treatment necessary during the period of reparation; the consequences of white swelling—namely, atrophy and stiffness of the limb. The principles laid down may be studied with advantage.

Chronic Inflammation of the Hip Joint.—This disease, called, in Ireland, by Colles, R. Adams, and R. W. Smith, first, *morbus coxæ senilis*, then chronic rheumatic inflammation of the hip joint, is known among the Germans as *malum coxæ senile;* but to the former is due the merit of having first investigated its pathology. The difference in form of the articulating surfaces, and especially of the head of the femur, are so great, that the same disease can hardly be recognised in all its multiplicity of form. We see the neck of the femur shortened; the head flattened and approximated to the shaft; or the head and neck may become wedge-shaped; the acetabulum acquiring a larger and deeper surface. The surrounding muscles are pale and atrophied; the fibrous capsule thickened, or even cartilaginous; the joint contains an inconsiderable quantity of thick viscid synovia. The ligamentum teres is first reduced to shreds, and then destroyed, and destructive ulceration of the head of the femur often commences at the point of its insertion. The synovial fringes are thickened and reddened, and form those fibro-cartilaginous bodies which, attached by a narrow peduncle, grow until they become brittle and break off, so as to become loose within the synovial cavity. Fanciful speculations have been entertained as to the origin of these loose articular bodies; but we can confirm, from personal observation, the opinion of Gurlt, that they spring from the synovial fringes; that they grow, covered by synovial membrane, so long as they maintain their attachment; that the presence of cretaceous matter renders the pedicle friable, when it breaks in some movement of the limb, and liberates the growth, which from that time ceases to increase The articular cartilages undergo fibrous degeneration, or, as some still term it, "ulceration from the surface;" they are removed, and the bones, often changed in form, become covered with an ivory-like deposit; the deepened acetabulum may project into the pelvis, and its border become so prominent and contracted as to prevent the escape of the head of the femur; the great trochanter then presents an articulating surface with the border of the acetabulum.

Hydatids in the Hip Joint are rare. Cases have been recorded by Fricke,[*] Rokitansky,[†] and Froriep.[‡] There is likewise a preparation in the museum of St. Bartholomew's Hospital.[§]

Osteoïd Tumours have been recorded by Hamilton Lebatt[||] in the museum of the Surgical Klinik at Berlin.[¶]

Instances of cancerous disease are related by Froriep,[**] and are contained among the specimens in the museum of the Royal College of Surgeons of England. They scarcely can be included in the pathology of joints. One fact, however, merits attention—that malignant disease commencing in the bones is more apt to be local than when it originates in the soft parts. We remember a case under the care of Mr. Stanley, illustrating this point. The whole upper part of the femur, enormously enlarged, was the seat of fungous disease, under which the patient, a young girl about 12, died. Upon examination of the body, every other tissue and organ were found healthy.

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[*] Hamburger. Zeitschrift für die Gesammte Medicin, Band vii.

[†] Handbuch der Path. Anat., Band ii.

[‡] Chirurg. Kupfertafeln. Weimar, 1842.

[§] Series I. No. 215.

[||] London Medical Gazette, vol xi. [illegible]8.

[¶] Pathol. Anat. Sammlung der Chir. Klinik der Univ. zu Berlin.

[**] Chirurg. Kupfertafeln, 440.

Congenital Luxations of the Femur have been recorded in a sufficient number of instances to render the possibility of the occurrence no longer questionable. The head of the femur is thrown in different directions, mostly upwards and backwards, when the luxation may be either complete or incomplete; it may be single or double—i.e., affecting one or both sides. Generally, there are some co-existent congenital malformations to explain the reason of the displacement. Thus we find in those joints which have never sustained the weight of the body, both the head and neck of the femur absent, as mentioned by Vrolik,[*] the articulating surface lying on the great trochanter; or the head of the bone flattened, and resting upon some abnormal surface exterior to the acetabulum, as recorded by Parise.[†] The changes in the form of the acetabulum are dependent upon the absence of the head of the femur: it is altered in shape, and filled with fatty or with fibrous tissue: all these observations were made in very young infants. In one case, related by Parise,[‡] there was congenital distension of the capsule by synovia (hydrarthrosis), the infant being 2½ months old at the time of examination. Other instances have been recorded by Cruveilhier[§] and Tortual.[‖] As the child gets older, the alterations of form in the head of the bone and the acetabulum become more marked; there is but a slight depression on the ilium to receive the displaced and deformed femur; but the ligamentum teres is generally present; nature makes but a faint attempt to construct another joint, and in this point of view there is a marked difference as contrasted with the processes which go on when dislocations occur at a more advanced age after accident.

We have not space in the present review to do more than direct attention to the chapters in Gurlt on old traumatic luxations and united fractures. He has collected all the more important observations; and presented, within a few pages, the results as described by most modern writers. One remark merits special attention: he says that, while a new bony capsule forms round the head of the femur, when dislocated upon the pubes, a simple fibrous cushion is deposited when the bone is thrown on the ischiatic notch. He quotes the well-known case of Sir A. Cooper, of non-reduced dislocation on the pubes, with the formation of a new acetabulum, and refers to the museum of the College of Surgeons of England (No. 881) in confirmation of the same fact. Non-reduced dislocations on the ischiatic notch are quoted from Gruber[¶] and

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[*] Tabulæ ad illustrandam Embryogenesin, § 106, tom. lxxxiv.

[†] Archives Gén. de Méd., troisième série, tom. xiv.

[‡] Ibid.

[§] Anat Pathol., liv. 2. pl. 2.

[‖] Ueber angeborene Abweichungen in der Contin. des Knochensystems. 1834.

[¶] Neue Anomalien als Beiträge zur Phys.-Chir. und Path Anat. 1849.

A. Cooper,[*] both of whom agree in speaking of the fibrous character of the repairing material.

It is, perhaps, not quite out of place to offer a reply to a simple question often asked by younger members of the profession—namely, Why, in cases of dislocation of the femur, or the dorsum ilii, or the ischiatic notch, is the head of the bone always thrown backwards, so as to cause inversion of the limb? It is not so generally known as it should be, that, while the anterior part of the capsule is firmly attached to the anterior inter-trochanteric line, strengthened by the fibres of the strong ileo-femoral ligament, the posterior part is unattached, and may almost be regarded as an annular ligament. In dislocation on the ilium, the front part of the capsule, only partially torn, still holds the trochanters, and presents an opposing surface to the round head of the femur, which then slips backwards through the non-resisting structures behind. In every specimen in the museum of St. Bartholomew's Hospital, this explanation holds good. It may be safely affirmed, that the functions of the different ligaments, and their influence on the movements of limbs, are far from being generally understood.

The Knee Joint.—Some few instances of congenital malformation of the knee joint, by union between the femur and tibia, or the femur and patella, are recorded by Wutzert[†] and Von Ammon.[‡] Of congenital dislocation some instances are related by Kleeberg.[§] Inflammation of the synovial membrane, in all its stages, has been described as often as any form of joint disease; and of late years, in connexion with similar affections of the surrounding synovial bursa, a point of very considerable importance, chronic synovitis, shows itself by the development of synovial fringes and loose cartilaginous bodies. Inflammation of the knee joint, involving the articular extremities of the bones, is that which merits most particular attention.

Internal derangement of the knee joint consists in partial displacement of one of the semi-lunar cartilages. This is of necessity the external, and not the internal, as was affirmed by the late Mr. Vincent. The internal semi-lunar cartilage is really of the shape its name imports. It is attached to the tibia by its widely separated extremities, and to the internal lateral ligament at the central point of the circumference. The external is nearly circular, and much more loose, being attached only by its convergent extremities. When this accident occurs, either the anterior or the posterior part of the external semi-lunar cartilage slips between the opposed extremities of the femur and the tibia in the flexed condition of the limb, when the lateral ligaments are relaxed, and the bones loosely held together. Forcible extension presses the bony surfaces together, and drives the displaced cartilage into its proper position.

The bursæ, which in the normal state most frequently communicate with the cavity of the knee-joint, are, 1. A bursa above the joint, between the quadriceps extensor cruris and the femur. 2. The bursa of the semi-membranosus. 3. Accidental communications between the knee joint and the bursæ of the sartorius, gracilis, semi-tendinosus, &c.

Inflammation of the Knee Joint seems in some instances to commence in the synovial membrane; in others, in the articular extremities of the bones; in a third class of cases, there may be a combination of the two; and this variety in the localization of the same affection merits special attention from the surgeon. The "white swelling" is characterized by effusion into the soft parts external to the synovial membrane, raising the integument into an irregular and often knobbed swelling. It is produced by an homogeneous, lardaceous, or gelatinous substance, but little vascular, deposited from the vessels supplying the diseased synovial membrane.

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[*] Op. cit.

[†] Müller's Archiv. Jahrg. 1834.

[‡] Die angebor. Chirurg. Krank., § 285.

[§] Hamburg. Zeitschrift, § 160. 1837.

The articular extremities of the bones are affected either superficially or deeply: when the former, the opposed bony surfaces, especially of the patella and the external condyle of the femur, are denuded and rough; when the latter, the spongy substance of the head of the femur may be deprived of vitality, though still adherent to the living bone, and infiltrated by pus; or the matter may be accumulated in a circumscribed cavity in the head of the tibia, lined by a vascular membrane, and constituting an abscess.

We do not think that Gurlt has laid quite sufficient stress upon these two forms of disease, involving, as they do, important points of surgical treatment. In both, operations are required; the head of the tibia must be denuded and explored, the outer wall being removed by the trephine. In the one case, the dead spongy substance must be scooped out by the chisel or knife; in the other case, the aperture made by the trephine permits the escape of the pus, and, it may be, of some small sequestræ; and these operations should not be too long delayed, for the process of inflammation in bone leads to thickening of its substance—often to loss of vitality. Should the layer of bone between the abscess and the surface of the joint persist, its exfoliation or separation leads to such destructive changes in the cartilage and synovial membrane, that but little hope remains for the preservation of the limb.

Bony anchylosis is not very uncommon in the knee, as is the case in most ginglymoid joints. Instances are preserved in the museum of the Royal College of Surgeons of England, and related by Sandifort.

The treatment of diseases of the knee must be conducted upon the same principles as that of diseases in other joints; but we cannot forbear referring to a remark by Bonnet, which is thoroughly borne out by both the anatomy and physiology of the joint. After remarking that after the treatment of a fracture of the leg or thigh, it is very difficult to flex the knee if it has been kept in an extended position, while it is easy to extend it if it has been flexed, he asks, Whence arises this difference? I attribute it, he adds, to the fact, that in flexion the articular surfaces of the tibia touch those of the femur in only a part of their extent, while in extension their co-aptation is complete and general. Injections forced into the joint after death, demonstrate that there is least capacity of the knee in extension.* These observations show that the limb should not be kept in the extended position, if we wish to avoid anchylosis. Flexion at a right angle would be preferable; but as such a position would interfere with progression, the surgeon must be content with a moderately extended position, which is always the best. The treatment of anchylosis must be either slow and gradual traction, or violent rupture of the adhesions.

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* Bonnet: Traité de Thérapeutique des Maladies Articulaires, p. 390.

On Gouty Deposits we need to say no more than that numerous authors attest to the frequency of their occurrence in the knee joint. Cases are recorded by Cruveilhier and Fanconneau-Dufresne,[*] Todd,[†] Handfield Jones,[‡] &c., by which we learn that the earthy matter may be deposited free in the joint, may be imbedded in the cartilage, or may take the place of the cartilage entirely. The synovial membrane is red and thickened. In the extreme forms of this disease, it may be seen that the patella becomes adherent to the outer condyle of the femur by bony anchylosis.

Two preparations in the museum of the Royal College of Surgeons of England are quoted by Gurlt, to show the changes of form in the articular extremities of the femur and tibia produced by aneurism of the popliteal artery (Nos. 3098 and 3099). Osteoïd tumours about the knee have been recorded by J. Müller;[§] and specimens are preserved in the museum of the Royal College of Surgeons.

Cancerous tumours are related by Gerlach,[‖] and by R. W. Smith;[¶] and numerous specimens of the disease, arising both from the end of the femur or the articular extremity of the tibia, are contained in the pathological museums of London.

Holmes Coote.

REVIEW X.

1. *Cours de Philosophie Positive.* Par M. AUGUSTE COMTE, Tome troisième, contenant la Philosophie Chimique et la Philosophie Biologique.—*Paris*, 1838.

Positive Philosophy. By AUGUSTE COMTE. Vol. III.

2. *Système de Politique Positive.* Par AUGUSTE COMTE. Tome premier. Introduction Directe, Naturellement Synthétique, ou Biologie. Chapitre troisième.—*Paris*, 1851.

System of Positive Politics. By AUGUSTE COMTE. Vol. I. chap. iii.

3. *The Positive Philosophy of Auguste Comte.* By G. H. LEWES.
4. *The Correlation of the Physical Forces.* By PROFESSOR GROVE. Second Edition.—1850.
5. *The Correlation of the Vital and Physical Forces.* By PROFESSOR CARPENTER. (Philosophical Transactions for 1850.)
6. *Psychological Inquiries.* By Sir BENJAMIN BRODIE.
7. *Hints towards the Formation of a more comprehensive Theory of Life.* By SAMUEL TAYLOR COLERIDGE.

(*Concluded from No.* 30, *p.* 315.)

PHYSIOLOGY has for its object the scientific ordination of the phenomena and laws of life; and in the preceding article we have endeavoured to review some of the answers which have been given to the question, What are we to understand by Life? We have seen that it is not to be considered as either the cause or the effect of organization; but allusion was made* to a third mode in which the problem may be viewed, and it is to this that we wish now to direct attention.

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[*] Cruveilhier: Anat. Pathol., avec planches, livr. 4, pl. 3.

[†] Todd: London Medical Gazette, new series, vol. iv. p. 1073. 1847.

[‡] Handfield Jones: Ibid., vol. vii. p. 850. 1848.

[§] Müller's Archiv für Anat. und Phys., § 396. Jahrg. 1848.

[‖] Gerlach: Henle und Pfeuffer's Zeitschrift, Band vi.

[¶] R. W. Smith; Dublin Journal of Medical Science, vol. xxi. p. 314.

M. Comte, in adopting the definition of De Blainville ("le double mouvement intestin, à la fois général et continu, de composition et de décomposition,") remarks that something more explicit should be said with regard to the "*organisme* déterminé," and the "*milieu* convenable." This accomplished, we find the following statement:—"C'est de l'action réciproque de ces deux élémens que résultent inévitablement tous les divers phénomènes vitaux, non seulement animaux, comme on le pense d'ordinare, mais aussi organiques."†

Professor Grove, in one of the most beautiful contributions to scientific progress that our language possesses,‡ advanced a theory now very generally admitted to explain or express the varied relations of the several physical forces to each other. The theory embodied in this treatise is, that all the forces which are operative in the material world are but different manifestations of the same force; and that "neither, taken abstractedly, can be said to be the essential or proximate cause of the others; but that either may, as a force, produce, or be convertible into the others; thus, heat may mediately or immediately produce electricity, electricity may produce heat, and so of the rest." Mr. Grove goes on to state that the "same principles and mode of reasoning might be applied to the organic as well as the inorganic world, and that muscular force, animal and vegetable heat, &c., might, and at some time will, be shown to have similar definite correlations." This idea is illustrated by reference to Matteucci's experiments, showing a correlation between muscular action and electricity, and leading to the following sentence, "some definite polar condition exists or is induced in the nerves, to which electricity is correlated, and probably this polar condition constitutes nervous agency." (p. 90.)

The hint thus thrown out by Professor Grove has been ably followed up by Dr. Carpenter, who has not only applied the idea of correlation to the several manifestations of vital force in the living organism, but who has also shown that these occupy a similar relation to the ordinary physical forces of the inorganic world.§ Dr. Carpenter makes no attempt to explain the process, but he urges more strongly than Mr. Grove had done, the necessity for a material substratum as the agent in dynamic conversion The character of this substratum is shown to determine the direction in which the several forces are manifested. With regard to the vital forces themselves, Dr. Carpenter remarks, "that so close a mutual relation exists between all the vital forces, that they may be legitimately regarded as modes of one and the same force." This is abundantly illustrated by reference to the "relation of reciprocity" between the several manifestations of vital power; such as the absence of notable nervous or muscular force, or of chemical changes in the cells of the early embryo, the special direction of whose activity is towards multiplication;

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* British and Foreign Medico-Chirurgical Review, vol. xxx p. 307.

† Cours de Philosophie Positive, tom. i. p. 301. 1838.

‡ On the Correlation of the Physical Forces.

§ Philosophical Transactions. 1850.

the absence of secretion in ciliated cells; the disintegration and loss of vitality in those cells which give rise to mechanical agency, &c.

The correlation of the vital with the physical forces is, however, the important point of Dr. Carpenter's paper, in reference to the idea which we take of life. This may be expressed most accurately in his own words:

> "The vital force which causes the primordial cell of the germ first to multiply itself, and then to develop itself into a complex and extensive organism, was not either originally locked up in that single cell, nor was it latent in the materials which are progressively assimilated by itself, and its descendants; but it is directly and immediately supplied by the heat which is constantly operating upon it, and which is transformed into vital force by its passage through the organized fabric that manifests it. . . . . All the *forces* which are operating in producing the phenomena of life are in the first place derived from the inorganic universe, and are finally restored to it again. . . . . . And there is strong reason to believe that the entire amount of force of all kinds received by an animal during a given period, is given back by it during that period, his condition at the end of the time being the same as at the beginning. And all that has been expended in the building up of the organism is given back by its decay after death." (pp. 752—756.)

The position taken by M. Auguste Comte was very far in advance of that assumed by Bichat and his followers. De Blainville perceived clearly, and Comte recognised the value of the perception, that there is not the antagonism between the organism and its medium which Bichat's definition expressed; but that, on the other hand, the vitality of the organism is dependent upon its position in a suitable medium,* and that it may be defined to be the reciprocal action of these two elements.

It would be quite inconsistent with the general method of M. Comte to pass beyond this point. The *facts* of correlation were recognised by him in reference to the vital processes; but Professor Grove systematized and represented prominently the general laws of correlation, and advanced to the inference, that all physical forces were but manifestations of one general and universal force undergoing endless diversification in the mode of its development. Dr. Carpenter's further extension of the theory is especially important in its recognition of the part taken in this process of conversion by the material substratum—viz., the transference of physical into vital force, "by its passage through the organized fabric," &c.

Is this idea of life either satisfactory or final? Distinguishing carefully the general truth expressed by the term correlation, from the theory very commonly confounded with it—viz., that all the forces are identical; or even allowing that the latter, although undemonstrated, is exceedingly likely to be correct, is there not a simple evasion of the problem, rather than its complete solution?

A special structure, the organized body, or its microcosmic representative, the isolated cell, has the peculiar property of transforming inorganic physical forces, such as heat, light, &c., into organic vital forces. The differentia of the organized body from all others, or, in other words, that which constitutes its special vital property, is this peculiar power of transformation, and the direction in which it operates. It is, therefore, in the material substratum *per se* that the real vital force or property resides; since it is the special constitution of that substratum which determines the kind and direction of conversion which shall be exerted between the correlated forces. There is a broad distinction between the organic and inorganic substrata in this respect, that although many of the latter have a similar faculty of conversion—*i. e.*, that different forms of inorganic matter may effect a similar transformation of the physical forces, the former alone have the power of inducing a special kind or manifestation of force, the so-called vital. Regarding this property of conversion inherent in matter as a force (and it cannot be regarded otherwise), here is a force not correlated to the physical, and there are in the several forms of matter of which our world is composed, distinct series of forces not correlated to the other forces, as these are to one another, but characterized, on the contrary, by special or individual peculiarities. These peculiarities constitute the differentia of the several forms of matter, and are termed by Coleridge "the life" of each particular form, or its "tendency to individuation." But we have already stated reasons for objecting to this use of the word Life,* inasmuch as to make general, in denoting by the same phrase, that which is common to several different objects, when the real end that we have in view is to appreciate the nature of their difference, is only one means of avoiding, rather than the true method of meeting. the difficulty. However, the theory of identical force, pendent from the general doctrine of correlation as it is represented by Professors Grove and Carpenter, compels us to adopt the idea of Coleridge, although we may object to some of the phraseology he employs.

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* Comte uses the term "milieu," to express "l'ensemble total des circonstances extérieures, d'un genre quelconque."

The essential difference between the organized or living body, and the inorganized or inanimate body, is the peculiar relation which each occupies to the several physical and correlated forces, constituting the media by which they are surrounded; and if we were to define Life, it would be that peculiar property of certain (organized) bodies by which they are enabled to exert a special conversion and direction of the physical forces; this speciality of direction constituting the differentia of Life (as we commonly understand that word), from every other tendency to individuation. The secondarily induced vital forces, or the directions given by living bodies to the correlated physical forces, are threefold, producing vegetal, animal, and mental phenomena; and to these we shall immediately allude.

This idea of life may appear to resemble that which was prevalent before the doctrine of correlation was propounded by Mr. Grove, and applied to the phenomena of the organic world by Dr. Carpenter—viz., that of a vital force latent in the organism, and developed only by the action of certain physical agencies which were regarded as "vital stimuli." But it is essentially distinct from that mode of viewing the subject; it takes for granted the doctrine of correlation, or conversion of forces, as the best mode of expressing the relation which subsists between those forces and the processes of life; while it asserts, that the idea of life itself which we are compelled to adopt, is not that conveyed by the special direction of the forces, that is, by the peculiar character of their phenomenal manifestation, but is that of a special property in the organic body which determines this direction, and determines it so that they shall be manifested under three particular forms—vegetal, animal, and mental.

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* British and Foreign Medico-Chirurgical Review, vol. xxx p. 311.

Thus we recognise as *the* vital force, that special property of the organized body by which it so influences or directs the ordinary physical forces, that they produce a new series of phenomena, which we term vital. Dr. Carpenter employs the term vital force to denominate that peculiar agency which results from the direction given by the organism to the physical forces; we prefer considering as *the* vital force itself, that which determines this direction. It is this determining and directive power which differentiates the living from the dead, and which is therefore the essential of our idea of life. If it is correct to apply the term force to this peculiar property, it is a force not correlated to the others, but presenting specific differences in every organized and inorganized body; and thus the forces in operation in the material world may be divided into two great classes —first, those which are mutually correlated, the physical and vital in the sense of this word as employed by Dr. Carpenter; and, secondly, those which are not, but which constitute the specific differences between all material bodies, although they present generic and higher alliances.

M. Comte is perfectly justified in stating, that it is as absurd to ask the question, What is life?, as to ask, What is gravitation, electricity, or magnetism? The attempt to solve such problems has been discarded from the list of legitimate objects for scientific inquiry. It is important, however, to strip complicated phenomena of as many of their veils as possible; to analyse them as far as it is possible to carry their analysis in the present day; and by so doing, to express as many different phenomena as we can by one general term, and to refer as many varying processes as we can to the operations of one general force. Thus we are led to consider the various processes of the organic body as but different manifestations of one vital force; the several directions which these manifestations may take being determined by the nature of the material substrata in which it is developed, and by the special character of those physical (inorganic) forces which it is its property to direct.

"Vital force," says Mr. Lewes, "is one of the metaphysical entities;" and so it may have been, but it is not so necessarily. Occasional misconception may arise from the strictest phraseology. Even the positive philosopher is led into expressions of this kind,—"Physics have for their object . . . ." "The part Positivism is to play in the coming years of struggle . . . ." and so on; but no one will accuse Mr. Lewes of being in a "theological stage" because he impersonates Positivism and Physics; neither will the above expressions warrant the belief that he considers either the science of physics or the system of M. Auguste Comte as "one of the metaphysical entities." Mr. Lewes speaks of "*chemical* force,"—as, for example, "whenever we think we see chemical force inoperative, it is simply because the force is acting in another direction,"—but why he should reject the term or the idea of *vital* force is by no means apparent.

If there are physical forces, there are as certainly vital forces, and it matters little what terms are employed to denote their existence, provided the terms used have a definite meaning. Certain groups of varied phenomena may be formed from the fact of their presenting generic similitude amidst specific differences, and it is convenient to have some word to express this fact; there is, further, abundant evidence to show that these varied phenomena hold such a relation to one another that there is something common to them all; and it matters little whether we denominate these groups electric phenomena, vital phenomena, and so on, or whether we term them manifestations of electric force, of vital force, &c., although the latter phraseology appears to us the least objectionable, and an advance upon the former, inasmuch as it presents, what we cannot but deem a legitimate induction from the facts, the idea that material substrata and dynamic agencies are distinct existences, although we know nothing of any one of the former in complete separation from every one of the latter, or *vice versâ*. And if there were not scientific grounds for the adoption of this belief, we should feel disposed to give some weight to the almost universal consensus of human opinion upon the distinctness of matter and force; just as we should appeal to the general human consciousness, as the final test of truth, with regard to the great questions of the existence of self and personal identity, the existence of an external world, and of a Supreme Being.

Adopting the view of Life which may be gathered from the preceding pages, it is at once obvious wherein the difficulty of definition lies. We cannot but regard as manifestations of the same vital force very different phenomena—viz., those which we have already termed vegetal, animal, and mental; and it is plain that the definition (from phenomena alone) which refers only to one class must necessarily exclude the others. Thus De Blainville's expression may answer the purpose, if we are alluding to the simplest and most general processes of vegetal life; but such a definition does not and cannot include the functions of contractility and sensibility. Vegetality does not express the whole of our idea or of the facts of Life: the muscle and the nerve have this in common with the other tissues, but they possess further properties which are not hinted at in the definition of De Blainville. The vegetal integrity of the muscular and nervous structures is necessary for their proper functional activity; the partial disintegration of their tissues is one condition or source of their action, but the expression of the former does not include the latter.

The same remarks apply to mental processes in their relation to those of animal life. The only means by which we can become acquainted with the external world are those supplied by sensation; the only method by which we can act upon the external world is that furnished by muscular contraction; but perception, consciousness, emotion, and volition, are neither sensibility nor contractility; and if they are vital properties, and as such form part of our idea, and of the phenomena of life, the definition of life itself must be so general that it will include these with those other properties (vegetal and animal) to which allusion has been made already.

We revert, then, to the definition, or most general expression, of Life which we have given at page 110; and, before passing to a consideration of the encyclopædic position of the science of physiology or biology, have a few comments to make upon the special objects of that science in reference to the three great divisions or groups of phenomena with which it has to deal.

I. *Vegetal Life.*—The most complete idea which we have of this phase of life is that afforded by the simple cell, in its growth, production, reproduction, and decay. Some isolated cells possess more than the simple vegetal properties—such as those endowed with motility from changes in form—but to these we do not at present refer. The essential vegetal processes consist of—

a. Growth, or assimilation.
b. Disintegration, or molecular death, and the ultimate death (or defunction) of the individual cell.
c. Production, either of reproductive or non-reproductive particles.
d. Multiplication, by fission of the cell or its nucleus.

M. Comte finds in the life of the single cell a type, and the source of not only the functions of the individual man, but also of the *Grand Etre*—Humanity; and he opposes, in a dualistic form, the property which conserves the actual existence of the cell, and that which provides for its successive development. This belongs rather to the "poetry of science" than to its "positive" development.

a. With regard to the function of assimilation, which necessitates some amount of disintegration and consequent production, it must be observed, that so far as the life of the cell itself is concerned, this is directed simply to the growth of the individual cell; although the condition in which the latter may be placed with regard to other cells may determine the formation of a compound tissue. The osmotic force is one great agent in this process; but the essential fact of assimilation is the conversion by the organic body of material supplied *ab extra* into the structure of the organism itself, and the endowment of the new tissue, to a greater or less degree, with the properties of the original.

In the vegetable, assimilation is accomplished from inorganic materials; in the animal, organic compounds are required. The vegetable lives in dependence upon an inert medium; the animal needs, as its pabulum, matter which has already passed through modifications by vital changes. But the difference is one of degree rather than of kind; recent researches render it evident that there is not the distinctness formerly supposed to exist between organic and inorganic (chemical) compounds; and it must be remembered that the vegetable cannot live upon chemically simple bodies. Water, air, carbonic acid, ammonia, and the earthy salts from which the vegetable cells derive their support, and which they assimilate to their own form, by varied combinations, are already compound bodies; and there is no evidence to prove that the vegetable cells can make any structural use of the elements of which these are composed, if those elements are presented to them in a simple form.

b. Disintegration of the cell is one essential condition of its further action, and also of its assimilating function. The duration of vitality is dependent upon the rate of life, and the activity of its processes; and as there is a definite limit to the amount of vital action which each cell can perform, so this limit varies with different cells, and in dependence upon the circumstances in which they are placed. The essential fact of disintegration is the converse of assimilation—viz., the reduction of organic and vital compounds into inorganic and devitalized. It has been shown by Dr. Carpenter (as we have already stated), that the cell-force ceases to exist as a vital power in giving rise to mechanical agency; and thus the process of disintegration is seen to be not merely one of static alteration —i.e., of change in the material substrata—but of conversion of some one or more of the several manifestations of vital force, as inherent in the growing cell, into either the higher agencies of life, or into simply physical (inorganic) forces to which the vital are correlated.

c. With regard to the productive action of the cell, the simplest fact is that of secretion, either of nuclei, other cells, or of a special fluid (blastema). The cell—in addition to its properties of assimilation and disintegration, but probably dependent upon these reciprocal movements of composition and decomposition, and their relation to the osmotic force—has the power of forming other compounds than those which become part of its own structure, or which result from the analysis of those already in existence. These compounds may have a purely eliminative destination, or they may have to fulfil further purposes, in the economy of the individual, or in the reproduction of the species. It is by this productive property that those compounds are formed which assist in building up the fabric of the several organized tissues. The growth of a compound tissue is something more, therefore, than the simple assimilative function of its component cells; and although we must regard the fundamental vitality of such tissues as reducible into that of its individual elements, there is in its life as a whole the indication of a consensus, probably under the direction of a more generally acting manifestation of vital force, just as there is, in the compound body of man or of the higher animals, a consensus of action among its various complex organs, by which their several or individual directions of function are subordinated to that of the whole.

It is by this productive function that various secretions are formed, from the conjoined action and materials of the cells, and the pabulum upon which they live; and thus the processes of nutrition (i.e., of compound tissues, and not of individual cells) and secretion do not differ, except in form, and in the destination of their material results. The same remarks apply to the reproductive function generally; but there is this difference, that its products are possessed of independent vitality, or of a higher degree and force of individuation.

d. Multiplication of the cell by subdivision of its nucleus, and subsequently by separation of the cell-wall, cannot but be regarded as one of the essential properties of vegetal life, irreducible into the others. The precise mode in which this change takes place is not accurately determined, and it probably differs in different cells, but in the present state of science it cannot be placed in the same category with "multiplication," as the result of reproductive particles.

The science of physiology has to discover and systematise the laws under which these several processes take place; so that the problem of physiology being stated (as Comte has stated it)—given, the organ or the organic modification, to find the function or the act; and reciprocally ('Phil. Pos.,' p. 304), we have to take into account the twofold conditions of the organism and its medium. Thus, histology and chemistry, together with general physics, and the observation of vital phenomena, as they are presented naturally and artificially (by experiment), become the elements for investigation, and their development and application the means of progress. Different organs of the same body, and similar organs in bodies differing not only individually but sexually, and in respect of both age and species, have to be separately examined, as the basis of comparative physiology; to be developed simultaneously with, and in connexion with, the science of pathology.

Considering the vegetal functions generally, we have some remarks to make upon both the organism and its media, and this we shall do before advancing to the properties of *animal* life.

1. The Organism.—It is now generally held that the essential organic form, the essentially acting, living organism, is the cell; and that the other tissues which enter into the construction of complex organs are but means for supplying the requisite conditions for the vital operations of the cells. This is the case, for example, with regard to secreting organs, such as the liver and kidney, the bloodvessels and areolar tissues of which (although undergoing vegetal processes in their intimate structure for the maintenance of their due physical form), do but supply the conditions by which the cells may perform their functions.

There is nothing which we have yet discovered to account for the different properties of different cells—i.e., why one series should separate certain elements from the blood, and another series different elements. It is certain that the ordinary function may be modified by disease, and thus that the several organs of the same body may act, to a certain extent, vicariously: as when, for example, colouring matter of the bile is eliminated through the kidneys,[*] and urinary constituents are found in the serous fluids. The muscular tissues may become the seat of fatty degeneration; and many conversions take place which appear to show that the special properties of cells are variable within certain limits, although there is, normally, a definite function conferred upon each kind.

Many modifications are doubtless due to the changes of external conditions, such as the conditions of the blood, and of the organism generally, in respect of temperature, &c. (as displayed in the development of pigment subcutaneously, for example); but we are finally compelled to regard the special property of the cell as its peculiar vital endowment, and an endowment the presence and character of which we can estimate only by its phenomenal products. There is much difficulty in drawing the line between the results of the proper productive (vital) function, and those which arise from simple physical changes of transudation, or osmosis; and it is probable that further examination will narrow rather than widen the limits within which variations of cell-life are possible.

We are thus led, in conformity with the views already stated, to consider the vital force, in its vegetal direction, as the peculiar property of the cell, by which it is enabled to effect physical and chemical changes in its own structure, and to produce new physical and chemical compounds out of the materials which are supplied *ab extra*, and by itself. Assimilation and production are phenomenally physical processes and the forces directly or proximately engaged in their performance are physical, and are correlated to those which exist and operate in the inorganic world; but the directive power which influences these forces and their resulting processes is the real vital force, or property; it is not correlated to the ordinary physical forces in the sense in which they are related, *inter se;* but it is the special attribute and individuality of the organic body, controlling and directing them in subservience to its own ends.

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[*] It may be said with regard to this process, that the kidney does not separate the colouring matter of the bile from the blood until that colouring matter has been already eliminated by the hepatic cells, and re-absorbed from the hepatic ducts or gall-bladder as such; so that the kidney does not "secrete" biliary colouring matter, but simply allows its transudation as a physical process. This does not, we think, apply to all cases; nor does a similar mode of viewing the subject apply to the presence of urea in the serum of the cerebral ventricles.

2. With regard to the media by which the organism is surrounded, there are two classes of conditions requisite for the performance of its vital operations: first, the supply of appropriate materials for assimilation; and secondly, a certain definite relation to external physical forces. The materials vary with the nature of the organism, but present great general resemblances in the subdivisions of the animal and vegetable kingdoms respectively; and it is one object of the science of physiology to estimate their characters in reference to special groups of living beings.

The forces, or, as they were formerly termed, "vital stimuli," are manifold; and it is mainly to the researches of Professors Grove and Carpenter that we owe our more correct appreciation of their mode of operation. We have already given (p. 109) Dr. Carpenter's mode of representing the relationship, and have stated reasons for employing the term vital force in a somewhat different sense; the vegetal processes may be represented or expressed in physical and chemical terms, inasmuch as they result in physical and chemical changes; and in this sense the processes of assimilation, &c., are correlated to the physical processes; and the forces directly operative in the production of either group of phenomena are correlated, but they are of the same character in each; that which constitutes their differentia is the direction that they take, and the vital force of the organism is its special directive power.

Dr. Carpenter has drawn attention to the effects of heat and light in their influence upon the vegetal processes; and recent researches render it probable that the same processes hold equally definite relations to electricity. Mr. H. F. Baxter* has shown, that during secretion there is a polar difference between the secreted fluid and the venous blood flowing from the secreting organ. This has been demonstrated with regard to the biliary, urinary, and mammary secretions.

II. *Animal Life.*—We have now to consider the present position and objects of the science of physiology in relation to the processes and phenomena of animal life. They are twofold, and may be grouped under the terms "contractility," and "sensibility;" the former being the special property of muscular, the latter of nervous tissue.

The peculiar features of the animal functions, or the conditions under which they are exercised, and which distinguish them from those of vegetal life, are of two kinds.

a. They are intermittent: periods of activity alternating with periods of repose. Bichat enunciated this great distinction in the mode of their action from that of the vegetal processes.

b. They are influenced by habit, which (as Comte urges) constitutes the necessary basis of individual improvement;[*] and, as one link in the chain of improvement, not only of the individual, but of the species, it is found that the results of habit exhibit a tendency to hereditary persistence. It is the object of physiological science to investigate these animal properties in their relation to the vegetal functions and structure, to external conditions, to each other, and to the mental processes.

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* Philosophical Transactions, p. 279. 1852.

1. The relations between the animal properties and the vegetal are twofold; the animal are dependent upon the vegetal, and in their turn supply conditions for the activity and maintenance of the latter. But the first question which arises is—Is there any special form of organization with which the animal properties are exclusively associated? The answer to this question must be in the negative; for although in the higher forms of animal life there are peculiar tissues which manifest the properties of contractility and sensibility to a notable degree, yet even in these animals similar functions are observed (as in cilia, for example) in textures not possessed of any recognisable peculiarity of intimate structure; and there are to be observed distinct movements not only of the contents of a cell, for example, such as that described by Dr. Sharpey in the granular matter of spherical epithelium cells in the tail of the tadpole[†] (of the toad), but also distinct movements of the cell-wall itself. Dr. Carpenter has given an interesting account of these movements, as witnessed by himself in the Amæba (or Proteus, one of the infusorial Protozoa), but he remarks that "the change of form seems due rather to actions taking place in the interior of the cell, than to any irritability of the cell-wall."[‡] This conclusion appears warranted by the phenomena described; but the latter, taken in conjunction with those of ciliated cells, justify the general inference that the motility of the animal tissues is not necessarily dependent upon any special form of their structural development which we have hitherto recognised. And again, the pale fibres of the "organic muscles" differ very widely from those which constitute the tissue of the voluntary muscles.

Regarding sensibility as the property of receiving impressions which shall be the occasional causes of muscular movements, and of other phenomena connected with the processes of individual and relative vitality, we cannot deny this property to some of the lowest forms of animal life, in which the tissues are either wholly homogeneous, or which, at all events, present no traces of the nervous organization, as this is discovered in the higher animals. We have, therefore, to regard these animal properties as functions of the vital force inherent in the cell, and as constituting two of its special endowments; the speciality consisting in the direction which these cells can give to the several physical forces which may be brought to bear upon them.

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[*] Although not directly bearing upon our immediate object, we are sure that the following quotation must be of interest: "J'ai depuis longtemps établi qu'une telle aptitude à la reproduction spontanée des fonctions périodiques n'est point exclusivement propre aux êtres vivants La philosophie positive y voit un simple cas particulier de la loi universelle de persistance, dont la manifestation objective commence envers l'existence mathématique, où elle constitue la première loi du mouvement. En effet, la tendance à reproduire spontanément certains phénomènes vitaux, sans le concours de leurs sources primitives, est essentiellement analogue à la disposition qui fait partout persister dans un état quelconque après la cessation de l'impulsion correspondante. L'unique différence des deux propriétés résulte de la discontinuité des fonctions envers lesquelles la persistance universelle devient l'habitude spéciale. Or, cette transformation n'est point strictement bornée aux corps vivants. Elle se manifeste aussi en cosmologie, surtout quant aux phénomènes du son, dans les appareils dont l'action s'interrompt, et qui reproduisent mieux les effets assez réitérés. Mais cette aptitude ne peut alors être qu'ébauchée, et son vrai développement appartient nécessairement à la biologie comme l'ensemble des conditions correspondantes. Toutefois, il ne s'y réalise point envers la vie organique, où il y a persistance sans habitude, vu la continuité des fonctions." (Politique Positive, tom. i. p. 607.)

[†] Introduction to Dr. Quain's Anatomy, p. lvi.

[‡] Principles of Physiology, General and Comparative, p. 244.

With regard to the inter-relations of the animal and vegetal processes, Dr. Carpenter has shown that there is a correlation similar to that subsisting between the several physical forces. In their most perfect development—i.e., when concentrated as the special functions of determinate structures (the nervous and muscular tissues), it is essential for the performance of the animal functions that the vegetality of the tissues should be duly maintained; and although, for the maintenance of this integrity certain assimilative and destructive processes are at work, these latter do not proceed with the activity or energy which is observed in those cells whose special function is growth, or productive action. Some disintegration of the tissues takes place, however, as the essential (physical) condition of the animal functions. Dr. Carpenter says "the cell-force ceases to exist as a vital power in giving rise to mechanical agency;" or, in other words, there is a re-conversion of the vital force (originated by the transformation of non-vital) into the physical.

Professor Graham, in the Bakerian Lecture, "On Osmotic Force," has thus stated the existence of another series of correlations:

> "In osmose there is, further, a remarkably direct substitution of one of the great forces of nature, by its equivalent in another force; the conversion, as it may be said, of chemical affinity into mechanical power. . . . . . . May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link which certainly intervenes between muscular movement and chemical decomposition?"

The same is true with regard to the nervous functions, a certain disintegration or disorganization of the tissue being an essential condition of their activity; but, in accordance with the principles already stated, we should make some alterations in the phraseology employed. It appears to us, that it is by appreciating this due relation between vegetal and animal processes that we may solve the question so often discussed, of the existence or non-existence of so-called "dynamic diseases," or "neuroses." If by the necessary dependence of morbid function upon morbid structure, it is intended that the former can only occur in connexion with certain pathologico-anatomical conditions which we may recognise directly by the senses, the *onus probandi* lies upon those who make the assertion, and it remains for them to show the nature of relation between the two classes of phenomena; but, in the meantime, we must fully believe in the existence of functional changes apart from any such coarse alterations in the structure. If, however, it is intended that certain changes in the intimate vegetal processes must exist to account for the morbid animal phenomena, we assent to this proposition, considering the deranged function to be dynamic in precisely the same sense that we regard its healthy exercise to be dynamic. We find, further, in the same correlation one mode of accounting for the differences observed amongst the symptoms produced by diseases presenting coarse structural similarities, and *vice* *versâ* for the resemblances between the symptoms of disease which have different anatomical bases.

But the animal and vegetal functions are *mutually* dependent, and the latter are not duly performed unless the former are properly exercised. Muscles and nerves undergo degeneration if they remain inactive. Of this fact there is such abundant evidence, that it needs no further comment. One important direction, not of the animal action itself, but of its resultant effect, is towards supplying the necessary conditions of vegetal life. The contractility and sensibility of the tissues are great means for carrying on the processes of nutrition, secretion, &c., in the complicated organisms. M. Comte has made much use of this relationship in the development of his sociologic science as the outgrowth of biology; and also in the construction of his encyclopædic scale. Thus:

> "The objective subordination of animality to vegetality determines the two general attributes which characterize it directly. In fact, the obligation of self-nutrition supposes in living bodies a faculty of discrimination, and a power of prehension. Thus sensibility and contractility become the necessary conditions of the mode of alimentation which defines animality. Without this double aptitude to recognise and to modify external bodies, animal existence would be directly contradictory. Through its instrumentality the living being, hitherto entirely solitary, naturally opens up habitual relations with all that surrounds it. But this life of relation at first presents a character which is purely individual, as having for its essential end the life of nutrition, which remains the fundamental attribute of the ensemble of organized beings." ('Pol. Pos.' p. 598.)
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> "The various apparatuses of animal life, either sensitive and locomotive, or intellectual and moral, are exercised habitually only in order to preserve the fundamental vitality" (vegetality). (Ibid. p. 610.)

We cannot but regard this latter statement as eminently unsatisfactory. There is an interdependence of the animal and vegetal processes, as we have already pointed out; and although the former are not so general as the latter, neither can be considered as the cause or effect of the other, as either its first cause or its end. There are purposes accomplished by the life of individuals, and of classes of animals, which have no definite relation to the well-being of those animals themselves, but which constitute them essential portions of the one great plan of creation in which they are placed. Many of these purposes are readily discoverable, and others will in all probability be known; but further than this, as M. Comte himself remarks, "the moderate exercise of the animal faculties procures *per se* a certain special and immediate satisfaction;" and we cannot but look upon this as one element in the direction of function, and one end to be attained. The proposition that "fundamental vitality" is the great end of all forms of existence, or that it is the direction which all life takes, appears to us belied by every organization possessed of more than simple vegetal properties. With regard to the ultimate direction of "intellectual and moral" life, it is as old as Job, or as old as humanity itself, that "all that a man hath he will give for his life," although the history of succeeding generations, and the heroism of "the noble army of martyrs," reveal that some men have more faith in, and concern for, the "unseen and the eternal," than for "the seen and temporal," and consider their real life to be something above, and unaffected by, the so-called accidents of time and sense; but the mere statement that all moral and intellectual activity is exercised habitually only for the conservation of vegetal existence, is an insult to humanity, fallen as it is, and is a most imperfect and erroneous representation of the facts. M. Comte's own unhappy experience of the results of his labours, in their bearing upon his nutritive existence, must teach him, that his ponderous tomes of 'Positive Philosophy,' and his ecstasies of developmental thought, are not all bent upon the conservation of his fundamental vegetality. Looking upon all the varied forms of life by which we are surrounded, their beauty and their joy; from the flowers that "open their fan-like leaves to the light," and

"The plumed insects, swift and free,
Like golden boats on a sunny sea,"

to the young lion in its play, with the consciousness of power, and in its repose, as it it lies basking in the sunshine; we cannot but feel that we would accept the words, "the trees of the fields clap their hands," as the expression of more scientific truth than the miserable statement,—that all life is directed to the maintenance of mere physical conditions.

2. The relations of the animal properties to physical forces and conditions form parts of the objects for physiological investigation. Dr. Carpenter has pointed out the correlation which subsists between the forces, and to these we shall not further allude, except with regard to the dependence of contractility upon stimuli for its occasional manifestation. Doubtless, the stimulus is essential for the production of contraction in the voluntary muscles; but great difficulty has arisen in determining the cause of rhythmical contraction in the heart: one supposition after another has been laid aside, and at last Dr. Carpenter has come, *per viam exclusionis*, to the conclusion, that these movements are "an expression of the peculiar vital endowments of its muscular tissue . . . . just as ciliary movement is in cells of one class, and secreting action in those of another."* This view Dr. Carpenter supports, by the fact of contraction in the embryonic heart, the analogous contractions of the uterus, the impossibility of discovering any occasional causes of contraction, and the recognition of movements in cells of various kinds; together with the general doctrine, that "the contraction of *any* muscle upon the application of a stimulus, must be attributed to an exercise of vital force engendered by previous acts of nutrition;" the difference with regard to the heart being, that its special vegetal existence supplies this alternation of contraction and relaxation as one of its peculiar vital properties. This view we may adopt provisionally, at all events; and necessitating as it does an important modification of the general laws of muscular activity, it is evident that the relations of this vital property to the physical forces *ab extra* require re-examination.

As forming part of this branch of physiological inquiry, we have the phenomena of reflex action, and those of sensation in dependence upon external impressions. It is quite unnecessary to make any comments upon the former, although some of the passages in Mr. Lewes's work are eminently defective upon this point. With regard to sensibility, it is now recognised that the real condition which becomes the phenomenon of consciousness, is that of the central nervous system, and not the properties of the external world, which operate merely by inducing some change in the internal organs. Thus special nerves have the power of inducing but one kind of phenomenon in the consciousness, and this they invariably accomplish, no matter what kind of impression is made upon them.

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* Principles of Human Physiology, fourth edition, p. 475.

With regard to the material conditions necessary for animal activity, there is evidence to show that the supply of blood must be maintained, and that it must be aërated, or oxygenated. M. Brown Séquard's experiments have confirmed most beautifully the truth of this proposition, which had been understood before, though by no means so fully.

3. The relations of sensibility to contractility.—There are many movements of more or less frequent occurrence in the animal body, which are not occasioned by the will, and which are not simply reflex actions, inasmuch as they are accompanied by sensation, the existence of the latter being essential to their production. These are termed the consensual, or sensori-motor; and in accordance with the results of experimental inquiries made by Flourens, Magendie, Longet, Hertwig, and others, they are considered to be the product of a chain of ganglia existing at the base of the brain. Our information with regard to these phenomena has been systematized by Dr. Carpenter,[*] who has also described their pathological relations, and has shown that the commonly termed "instinctive actions" belong to the same category.

Dr. Marshall Hall recognised the dependence of instinctive actions upon sensation and a reflective action.[†] Mr. Lewes speaks of "instinct as a rudimentary reason;" but the simply reflective function of the spinal cord might be so denominated with equal justice. From old-fashioned prejudices, we have been disposed to deny the existence of reason in the lower animals, on account of ulterior questions which might arise; but the Huntsman in Schiller's play has rebuked us,[‡] and the application of just observation must at once show that the two faculties are not graduated the one into the other, but that they are developed distinctly, and are antithetic rather than analogous.

In the 'Psychological Inquiries' of Sir Benjamin Brodie, this distinctness between reason and instinct is fully appreciated, and it is at once allowed that the higher members of the brute creation possess the former. Sir Benjamin adopts the views propounded by Dr. Carpenter, and based upon the observations of Flourens and other physiologists, that—

> "Those bodies, situated at the base of the brain, to which in the human subject we give the names of medulla oblongata, cerebellum, thalami, corpora striata, and tubercula quadrigemina, and the parts corresponding to these in other vertebrate animals, are connected with that class of phenomena which belong to the animal appetites and instincts." (p. 172.)

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[*] Principles of Physiology, General and Comparative, p. 1085 et seq.

[†] Observations in Medicine. Second Series.

[‡] "Ein unvernünft'ges Vieh!
Ist bald gesagt. Das Thier hat auch Vernunft;
Das wissen *wir*, die wir die Gemsen jager:
Die stellen klug, wo sie zur Weide gehn,
'Ne Vorhut aus, die spitzt das Ohr und warnet
Mit heller Pfeife, wenn der Jäger naht."—*Tell.*

He also alludes to an important element in the perfectionizing of instinctive acts in the lower animals, although the language employed is not so distinctive as could be wished. Thus:

> "In birds, the eye is a more complicated, and evidently a more perfect organ than it is in our own species, or in the mammalia generally. The eye of an eagle is nearly as large as that of an elephant; he has a wider range of vision. . . . . In this respect, he has means of obtaining knowledge which men do not possess, and so far has an advantage over us. . . . . There are many other animals which have organs of sense more perfect . . . . than they are in the human race, and whatever that difference may be, it must lead to a like result, by modifying their perceptions, and, if I may be allowed the expression, their notions of things external to themselves" (p. 180.)

We cannot but think that these modifications of the sensory organs are mainly influential in giving an avolitional direction to the muscular movements; and that "knowledge," "perception," and "notions," scarcely enter into the chain of causation. The final conclusion that "Certain feelings exist which lead to the voluntary exercise of certain muscles, and to the performance of certain acts, without any reference at the time to the ultimate object for which these acts are required" (p. 186), may apply to some instinctive functions, but certainly not to all; as in some there is no evidence of "feelings," except as simple sensations (which is not the ordinary limitation of the word), nor of "voluntary" effort.

The science of physiology has to take into consideration the modifications which external circumstances, habits, and hereditary antecedents may exert upon the connate instincts of the animals. There are some interesting remarks upon this subject in the 'Psychological Inquiries;' although we do not see sufficient reason for thinking with their author, that "The conversion of an acquired habit into an instinct is attended with some actual change in the organization of the brain." New formulæ are created for the combination of muscular movements, and this process takes place in those higher animals possessed of some reason and volition; so that the combinations which at first required an effort of the will under the direction of sensational impressions, are, by the simple fact of repeated association with the latter, rendered so easy of production, that there is no further necessity for volitional interference.

4. The relation between the animal and mental processes is also a legitimate object for physiological investigation. From this point of view we have to determine, on the one hand, the part taken by sensibility in the acquirement of new ideas with regard to external truth—i.e., the relation of sensation to perception: we have, on the other hand, to study and ascertain the part taken by contractility in the process by which volition becomes effective upon surrounding objects.

Whatever may be the view which we take of the nature of Mind, and its operations, we cannot fail to recognise the fact, that sensibility and motility constitute its points of contact with the material and external world. The mode in which this contact is established, and the general relations subsisting between the processes of animal and mental life, form, however, part of a distinct field for inquiry—viz., the science of psychology; and the interdependence of this science and physiology we shall consider at some future period.

J. Russell Reynolds.

REVIEW XI.

Malaria und Ozon, oder Untersuchung der Frage, inwiefern stehende Wässer durch Gasexhalationen oder Miasmen der menschlichen Gesundheit nachtheilig seien,—mit besonderem Hinblick auf den, neben dem Hospitale Zum Heiligen Geist in Frankfurt a. M. belegenen Weiher. Von Dr. THEODOR CLEMENS in Frankfurt a. M. ('HENKE's Zeitschrift für die Staatsarzneikunde, fortgesetzt von Dr. Fr. J. BEHREND, prakt. Arzte in Berlin, Mitglied mehrerer gelehrter Gesellschaften.' *Erlangen*, 1853. Heft i. p. 1.)

Malaria and Ozone; or, Examination of the Question, How far Stagnant Waters are, through Gaseous Exhalations or Miasmata, Injurious to Human Health,—with especial reference to the fish-ponds situated near the Hospital of the Holy Ghost, at Frankfort on the Maine. By Dr. THEODORE CLEMENS, of that city. ('HENKE's Journal of States-Medicine.' 1853. Page 1.)

IN a classically and almost poetically written exordium the author states, that having been for many years occupied with the examination of media of atmospheric and telluric origin hostile to organisms, he could not, as resident and house physician of the institution, regard with indifference the sheet of water, presenting a surface of 79,680 square feet, which washes the northern wing of the Hospital of the Holy Ghost. When in 1827 a grant of the site on which the hospital is built was made by the Senate of Frankfort, various opinions were expressed in defence of, and against, the salubrity of the place, and some even spoke of the necessity of drying up the pond adjoining the building. It was, therefore, very willingly that Dr. Clemens undertook the laborious task of investigating, both qualitatively and quantitatively, the chemical constitution of the gaseous exhalations of the entire surface of the water, at various hours of the day and seasons of the year. His investigations, much facilitated by the practice he had acquired during his previous researches of a perfectly similar nature, were continued with the greatest accuracy during five quarterly periods, and he felt himself the more rewarded for his exertions, as he believed he had obtained the scarcely looked-for result of being enabled to come forward, on a scientific basis, as a defender of the locality. His labours were extensive enough, when collated with the results of his previous studies, to justify him in taking a deeper and more general survey of the reciprocal actions of the atmosphere and water surfaces, on which account he has partially incorporated with his recent special inferences, so far as they accorded with them, the conclusions deduced from his former general experiments in this department of science.

"The hospital is situated at the eastern extremity of Frankfort, with its front looking westerly towards a wide street, and southerly towards an open space, which, adjoining the municipal library, is touched by the river Maine. On the east it is bounded by the pleasure-grounds which surround the entire city, and on this open side it looks towards a plain, which, extending in the direction of Hanau, forms the flat shore of the river. The northern wing of the house is almost washed by a reservoir situated in the pleasure-grounds, and presenting a superficial extent of 79,680 square feet. The evaporation of this water is wafted by the north and north-east winds, which very frequently prevail in Frankfort, into the windows of the adjoining wing of the institution. In this wing my residence was situated, with its windows looking directly to the sheet of water; only four ward windows open on the same side, the others belonging to the apartments of officials, or to rooms applied to other purposes. The reservoir, situated in a sort of ravine, is of various depths (from five to ten, and from sixteen to twenty feet). The level changes somewhat according to the season of the year and the quantity of rain. The supply, which consists chiefly of spring water, is variable. In addition, the levels of the reservoir and of a trench from a marshy meadow continually counterpoise one another. The discharge, which passes under the hospital, is small, but the entire basin can be emptied in a few days by means of a sluice and a canal, which likewise passes under the building. The surface of the water has usually a perfectly stagnant appearance. The colour varies, according to the season of the year, from a dull sea-green and greenish-brown to dark brown; sometimes, however, from an overflow of the Maine, which in that case joins the marsh, it acquires a muddy appearance, and may rise considerably. The water is often so clear, that we can see into it to the depth of a foot, [!] particularly when it has been long without having been disturbed by a fall of rain. In very hot summers a slimy pellicle forms on the surface of the water adjoining its place of exit, and often covers nearly a third of the pond, and is then gradually removed by sudden and violent showers of rain. The mud which settles on the bottom, formed by the washings of rain, the deposit from the water, atmospheric dust, the excrement of fish, &c., is removed from time to time by emptying the reservoir." (p. 3.)

The author having given this full description of a locality which, in our judgment, should be about the worst that could be selected for the erection of an hospital, proceeds to explain his mode of conducting the inquiry he had proposed to himself. The ordinary methods of examining the gaseous exhalations of a sheet of water, by stirring the bottom with a stick, and receiving the air bubbles which are then formed, or by collecting those which spontaneously rise, are not to be relied on, as every body of water which has not a rapid supply and discharge, deposits a mud, composed in part of the dust wafted into it, and consequently, the gases so collected will often be but accidental admixtures, and will not fairly represent the exhalation of the entire surface. It is in the *exhalation* of the water, a process which we ordinarily designate by the familiar term "evaporation," that we are to seek the solution of the question of the reciprocal action between atmosphere and water surfaces. Do we by the word "evaporation" understand a peculiar chemical decomposition of water into its constituents, excited by contact with the air, or a mere taking up by the atmosphere of infinitely minute aqueous particles? There is evidently a great difference between the evaporation of water as in boiling, and its galvanic resolution into its constituents, and yet there may be other processes which withdraw its particles from a body of water to give them to the air. Such doubts induced the author to adopt the following mode of examination:—After having collected and carefully analysed the air-bubbles rising from the water to be examined, he filled large accurately-measured vessels with the water, separated, by means of a fine sieve, from its vegetable particles, while another was filled with water not so purified, and a third, again, with purified water, but with the addition of some of the mud found in the water. The glasses were so arranged that the gas which collected above the surface of the water was received in a small apparatus adapted to each. They were now exposed to the sun, as long as the greater portion of the surface of the pond received its rays, and, after a given period, the gas which had collected above the water was chemically examined, both qualitatively and quantitatively. The water itself was microscopically examined about every eighth day, and was occasionally subjected to a rough chemical investigation. The water in glass No. 1 could, during the first days, continue its *vital process* unimpaired; as a commencing so-called putrefactive process might, without prejudicing the investigation, set in in the almost stagnant water of the reservoir, it occurred to the author to examine the gases developed during the gradual putrefaction of water. Glass No 2 contained all the organic admixtures of the water without the mud, and must, consequently, present the characters of the proper water of the reservoir, with the exclusion of the gases rising from the mud. The water in the receiver No. 3, lastly, showed the result of experiment No. 2 plus the exhalation from the mud. The water of each vessel was, previously to filling it, examined chemically and microscopically, weighed, and the day and hour of filling were exactly marked on it. In addition, fresh ozonometers were frequently fixed in different places and at different periods of the day, at various heights above the surface of the water of the reservoir to be examined, in order, through the general reaction of the watery exhalation on atmospheric ozone, to obtain a definite, or perhaps even an universal, point of reference. With slight modifications, such examinations of various reservoirs in the neighbourhood of Frankfort were for a long time carried on. All these reservoirs were of evil report, and were accused of generating the dreaded marsh miasmata. The author examined the strata of air above the surface of the water for atmospheric ozone, because his experiments, carried on during many years in reference to this still enigmatical body, led him to believe that he could employ the fact of its being destroyed by compounds of hydrogen, miasmata, &c., as a test of the presence of such like poisonous marshy emanations. On the other hand, it had not escaped him, in former researches on the respiration of plants, and especially on the properties of the oxygen set free by them under the influence of light, that, under certain circumstances, a mysterious presence of ozone may be caused by the decomposition of the water, (?) of vegetable organisms, a circumstance which might prove in many ways characteristic, in marshes pervaded by vegetable and animal organisms. In former and steadily-continued investigations on aërial ozone, Dr. Clemens had, in his measurements of that body in a very marshy situation, obtained such considerable local variations, that at the time he took up his present special subject of inquiry, he had begun to think a connexion might be traced between marshy malaria and the formation or destruction of ozone.*

The charges latterly brought against the exhalation from the water adjoining the hospital were of two kinds—first, that the smell perceptible in its immediate neighbourhood on sultry summer evenings indicated that the vapour rising from it must be prejudicial to health in general. Secondly, the increase of intermittent fever, and the unusual and obstinate course, and the frequent relapses of the cases of that disease treated in the hospital were attributed to it. The steadily increasing prevalence of those fevers, during the last few years, in and around Frankfort, supported this view. Cases occurred at times in which patients admitted into the hospital on account of trifling illness, were suddenly and unexpectedly attacked with a paroxysm of intermittent fever, which was often followed by others; and in some instances, those who came in for a slight gastric affection, were sent home with a large ague-cake in their abdomen. Those patients who were, by a considerable expenditure of quina, sufficiently restored to leave the house, soon returned, in consequence of relapse, with an earnest petition for a fresh supply of the "fever drops;" but it was found that they were better, were more quickly cured, and were much less subject to relapse, when treated as externes, than when they were repeatedly taken into hospital. Notwithstanding these appearances, the author is more inclined to attribute the prevalence of the fever in the hospital to the crowding together of such patients, and the consequent bad ventilation of its wards, than to the proximity of the turbid sheet of water; and he thinks that the endemic occurrence of intermittents in watery districts in general is to be attributed, not to the presence of so much stagnant and flowing water, but to totally different terrestrial influences. In support of this view, he adduces the fact, that the state of health in all the other buildings adjoining the reservoir is very good, and that intermittent fever is entirely absent from them. Also, that no special forms of disease prevail in the district, and that a few years ago intermittent fever was of rare occurrence in Frankfort, and came under treatment in the hospital much less frequently than at present; and that if the water were the cause of its origin in the house, it should not have been deferred for a period of nearly twelve years.

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* The subject of ozone will be found alluded to in several of the previous volumes of this journal—viz., vol. i. p. 542; vol. iii. p. 529; vol. vii. p. 540; vol. viii. p. 460; vol. ix. p. 520; vol. x. p. 254. We may here briefly remind our readers that ozone is now considered to be electrified oxygen, that it blues paper impregnated with starch and iodide of potassium, and that the different degrees of intensity of the blue coloration, corresponding to the different amounts of ozone diffused through the atmosphere, constitute the ozonometric scale of the discoverer, M. Schönbein.

> "With regard to the hospital gangrene which broke out some years ago, its appearance in a house almost newly built, and kept in a state of exemplary cleanliness and order, was a circumstance strongly calculated to fix the attention of the physicians. It afforded an illustration of the fact, that notwithstanding the greatest care and cleanliness, and the most judicious treatment, a miasma may arise which, then feeding on itself, may so actively and obstinately continue its destructive course. While in a short time the simplest wounds began to assume an ulcerous, granular surface, and to increase in size, leech-bites and cupping scarifications put on the same character. This degeneration of wounds presented a very different degree of intensity in different wards, and threatened shortly to invade the whole house, when the calamity was, through energetic interference, seasonably subdued. This dreaded malady, which may indeed owe its origin to a peculiar vitiation of the atmosphere, even at that time attracted my attention to the nature of the air of the hospital and its neighbourhood, and I shall at a future period give an account of my experience acquired during the epidemic, in connexion with the general observations with which I purpose to follow up the more special subjects of this paper." (p. 11.)

1. *The Reaction of the Aqueous Exhalation on Atmospheric Ozone* is the first point to which the author directs his attention; he had already instituted extended observations on the gases which destroy atmospheric ozone, and had found that most of those injurious to the lungs of man

(and of the lower animals), and particularly such as are more frequently spontaneously developed in nature—viz., sulphuretted, carburetted, and phosphuretted hydrogen, and similar emanations—quickly, and according to the intensity of their action completely, annihilate atmospheric ozone.* Now, if a sheet of water conveys to the atmosphere a quantity of irrespirable or directly poisonous gases (as for example, sulphuretted hydrogen, carburetted hydrogen, carbonic acid, &c.) sufficient to prove injurious to the health, these emanations must produce a gaseous mixture which, entering into reciprocal action with the atmosphere, must in an especial manner modify the ozone contained in it. This general proposition the author adopts as his axiom, as the basis of his experiments. Dr. Clemens' mode of proceeding was as follows:

The reaction indicated on the ozonometers was accurately noted in a journal arranged for the purpose, and at the side were given the results of his chemical examinations of the water drawn from the reservoir, so that he could with precision compare the weekly variations of the ozone obtained above the surface of the water with the gases developed in the glass vessels filled in the same week; in this manner his observations were extended by noting the reciprocal action between the atmosphere and the water surface. His mutually controlling ozonometers were disposed in the following manner: all were changed every twenty-four hours, and while one was constantly fixed two feet above the surface of the water, two others were at the same time arranged twenty feet above it, on two different banks of the reservoir; four others were also at the same time placed so that, being removed from the influence of the sheet of water, they should indicate the ozone of the atmosphere. These four were thus disposed: one two feet above the ground (free from vegetation, and covered with gravelly sand), the second about seventy feet above the earth (the distant water-surfaces being taken as the level), facing the wind blowing at the time; and the third at the same height, looking with the wind. A fourth ozonometer, lastly, was fastened in the middle of a ward, generally between the beds of two patients in intermittent fever. To facilitate comparison, he created a scale representing seven shades of colour, (0 being blank), 1 corresponding to the first trace of reaction, and 7 to the greatest intensity of colour observed with the ozonometer. These shades of colour are easily obtained by developing some ozone in a little balloon, and keeping the prepared slips of paper* exposed during different lengths of time to the vapours, until we have obtained the degrees of colour, which are then arranged in a series, and enclosed between two accurately fitting glass plates. When the ozonometers are brought in, each is moistened and applied to the scale, and its degree of reaction is numbered and entered in the journal. Thus, on the 6th of July, Dr. Clemens obtained the following reactions:

	6th July.		7th July.
Ozone, 70 feet high, in the wind	4°	...	5°
„ „ sheltered from the wind	3°	...	4°
„ 20 feet high, over the water	4°	...	4°
„ 2 feet high, over the water.	4°	...	4°
„ „ over the earth	2°	...	2°
„ in the ward	0°	...	0°

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* Henle und Pfeuffer's Zeitschrift, Band vii. Heft 2, p. 237.

* See note, p. 125.

Being accustomed to find the ozonometer in the wards indicating 0°, or at most 1°, he was on one occasion surprised to observe it representing 3°; on investigating the cause, he discovered that a bottle containing a strong solution of chlorine had, through carelessness, been left open in the room, a source of fallacy he thenceforward guarded against. The general results he obtained by the mode of observation we have just described, may be briefly stated as follows: The reciprocal variations of the ozone reaction in summer were such that the maximum degree always fluctuated between seventy feet high in the wind, and two feet over the water; the minimum, on the contrary, as was to be expected, was invariably found in the wards. This state of things was most striking in the warmer summer months, at which periods the total sum of the degrees of the ozonometer situated two feet above the water considerably exceeded that placed in the wind at an elevation of seventy feet. The author gives as a specimen a fragment of his ozone calendar, extending from the 10th to the 22nd of June. The first column shows the date, the second the height of the barometer at zero of Reaumur's thermometric scale,* the third the height of the thermometer, the fourth the direction of the wind, the fifth the description of the weather, the following six the degrees of ozone under the several circumstances in the scheme given above, and the twelfth the number of patients in the ward.

The *total* numbers of the degrees of ozone during the thirteen days were as follows:

At an elevation of 70 feet in the wind	40°
At an elevation of 70 feet sheltered from the wind	35°
Twenty feet above the water	33°
Two feet above the water	47°
At an elevation of two feet above the earth	9°
In the ward	1°

It is worthy of note, that the day of greatest ozone is that of the occurrence of a thunderstorm.

The author, having already examined the exhalations of so many marshes, and having obtained so frequently, and in such remarkable amount, the various gases capable of destroying ozone, was surprised to find the quantity of this agent at two feet above the water balancing, and often exceeding, that contained in the atmosphere; it was moreover natural to expect that the organic substances liable to decomposition which are mixed with all stagnant water, should, in the warmer summer months, have developed gases enough to act destructively on the ozone, and so to affect the lower strata of the air which were in contact with the aqueous surface. Dr. Clemens, therefore, at first suspected that the sheet of water he was examining might possess some hidden peculiarity, and he consequently extended his subsequent chemical investigations far beyond what he had originally intended.

Having, however, established beyond doubt, in the lower strata of air, in which he had hitherto been accustomed to find it either diminished or absent, the existence of ozone—a body usually met with in increased quantity only in a pure and elevated atmosphere, and the presence of which negatived that of marshy gases—he puts the question, "In what relation does this development of ozone over the sheet of water under consideration stand to the salubrity of the surrounding air?" A question which prepares the way for the next inquiry, "Can the manifestly increased development of ozone over the surface of the water be in itself, apart from its mode of origin, a disease-producing agent?" Approaching these questions, he enumerates the properties of ozone, its power of gradually destroying organic beings, and its capability of oxidizing even noble metals without elevation of temperature, and of slowly destroying them. It appears to be an active excitant of combustion, and possesses in this respect, as in many others, properties corresponding to the effects of pure oxygen. It takes a position next to chlorine in its irritating action on the bronchial mucous membrane; and equals, if it does not excel that gas, in its disinfecting powers. Very small quantities of the exhalations which we usually comprehend under the term "miasmata," are capable of destroying or modifying the amount of ozone in the atmosphere with which it comes into contact—as in churchyards, anatomical theatres, the neighbourhood of cesspools, dung-heaps, stables, sulphur-springs, in rooms much frequented and closed against the access of fresh air (as schools, theatres, &c.).

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* The barometric measurements are given in *lines*; as the Bavarian foot is equal to 11·667 English inches, the highest number given in the table, 337‴2, will correspond to 28″9 of our scale; the lowest, 329‴7, to 28·259 English inches. The weather was predominantly wet and stormy during the ten days.

From the author's results it appears that the quantity of ozone indicated by the instrument placed two or three feet above the surface of the water, was constantly in excess of that present at an elevation of twenty feet, while the latter differed but little from that shown by the ozonometer at an elevation of seventy feet, sheltered from the wind; this makes the amount developed in the lowest strata still more remarkable, and the latter presents the strongest contrast to that in the ward; in the fragment of the ozone journal given above, these quantities are as 47 to 1. Hence we may safely infer, *that even if we assume as an effect of this development of ozone a possibly injurious influence on the health, such an effect on the air of the neighbourhood of the sheet of water in question is completely refuted.* The second question, "Can the increased ozone of that aqueous exhalation constitute a morbific agent?" is answered by the author with a direct negative. He would desire every hospital to possess such a ventilation as should ensure in each ward at least from one to two degrees of ozone in twenty-four hours, an atmospheric constitution which he considers would be sufficient to prevent the occurrence of hospital gangrene. At the same time the disinfection, by means of chlorine fumigation, of air containing injurious matters suspended in it, is not to be neglected, for it is probable that the latter are as fully destroyed when the air acts on the ozonometer by reason of the chlorine present in it, as when such a reaction is owing to the natural ozone of the atmosphere. But if an atmosphere exhibiting not more than 4° of ozone in twenty-four hours maintains an active metamorphosis of tissue, one indicating 7° in the same time may, by over stimulation, produce disease; and one showing 7° in a few minutes may, in like manner, in time, produce death, just as an excess of oxygen would be injurious.

The effects of atmospheric ozone are visible in the organic systems of the animals inhabiting the several strata of our aërial ocean. The author contrasts in this respect the energetic circulation, the arterial development, the warm blood, and rapid metamorphosis of tissue of birds frequenting the higher atmospheric regions, where our ozonometer would in a few hours indicate 7°, or perhaps more, with the preponderance of venous life, and incredible slowness of metamorphosis, of the toad and the salamander, which in their muddy clefts, where scarcely a trace of ozone is discoverable, can, without injury, long dispense with air, light, and food. A very vigorous reciprocal action appears to take place between the higher and purer atmospheric strata, and the lower ones which are rendered impure by the animal and vegetable vital processes going on in them, and explains the variations that have been observed in reference to ozone.

2. *Amount of Ozone in Aqueous Exhalations in general.*—To ascertain whether the development of ozone alluded to is a peculiarity of the sheet of water in question, or whether it is common to all standing waters, Dr. Clemens instituted similar experiments on several pools in the neighbourhood of Frankfort, the results of which showed a great variety in the ozone reaction over different sheets of water.

3. *Reciprocal Action of the Bottom and the Sheet of Water.*—The next point to which the author directs his attention, is the influence which the nature of the bottom may exercise on the supernatant water. It will evidently make a great difference whether the latter stands on a pure gravelly bottom, free from particles of mould, or on a marshy soil in a state of fermentation. This influence will also be much modified by the depth of the water: the shallower this is, the more quickly will the disengaged gases pervade it. Should the water be saturated with, or have little affinity for, these gases, they will escape into the atmosphere, destroying, in part or whole, the reciprocal action of the atmosphere and the surface of the water—as we see best marked in mineral springs, and standing waters supplied by them. Water will also be modified by the salts soluble in it contained in the soil on which it stands. Water, too, is often decomposed by the constituents of the soil; and carburetted, sulphuretted, and phosphuretted hydrogen, as well as ammonia, are sometimes formed at its expense. Dr. Clemens proceeds to consider, "whether this development of gas between the mud and the water has an influence on the evaporation of the latter, and whether it proceeds at the expense of the constituents of the water or not?" This question he investigated by means of the glasses already described, and he found that the level of the water with which the mud was mixed was constantly below that in No. 1, the maximum difference between Nos. 1 and 3 varying between four and six lines. The greatest difference existed between the purest water, No. 1, and the most impure, No. 3. This difference he explains by considering that the contact of the soil (one capable of oxidation) with the water, caused a decomposition of the constituents of the soil at the expense of the water. Water, therefore, standing on an oxidizable soil, may be considered as a sphere of water in the air, evaporating on all sides, and when the surface of contact with the soil greatly exceeds that with the atmosphere, the loss by chemical change may preponderate over the atmospheric evaporation. As a fish is in a condition, gradually, by

its respiration, to corrupt the water in a vessel, especially when the diameter of the surface of contact with the atmosphere is less than the greatest diameter of the mass of water in which it lives, so may a soil at the bottom of a collection of water, if undergoing oxidation, corrupt the water above it, which will especially occur if the sum of the surface of contact with the soil exceed the sum of the surface of atmospheric contact. This last element exists in all stagnant waters, and cannot be too strongly borne in mind. If, for example, we assume that a standing water touches the atmosphere with a surface of 7000 square feet, and the soil with one of 14,000—that it daily exhales into the air, for each square inch of contact, two cubic inches of sulphuretted hydrogen, and receives from the soil a like proportion—how much of that poisonous gas, which is so soluble in water, will it have taken up in the course of a year? The author appends diagrams to demonstrate the necessary excess, in all cases, of the surface of contact with the soil over that with the atmosphere. Mineral waters, originating in chinks in the earth, are slow in attaining contact with the atmosphere, which is then but limited, hence the preponderance of their earthy constituents. For an opposite reason, river water contains scarcely a trace of free carbonic acid; consequently, no carbonates are present in it. It may be objected, that a soil will occasionally be capable, from its chemical constitution, of producing such gaseous emanations without the interposition of the water, as strata of coal may develop carburetted hydrogen; but the author asks if this is not to be attributed to the interstitial presence of water, for perfectly dry coal never evolves that gas. The reciprocal actions between the bottom and the supernatant water will diminish, and finally cease, where the latter is of great depth, as in parts of the ocean, in consequence of the coincidence of the vast pressure of the column of water with the absence of light and warmth.

The author believes that the decomposition of water by atmospheric and telluric contact is a process similar to, if not identical with, the resolution of that fluid into its constituents produced by galvanism. He adopts Faraday's explanation of the galvanic decomposition of water.

4. *Microscopic and Chemical Investigations of the Water of the Reservoir: its Deposit and its Exhalation.*—Under this head the author devotes his chief attention to the microscopic examination of the water and its deposit, and to the investigation of the gaseous exhalation, considering the latter as a vital expression of the water. The microscopic examination is a necessary preliminary in all such researches, as it affords a deep insight into the nature of the organic matters mixed with the water, a department in which Professor Deville has lately (though, unfortunately, without the aid of the microscope) laboured with great success, in his analytical examinations of drinking and river waters.* A glance with the microscope convinced the author that he had to do with a water swarming with organisms; he accordingly examined it about every eight days, or oftener if there was a change of colour in the water. The most common varieties of infusoria he found to be more rapidly developed after continued rain, and after warm showers their quantity was quite incredible. He compares the growth of fishes weighing six or

* Annales de Chimie et de Physique, p. 32. May, 1842.

eight pounds, in this water, otherwise poor in vegetable matter, out of gallionellæ, monads, vibriones, rotatoria, &c., to the narrow-gulletted whale of the polar seas feeding on medusæ, or to the polishing slate of Bilin, which has been shown to consist of the shells of gallionellæ, a cubic foot containing the remains of about seventy billions of these animalculæ.

The water of the reservoir—examined during dry weather, that it might be free from extraneous matters washed into it by the rain—was found to contain 0·4 of solid constituents in 1000 grains. These consisted chiefly of carbonate, chloride, and sulphate of lime. In the residuum of larger quantities the author found, in addition, silica, and sulphates of soda and potash. The chemical examination of the water, therefore, presented no peculiarities, nor did that of the gases evolved, no sulphuretted hydrogen being discoverable, and carburetted hydrogen being, as is the case in all stagnant water, predominant; while carbonic acid and atmospheric air (nitrogen) were present in small and continually varying proportion. The absence of sulphuretted hydrogen is important, as its presence in considerable quantity would limit the development of animal and vegetable organisms. Neither did the deposit present any peculiarity, consisting in part of clay washed into the reservoir, in part of a precipitate from the water and of the living organisms it contained.

The examination of the gases collected over the water in the glass receivers showed a surprising quantity of oxygen, which was sometimes extremely pure, and was the more striking as the glasses were always perfectly closed from atmospheric contact for some time before the examination of the gases was made; consequently, the oxygen must have been developed from the water. As the author was not the first to obtain such results, and as he was already aware of the development of oxygen by animals of low organization (gallionellæ, &c.), the explanation of the phenomenon at once occurred to him. The oxygen developed after the first months was invariably the purest, and in largest quantity in the receiver No. 2; in No. 1 it was less in quantity, but the gas was almost perfectly pure; in No. 3, in which the mud was added, carburetted hydrogen and carbonic acid were mixed with the oxygen, but never in such quantity as to cause the extinction of a flame. It was thus evident that the gaseous exhalation of the water of the reservoir was dependent, in the first place, on the amount and nature of the living organisms contained in it; and, secondly, on the mud and its capability of oxidation. The generation of oxygen was further found to be proportionate to the development of living organisms in the water, and when the examination of the gases in Nos. 1 and 2, exceptionally showed but very small quantities of this principle, the microscopic investigation exhibited a dying off of the infusoria, and in No. 2 a more limited development of vegetable admixtures. The largest amount of oxygen was, on the contrary, always found to be combined with an increase of the infusoria (especially of the green varieties), and in No. 2 with the formation of the spores and filaments of the confervæ. Exposure to the almost uninterrupted rays of the hot sun, combined with absolute rest of the vessel containing the water, caused, however, rapid maceration of the vegetable formations, and death of the infusoria; in such cases the active development of oxygen gradually ceased on the third day, and on opening the apparatus on the sixth day a gas was obtained containing carbonic acid and carburetted hydrogen in excess, in which a burning splinter was immediately extinguished. Microscopic examination now exhibited swarms of monads (not green), while all the other infusoria previously observed in large quantities were found to be dead, and the vegetable formations were in a state of incipient decomposition. The vorticellæ also were dead, while vibriones and funguses appeared to be coming forth. Considering this rapid change, where, as is so often the case in nature, extremes appeared to meet, and the conditions of germination and of death almost to touch each other, the word *miasma* involuntarily escaped the author. Dr. Clemens further varied his experiments, trying amongst other things, the effect of the exclusion of light. He infers that the exhalation of oxygen in water occupied with life is not unrestrictedly connected with a green colour, although green plants and animalculæ always disengage oxygen most abundantly, but that infusoria and plants which are not green are capable of disengaging oxygen in the light.

5. *Possible Influence of the Respiration of Infusoria on the Condition of the Atmosphere.*—Under this head the author expatiates on the vastness of the microscopic world, and alludes to the infusorial meadows, spreading through extensive marshes, from which invisibly bubbles up into our atmosphere a rich supply of exceedingly pure oxygen, forming, with that derived from other similar sources, an amount of that element sufficient to maintain the integrity of the air. All infusoria do not, however, disengage oxygen in the light; with the increase of some a diminution of that gas will be observed, but against these we have a vast preponderance of oxygen-exhaling individuals.

6. *Results of the Quantitative and Qualitative Examinations of the Exhalation of the Expanse of Water, and their bearings on the Development of Ozone.*—From his experiments, the author calculates that a square foot of the water surface under investigation yields daily at least two cubic inches of oxygen, at which rate the entire expanse would, in the course of a year, evolve 33,661 cubic feet and 192 cubic inches of that vital principle. Extending this view to all the waters of our globe, and assuming the development of the gas at but one-fourth of a cubic inch, we should obtain (exclusively of that afforded by the phanerogamic vegetable world) an amazingly large supply of oxygen; and the author believes that observations on the reciprocal actions of the atmosphere and water-surfaces are destined to throw much light on the maintenance of the proper constitution of the former. A long series of experiments on the subject strengthened him in the opinion that the increase of ozone he observed above the surface of the water might be owing to an electrical excitation of the exhaled oxygen, due to a similar condition of the aqueous surface it has left, and which might be produced either by the friction of the strata of air in contact with it, by the evaporation of water, or by the action of the solar rays. This view was corroborated by the fact that the ozone reaction always corresponded to the greater or less evolution of oxygen from the water. The author here refers to a circumstance he had before alluded to, that waters which exhaled much oxygen often considerably exceeded the general ozone reaction, showing

an evident increase of ozone in the strata of air in contact with their surfaces, while the diminution of ozone and the trifling ozone reaction above the surface of various marshes, were owing partly to the want of oxygen in the exhalation, and partly directly indicated a gaseous emanation or marsh miasm, destroying the normal amount of ozone in the air. Two important results were thus obtained from the experiments. *In the first place, the ozone-destroying property of the marsh miasm was established; and in the second, attention was directed to a peculiar ozonization of the oxygen abundantly set free from many sheets of water.* The former may assist us in attaining a knowledge of the characters of marsh miasm, the second may be serviceable in finally establishing the nature and essence of ozone. It is also probable that the motion of a water surface caused by currents of air may play an important part in its electrical excitation, and in increasing the contact of ozone; accordingly we find that absolute rest facilitates the putrefaction of a sheet of water. Hence, in southern regions a long and persistent calm may, at high degrees of temperature, exercise a doubly injurious influence; for the miasmata developed in abundance might also arise from waters which, under favourable conditions, would exhale ozonized oxygen, and the increasing putrefactive emanations, becoming stratified in the unmoved aërial ocean, cannot be purified by interchange with other atmospheric strata; for in summer, if ozone be not conveyed by the wind to purify the lower portions of the air, diseases of all kinds arise.

The author next describes additional experiments performed to demonstrate the ozone-destroying properties of marsh miasmata, through the details of which our limits do not permit us to follow him.

7. *Action of the Perhalation, from the Bottom of the Reservoir, on the Ozonization of its Evolved Oxygen, with some General Remarks on this Influence.*—This action in the reservoir, under special consideration, was, partly owing to its depth, not perceptible. Over the very shallow fish-pond of Hellerhof, on the contrary, which is crowded with vegetable slime and infusoria, and in which the perhalation from the fermenting mud and deeply-percolated soil, with putrefactive decomposition of the constituents of the water, creates a miasm destructive of ozone, the reaction was constantly *nil.* (The inhabitants and workpeople of Hellerhof suffer from very obstinate intermittents occurring in summer, and perfectly endemic in that locality.) The influence of the seasons on the perhalation from the bottom of collections of water is often incredible. Of the colossal scale on which the hydrogen compounds of such exhalations may occur, we have proofs in the non-volcanic islands which suddenly appear in summer in many stagnant waters, as well as in the rising and falling bogs of various localities.

The author suggests, that the sudden bursting of bubbles of water containing oxygen gas, escaping from a water surface exposed to the solar rays, may be a source of its electrical excitation or ozonization. From further and special observations on this subject, it appeared that the most abundant eruption of ozone from water surfaces warmed and still irradiated by the sun, took place in summer between the hours of five and nine in the evening; and it might almost seem that the ozonization of oxygen attains its highest point with the setting of the sun, when we in general observe the greatest increase of electricity, so that the characters of a water-surface, heated and electrically excited by the sun, are first lost with its cooling and the increasing dusk.

"When, in summer, a copious and early dew falls towards the end of days which have been hot and dry, I have found the ozonometer of the open air always to correspond in its evening reactions with those suspended above water-surfaces, a circumstance which makes me think it very probable that a general increase of electricity takes place during the precipitation of dew. How our so-called diseases from cold (which, perhaps, all belong to the category of disturbed electrical equilibrium) may be connected with these summer evening exacerbations of electricity, is a difficult problem, and its solution can be attempted only by a physician who, in addition to the most accurate philosophical instruments, possesses an extensive acquaintance with natural science, and an indefatigable perseverance in observation. It is an undeniable fact that colds, and especially rheumatism, occur frequently, and with threatening symptoms (tetanus rheumaticus), when the body, warm and perspiring from the heat of the day, cools too suddenly in the commencing dew of sunset. These observations apply still more forcibly to tropical regions, where such occurrences are highly dangerous." (p. 84.)

8. *Difference between the Oxygen contained in the Air of Marshes and that of Innoxious Water Surfaces.*—Although, from the author's experiments, it appears that the gases developed from marsh-water in a state of putrefactive decomposition often contain little or no oxygen, it is a well-known fact that the amount of oxygen in the neighbourhood of marshes, as well as in an apartment in which many people have breathed, is almost as great as that in other and perfectly salubrious places. In proof of this statement, the author refers to the experiments of Humboldt and Gay Lussac in the Théâtre Français, of Edmund Davy in the wards of a hospital in Cork, of Saussure, Séguin, and others. Hence it would appear, that the peculiar properties of the marsh miasm, and the unwholesomeness of the air of crowded apartments, do not depend upon a deficiency of ordinary oxygen, but of ozone. In the former case, the miasmata commingled with the air are allowed to act; in the latter, a powerful vital stimulant is absent. Thus a considerable remission of atmospheric electricity (such as is stated to have been observed in many places, and particularly in St. Petersburg, during epidemics of cholera) might have a great effect in favouring the production of miasmata, as much less ozone would, under such circumstances, be developed. The author recommends that, in conducting ozonometric observations in suspected situations, the night air should be especially examined, as some marshes during the day, and under the influence of the sun's rays, disengage a trace of ozone, which during the night send forth nothing but baneful miasmata. He divides stagnant waters into two great groups—namely, into marshes which, in the sunlight, separate ozonized oxygen, and those which, under all circumstances, annihilate the ozone of the air. But he is far from saying that the former species is wholly innoxious, as he is from assigning the destruction of ozone as the sole cause of the production of disease by the latter.

9. *The Day and Night Exhalations of Marshes and Stagnant Waters.*—At night, the author observed a diminution of ozone, both in the atmosphere at large, and over such water surfaces as he examined; the greatly increased noxiousness of the nightly exhalation from marshes is accordingly striking. The author obtained demonstration of the effect of thunder-storms, particularly those occurring at night, in purifying the air.

10. *Summer and Winter Exhalations of Stagnant Waters, and their*

influence on the Ozone-reaction of the surfaces of the latter.—The peculiar distinction between the composition of the air in summer and winter depends chiefly on the evaporation of water. The watery vapour which is mixed with the atmosphere in summer being absent in winter, the air will, for equal volumes, contain more oxygen in winter than in summer. On the other hand, the development of oxygen from water surfaces, which is chiefly dependent on the greater or less amount of living organisms in the water, sinks in winter, and increases in summer with the progressing development of the latter. As the sources which exhale miasmata are closed in winter, the rich supplies of ozone from stagnant waters cease with the increasing frost, accordingly the ozone reaction, which is, on the whole, much more considerable in winter than in summer, is also then more uniform.

Whether all that has been advanced as to the origin, nature, and effects of ozone, shall stand the test of further experience, it will require both time and careful observation to determine. Sufficient has, however, been brought forward to show that the subject is one worthy of diligent investigation, and which may possibly lead to the most important results. Should the verdict to be brought in be favourable, it will award to M. Schönbein, and such able and zealous followers of the path initiated by him as Dr. Clemens, the creditable distinction of introducing us to a more material acquaintance, if we may be allowed the phrase, with the occult causes on which diseases depend, than we have as yet attained to. Of the necessity of an extensive and combined system of ozonometric observations, especially during the prevalence of epidemics, there can no longer be any doubt.

With regard, however, to the question of the salubrity of the situation of the Hospital of the Holy Ghost, it appears to us that the occurrence and obstinate persistence of the severe intermittent fever described by the author, are practical proofs of its unhealthiness, and that the latter may perhaps depend as much upon the wafting of the aqueous vapour rising from the surface of the reservoir, into the hospital, as upon the existence in the atmosphere of any more specific malaria. It is scarcely necessary to recapitulate the various points of interest referred to in Dr. Clemens' important paper, or to direct attention to the many well-known facts which would be elucidated by the establishment of the ozone theory. It may suffice to mention a few, such as the renewal of our atmosphere, partly through the respiration of animal and vegetable infusoria, the greater insalubrity of shallow than of deep stagnant waters, the beneficial effects of currents of air on water surfaces, the influence of thunder-storms in purifying the atmosphere, the greater danger of nocturnal than of diurnal exhalations, the oppressive and stupefying effects of the air of crowded apartments, while oxygen is found to be present in it to nearly the normal amount; perhaps, too, it is in the varying proportion of ozone in our atmosphere that we are to seek the explanation of the freshness and buoyancy communicated to the spirits on what are termed "bracing" days, and of the dulness and depression generally felt on those of an opposite character. In conclusion, we must express our admiration of the indefatigable industry with which Dr. Clemens has pursued this important subject.

William D. Moore.

REVIEW XII.

Report on Epidemic Cholera in the Metropolis in 1854. By Dr. SUTHERLAND. Presented to Parliament, 1855.—Octavo. pp. 120.

Report on the Results of the different Methods of Treatment pursued in Epidemic Cholera. By the Treatment Committee of the Medical Council of the Board of Health. Presented to Parliament, 1855.—Octavo. pp. 28.

FROM the period of Sir B. Hall's appointment as President of the General Board of Health, on the 12th of August, 1854, he seems to have devoted himself earnestly and judiciously to the important duty of arresting and mitigating, as far as possible, the epidemic of cholera which had shortly before broken out, and was rapidly on the increase. He accordingly appointed two medical inspectors to advise local authorities in the exercise of their powers under the Nuisances Removal and Diseases Prevention Acts, and to assist himself in the preparation of the necessary regulations and directions.

In consequence of the spread of cholera in the metropolis, nine medical inspectors were appointed, on the 31st of August, to visit the infected districts, and inquire into the local causes which might appear to have determined the outbreak of the epidemic; to see whether the cleansing and removal of nuisances required by the regulations had been done; whether there was sufficient medical attendance for the sick; to urge, in the strongest manner, the necessity of medical house-to-house visitation and the provision of a sufficient amount of dispensary relief; to report any deficiencies in the existing arrangements, and such additions and improvements as might be requisite; and specially to ascertain the state of the water supply in the southern districts, on account of serious complaints having been made against it. The reports of these inspectors were forwarded daily to Sir B. Hall, who immediately gave the necessary instructions upon them. From these documents Dr. Sutherland, who was Superintending Medical Inspector for the Metropolis, has prepared the Report now before us.

In addition to the inspectors, a medical council was appointed, and the reasons for this are so fully stated by Sir Benjamin, that we prefer to quote his own words:

> "Meanwhile, though the services of the medical inspectors enabled me to carry out the administrative medical arrangements which the law directed, I became every day more and more impressed with the necessity of some means by which this department might be enabled to avail itself of the best medical assistance, in matters coming within the domain of scientific medical inquiry. With this view, it seemed to me desirable that scientific inquiries (chemical, meteorological, and microscopical) should be instituted into the circumstances attending the epidemic, and that a medical council should be appointed to act during the prevalence of the epidemic."

The names of the gentlemen nominated members of the council are a sufficient guarantee, alike of the earnest interest taken in the question by Sir B. Hall, and of the able and scientific manner in which the various branches of the investigation have been conducted. It is to be hoped that the results of their labours will satisfy Government of the necessity for a permanent medical council attached to the Board of Health, to assist in carrying out, deliberately and fully, those important investigations on the subject of hygiène which they were called upon hurriedly to undertake in a pressing emergency.

We shall now endeavour tō lay before our readers, as briefly as possible, the leading facts contained in these two Reports.

I. During the year 1853, cholera prevailed to some extent in London, the deaths having amounted to 847. They were distributed as follows:—Jan.—July, 32; Aug., 48; Sept., 99; Oct., 293; Nov., 318; Dec., 57. The disease may be said to have then disappeared, for although occasional deaths were registered, they only amounted to 16 in the first half year of 1854. In July, however, it again became epidemic, not spreading, as it were, from a central point, but appearing about the same time in the most opposite quarters of the metropolis. Thus, the earliest fatal cases were recorded in South Chelsea, Rotherhithe, St. George's in the East, a ship off Bermondsey, Marylebone, Brixton, and Shoreditch. It rapidly increased, attaining its maximum in the week ending 9th of September, when the deaths from it amounted to 2050. It then began to decline, and may be said to have ceased in the beginning of December, having run its course in twenty-two weeks, during nine of which it was on the increase and thirteen on the decrease. During the whole of its course diarrhœa was also unusually prevalent and fatal, following with great regularity, as indicated by the weekly deaths, the same course as the cholera.

The mortality by the epidemic on the north side of the Thames amounted to 1 in 353 inhabitants, and on the south side to 1 in 108; and was, generally speaking, greatest on the lowest levels, with the exception of St. James's, Westminster, which Dr. Sutherland states will form the subject of a separate report. The district which suffered most severely, both in 1849 and 1854, was Rotherhithe; next to it, in the last epidemic, stand Bermondsey and St. Olave, the latter of which stood second in the epidemic of 1849, and first in that of 1832—33.

The reports of the inspectors concur in stating, that wherever "cholera had become localized, they found it connected with obvious removable causes." Of these, the principal were the *open ditches*, used in most instances as sewers, or receptacles of all descriptions of filth, and receiving the drainage of numerous privies. The exhalations from these are so offensive as not only to be sufficient to "account for the outbreak of cholera, but to create surprise that malignant epidemic disease should ever be absent" from the neighbourhood. In the low-lying parts of the metropolis,

> "There are large masses of population dependent for their drainage on open ditches, tidal ditches, old badly-constructed sewers, and still worse house drains; the result of the whole being, that the excreta of a large part of the metropolis are not conveyed away, but left to putrify and rot in the open air, in cesspools under houses, or in large underground sewers, always generating foul gases, which are poured out into the streets, or into the houses: while, in the more open districts, the exhalations from the ditches keep the atmosphere in a constantly malarious condition." (p. 27.)

The *structural defects in streets and houses*, such as the existence of numerous narrow overcrowded courts and alleys, many of them mere *cul de sacs*, houses built back to back, and consequently without the means of thorough ventilation, the deficient privy accommodation, and the disgraceful state of the paving of the courts and alleys, are fruitful and constant sources of zymotic disease. To these may be added the neglect, or at least the imperfect mode, of cleansing the back streets, with the general deficiency and imperfect nature of the sewerage and house drainage. The latter in particular is strongly dwelt upon as a cause of disease. The effluvia arising from collections of night-soil, the direct vitiation of the air by open privies or by drains, the exhalations from badly-constructed sewers, and from open gully-holes in the streets, are pointed out as among the most powerful localizing causes of cholera.

The mischievous results of drains passing under houses attracted the attention of the Inspector, and some very striking illustrations are given, not

> "From the worst streets or houses, but from the dwellings of respectable tradespeople in comfortable circumstances. Both inhabitants and proprietors expressed a strong desire that this most injurious and dangerous manner of draining should be discontinued, and the sewage removed without passing under the houses." (p. 33.)

Noxious trades, the accumulation of dust, road-scraping, and night-soil in the dustman's yard, and the effluvia proceeding from filthy stables, cow-yards, and pig-sties, are cited among the aggravating causes of the epidemic. Dr. Greenhow reports that complaints of these trades were very early made to him in the Borough, "and there can be no doubt that the insalubrity of the district is much increased by these being carried on in the midst of a thick population."

Perhaps we cannot better conclude this brief notice of the chief causes of the disease, than by quoting the summary of Mr. Patterson's inspection of those parts of the parishes of St. James, Westminster, and St. Anne, Soho, which were so severely affected by the epidemic. He describes

> "The dwellings as overcrowded, unventilated, without any efficient drainage; the cesspools and privies overflowing; the supply of water scanty; some of the houses so filthy and unhealthy as to be unfit for human habitation; some of the sewers of the district in a foul condition, pouring forth sewer air through the gully-grates into the streets and houses. He says there are in the district 'almost every nuisance and abomination,' slaughter-houses, cow-houses, boiling and other noxious and deleterious trades." (p. 38.)

The influence of the water supply upon the spread of cholera in the metropolis is fully discussed in the Report, and much interesting information afforded. The water used in the houses and neighbourhoods where the disease was most prevalent was examined microscopically by Dr. Hassall, and chemically by Dr. R. D. Thomson. The deduction from the microscopical analyses is,—

> "That there is no water supplied to the metropolis that does not contain dead and living organic matter, animal and vegetable. But the Thames Ditton water supplied by the Lambeth Company, is by much the purest of the waters, while the Southwark and Vauxhall water is one of the worst, and the waters of the other companies might be arranged in a series between these two." (p. 41.)

For detailed information on the subject of these analyses, and for numerous illustrations of the influence of the water in the production and propagation of cholera, we must refer our readers to the Report, confining ourselves to the following striking results of an inquiry instituted by the Registrar-General:

> "In 26,107 houses that derived the water from Ditton, 313 deaths from cholera occurred in ten weeks. In the 40,046 houses that received the impure water from Battersea, 2443 persons, it was ascertained, died from cholera in the same time. The deaths in the latter districts exceeded by nearly 2000 the deaths that would have occurred if cholera had only been as fatal as it was in the houses that derived their water from Ditton." (p. 46.)

Dr. Sutherland makes the following remark upon these results:

> "When it is considered that the sanitary condition of the population does not materially differ, except in the quality of the water supplied by the two companies, it is difficult to resist this statistical evidence of the predisposing effect of the Battersea water, and of the loss of life which has arisen from its use." (p. 46.)

The object of the Board of Health in all the measures it undertook, was—

> "To aid the local authorities, without interfering with their freedom of action, and to lead them to fulfil the obligations imposed on them by the statute, without resorting to any attempts at compulsion, by means of provisions under which the President was advised that the directions could not be enforced."

That this object was not attained is too evident from the part of the Report which relates to the measures adopted by the Boards of Guardians. Several Boards applied for advice, but in some instances did not act upon it; others declined assistance; and from some no answer was returned to the President's circular. Of the few who accepted assistance, the majority were not parishes which suffered much from the epidemic. In some parishes, where inspectors of nuisances were appointed, they were so inadequately paid, that they did but little, and often resigned when their services were most required. Even the recommendations they did make were most imperfectly carried out.

> "Taking all the parishes together in which the epidemic was most fatal, it appears that in not one of them was the preventive machinery, sanitary and medical, organized in accordance with the minute of instruction, although some parishes had done more than others." (p. 51.)

The opening of houses of refuge—a measure attended with great benefit in 1849—was wholly neglected before the epidemic broke out, and only one or two were provided after it had become general. The arrangements to afford medical relief were, in almost every instance, inadequate, and in some parishes no provision of any kind was made.

The result of the inquiry shows—

> "That some more suitable local authorities, with adequate powers for carrying out permanent sanitary works and measures, and for providing the working classes with adequate medical relief during seasons of pestilence, are absolutely necessary for the metropolis." (p. 62.)

The necessity for a well-organized staff of health officers is very obvious, for—

> "So-called sanitary improvements, carried out in the absence of the necessary knowledge of the effects of those local conditions which they are intended to remedy, may in any case, as they have been in many cases, be mere empiricism and waste of money." (p. 65.)

The sixth section of the Report treats of the effects of sanitary improvements in preventing attacks of cholera, and contains many interesting examples of the benefits resulting from these. It forcibly illustrates the advantages which have arisen from Lord Shaftesbury's Act for the regulation of common lodging-houses, and gives some very striking statistical details, showing the great exemption from cholera and diarrhœa, during the late epidemic, enjoyed in the establishments of the Society for the Improvement of the Dwellings of the Labouring Classes, which have been constructed or altered with every attention to sanitary arrangements:

> "The experience, so far as concerns the results of sanitary improvements, is most satisfactory, although it also shows that other things must be attended to in order to ensure security from epidemic disease." (p. 71.)

The Report leaves on our minds a very favourable impression of the zeal and intelligence displayed by the inspectors appointed by the Board of Health, and a strong feeling of the folly of entrusting the care of the public health, as at present, to a "multiplicity of authorities with ill-defined duties," more especially as it is no unfrequent occurrence to find the administrative authority in the hands of parties directly interested in the continuance of the present state of matters. More ample powers are required than now exist for the enforcement of sanitary improvements; but judging from the activity displayed by Sir B. Hall, and the thorough knowledge he has shown himself to possess on the subject, we look forward with confidence to his proposed measure providing a satisfactory remedy for existing evils.

II. The Report of the Treatment Committee is of a nature which renders it almost impossible to present to our readers a summary of the results. It is, in fact, itself a summary of the returns received from the members of the profession, and contains numerous tables which require and deserve careful study. The general conclusions deduced from these, as to the relative success of the different modes of treatment in cholera cases, are given in the following extract:

> "The evidence of the tables condemns the eliminant treatment altogether as a principle of practice.
>
> "It testifies against the stimulant principle, excepting as a resource in extreme cases.
>
> "It displays a decided advantage in the alterative principle, especially as carried out by calomel and opium; and it shows a still superior advantage in the astringent principle as applied through the means of chalk and opium—the general percentage of deaths following each plan of treatment being,

	Per cent.
Of Eliminants	71·7
Stimulants	54·
Alteratives, calomel and opium	36·2
Astringents, chalk and opium	20·3

> "In order to judge correctly of the value of this evidence, it is necessary to examine, as far as may be possible, the degree of severity of the cases brought beneath this test. The only means at our command (on the present occasion, at

least) to make this examination is to consider the relative proportion which the cases of collapse bear to the number of deaths of their own classes respectively. Examining, therefore, the collapse cases with the number of deaths, we find that calomel and opium stand highest in the scale of success, and the order of preference appears as follows:

	Per cent.
Calomel and opium	59·2
Calomel (larger doses)	60·9
Salines	62·9
Chalk and opium	63·2
Calomel (small doses)	73·9
Castor oil	77·6
Sulphuric acid	78·9

> "According to this result, the superior success of calomel and opium in severer cases appears as a distinct fact elicited by the present inquiry. It is accompanied by other facts—viz., the relative advantages of those other modes of treatment which follow in their order of success. This order marks the use of calomel in small doses, of castor oil, and of sulphuric acid, as actually to be deprecated in severe cases." (p. 13.)

The results, however, are different as regards diarrhœa; for while chalk with opium, ammonia, and catechu, stand first, sulphuric acid, with opium, was found very nearly as successful.

The Reporters observe:

> "It is apparent from the returns that the success of the various modes of treatment in the hospitals follows the same ratio as those in private practice. As far as it goes, this is a valuable and interesting testimony, that there is something real in the result arrived at." (p. 14.)

The Report of the Committee is very imperfect, not from any fault on the part of the members, but from the circumstances under which it was prepared:

> "The inquiry was not undertaken until the epidemic had reached its culminating point, and when leisure for due pre-arrangement was wholly wanting. The work was partially impeded by the absence of experience in devising the most perfect method of drawing out the returns; and the answers have been less general than might have been expected had the papers been sent out earlier, and the medical profession been more prepared to co-operate." (p. 27.)

The Committee, however, have broken ground, and we trust the results of their labours are such as to satisfy the Government of the necessity of a permanent Medical Council, forming part of the General Board of Health, that their advice may be at all times available on questions relating to public health, and that they may be at all times prepared to collect facts relating to any unusual prevalence of disease, which may be turned to account in removing the causes and guiding the treatment. Most sincerely do we trust that this Report may prove to be what the Committee characterize it: "The commencement of a system of national statistics—a system which is intended to produce, not opinions, but materials, on which philosophical deductions are hereafter to be based."

PART SECOND.

Bibliographical Record.

Art. I.—1. *The Elements of Materia Medica and Therapeutics.* By Jonathan Pereira, M.D., F.R.S., and L.S., &c., &c. Vol I. Fourth Edition. Edited by Dr. Alfred Swaine Taylor and Dr. George Owen Rees.—*London*, 1854. pp. 982.

2. *The same.* Vol. II. Third Edition. Part 1. By the Author.—*London*, 1850. pp. 638.

3. *The same.* Vol. II. Part 2. Third Edition. Edited by Drs. Taylor and Rees.—*London*, 1853. pp. 827.

So universally known, and so highly prized, is the unequalled work of the late Dr. Pereira, that to offer our commendations thereof were to attempt to gild refined gold. The issue of a fourth edition of so voluminous a treatise within a very few years speaks for itself, and affords sufficient evidence that no other treatise upon the subjects with which this is occupied, has been put in competition therewith. So far as it is possible that scientific attainments and literary acquirements can confer immortality of reputation, the deeply lamented author has assuredly earned it; so long as British medical literature shall exist, so long, assuredly, will the name of Pereira be held in highest honour. We have only now this melancholy conviction to compensate us for the heavy and sudden bereavement which the profession has sustained in the unexpected death of the author.

At the time that death laid his pitiless hand on our distinguished brother, he was engaged in preparing the fourth edition for the press. Had his life been spared to have completed this task, the simple announcement of the fact would, on our part, have sufficed. It is now, however, our duty to note the circumstance of the completion of the present edition having devolved upon other editors, and to show the manner in which Drs. Taylor and Rees have performed the duties of so difficult, onerous, and responsible an office.

In the year 1853 the editors incorporated into the third edition of the second part of the second volume a large mass of matter left in manuscript by the author. These we perceive to have been very considerable in the article on Opium. There are large additions throughout, by the editors, in this volume.

We have, however, more particularly examined the new edition of Vol. I., as being first in order, and latest in publication. Almost every page bears evidence of the diligence with which the editors have endeavoured to fulfil the author's intention—that his work should constitute an exact representation of the state of pharmaceutic and therapeutic science;—that his volumes should be in reality, as well as in name, a "Cyclopædia of Materia Medica."

We have counted nearly two hundred pages in which editorial additions are to be found, irrespective of a complete revision of the formulæ and processes of the London and of the Dublin pharmacopœias, new editions of which have appeared since the publication of the former edition of the first volume of Dr. Pereira's work. These revisions alone must have cost no little labour and expenditure of time.

We should greatly exceed our limits were we to attempt an enumeration, or seriatim notice of the several additions to which we have referred. There are many of them necessarily brief, but there are many of considerable length, and conveying very valuable and interesting information regarding improvements in the processes for the manufacture of the various salts, for commercial as well as for pharmaceutic purposes. All the most recent observations and experiments upon the medical and surgical uses of old and new remedies receive ample notice. The researches of scientific naturalists, and the acquisitions of natural philosophy, where they have any bearing upon the treatment of disease, are, as by the lamented author, made subservient to the objects of the treatises. Without going into further detail, it may suffice to affirm, from a close examination and attentive perusal of the greater part of the new portions, that the editors have not been sparing of pains to maintain the high reputation of this work.

So rapidly augmenting is the great store of facts in medical science; so untiring the energy with which, at the present time, the mode of operation of medicines is investigated, that, although but so short a space as eight years have elapsed between the last and the former editions of the first volume of the work now under consideration, great industry, an unbiassed mind and unfettered judgment, are required to sift the wheat from the chaff, and rightly to estimate the value of opinions and speculations upon general therapeutics, as well as the actions of individual articles of the materia medica. We think that these endowments have been manifested by the editors in the performance of their onerous and responsible duties, and that they have produced editions in every respect worthy of those which emanated from the hands of their originator.

ART. II.—*Osservazioni Microscopiche e Deduzioni Patologiche sul Cholera Asiatico.* Memoria del Professore FILIPPO PACINI. pp. 29.

Microscopic Observations and Pathological Deductions on Asiatic Cholera. By Professor FILIPPO PACINI.

THE Florentine micrographer prefaces his memoir with the statement, that the results recorded in it are *few and incomplete*, owing to the smallness of his opportunities for the practical study of pathological anatomy. Four cases of Asiatic cholera were the bases of these observations. In the only two cases in which he examined the vomited matter it contained sarcinæ ventriculi; in one he additionally found some vibrios of the genus Banksium. Only once did he examine the fæces, and then without particular result. In three of the cases he examined the intestinal fluid, and found floating in it a large quantity of epithelium, and some detached villi. In one of the cases he additionally found many of the choleraic fungi, and an immense number of vibrios. On examining the corresponding mucous membrane, it appeared anæmic, and completely stripped of its epithelial covering; this condition, by permitting the extraordinary serous extravasation, he regards as the first and principal pathological condition of cholera. To develop this proposition, our author engages in lengthened arguments, which however ingenious, have failed to remove from our mind an impression of regret that before their publication a much larger number of observations was not instituted, and the micrographer's power of judging their value increased by his studying the disease *en clinicier*. We cannot but think that, in studying cholera, the renowned discoverer "dei nuovi organi" if he has not actually mistaken an effect for a cause, has raised an incident to the unmerited position of an essential and fundamental feature.

ART. III.—*The Pocket Formulary, and Synopsis of the British and Foreign Pharmacopœias.* Edited by HENRY BEASLEY. Sixth Edition, corrected, enlarged, and adapted to the last editions of the Pharmacopœias. —*London*, 1855. small 8vo, pp. 556.

THIS little volume has reached a sixth edition, which in itself may be considered a sufficient proof of its usefulness. The convenience of the arrangement, the facility with which the preparation required may be found, and the fulness of the information imparted, render it equally valuable to the pharmaceutist and to the medical man.

ART. IV.—*Pharmacopœia Norvegica, regia auctoritate edita. Christiania,* 1854.

IN the new Norwegian Pharmacopœia, issued at Christiania in September, 1854, there are some points which it may be interesting to notice, and compare with those in the corresponding works published in our own country.

The drugs made use of in Norway are for the most part the same as in Great Britain; but there is no arrangement of the articles of the materia medica, such as is found in all British pharmacopœias, in a separate list at the commencement, and therefore simple drugs and Galenical preparations are completely intermingled throughout the work. The arrangement is strictly alphabetical, and the various preparations of the same drug are much dispersed, the first portion of the Latin name of the medicine being taken for the guide; for example, crystallized chloride of iron and its aqueous solution are found under the respective heads, *Chloretum Ferricum Crystallisatum* and *Solutio Chloreti Ferrici;* we have also from this arrangement the most opposite drugs and preparations following each other: thus, *Nux Vomica, Cacao Nuts, Isinglass Plaster, Alum Whey,* and *Sesquicarbonate of Ammonia* form a sequence under the heads of *Semina Strychni Nucis Vomicæ, Semina Theobromatum, Sericum Adhæsivum, Serum Lactis Aluminatum,* and *Sesquicarbonas Ammonicus.*

The inorganic substances are thus treated: if elementary, such as sulphur or phosphorus, the chemical symbol and equivalent is given, the oxygen scale (0 = 100) being employed, but there is no further account; if compound, then the chemical formulæ and per-centage of elements are given, as also the solubility of the salts, &c., in water. We may state here, that throughout the work, whenever a drug possesses powerful or dangerous properties, the medium dose, as well as the dangerous one, is indicated. For example, of strychnia we find the following:

$$\text{“}C_{42}\,H_{22}\,N_2\,O_4 = \overset{+}{\text{S}}\text{tr.} = 4175{\cdot}12$$

"Partes centum (100) partium aquæ sexcentis sexaginta sex millibus sexcentis (666600) solvuntur.

"Dosis media grani vicesima—decima ($\frac{1}{20}$ta—$\frac{1}{10}$ta) pars.; periculosa grani quadrans."

The smaller equivalent of mercury is taken, the one proposed by Professor Graham, and hence calomel and the green iodide of mercury are represented as subsalts.

When treating of barks, roots, leaves, &c., references to the plants yielding them are made; sometimes, however, these are erroneous, being those of our former pharmacopœias: for example, yellow cinchona bark is referred to cinchona lancifolia, although for many years its true source—viz., Cinchona Calisaya, has been discovered by Weddell.

Most of our novel remedies have been introduced, as valerianate of zinc, chloroform (with a process for its formation), lactate of iron, collodion (with a process), cod liver oil, acetate of ethyle, &c.

The Galenical preparations closely resemble those in our own pharmacopœias, and we have little to remark upon them, unless we attempt to enter much into detail, of which we question the utility to our readers, except required for a special purpose.

ART. V.—*Household Chemistry, or Rudiments of the Science applied to Every-day Life.* By ALFRED J. BERNAYS, Ph.D., F.C.S., &c. Third Edition, considerably enlarged.—*London,* 1854. Small 8vo, pp. 384.

THE more well-written books on "Common Things" are circulated among the reading classes of the community, the more extensive will be the diffusion of a strong antidote against the poison of spurious and vain knowledge. In no branch of science does this remark apply more than in medicine; but in order to enable the unprofessional public to form a proper estimate of what medicine and the medical man can do, it is necessary in the first instance that the former should have opportunities of becoming better acquainted with other departments of natural science. The book before us answers this purpose with regard to the numerous questions that constantly suggest themselves in connexion with the chemistry of every-day life. They are treated with perspicuity and conciseness, and with a due regard to the requirements of the class for whom the work is specially intended,—the general reader.

Art. VI.—*The Quarterly Journal of Public Hea'th, including the Transactions of the Epidemiological Society of London.* Edited by Benjamin W. Richardson, M.D.—*London*, March, 1855.

The questions affecting the public health are of such manifold and general interest and importance, they embrace so many points affecting the health and morality, life and happiness of mankind, that we hail with no little gratification the establishment of a journal under the editorship of Dr. Richardson, devoted exclusively to these topics. There is a growing tendency to appreciate the labours of the medical profession, which, in consonance with the first principles of Christianity, has long striven, at a sacrifice of all selfish interests, to impress upon the community the duty of looking to the prevention of disease by sanitary measures, of anticipating the pestilence and warding off death by neutralizing the slow poisons that carelessness and disregard of divine laws cause to be diffused through our habitations. Such considerations come home to every individual. It is the duty of governments to foster and protect nations by promoting sanitary legislation; it is no less the duty of all whose position enables them to do so, to urge the scientific and moral grounds upon which that charge should be conducted.

It is no slight recommendation to the journal before us that it incorporates the Transactions of the Epidemiological Society, whose labours have not hitherto been brought before the profession or the general public in a sufficiently prominent form, although their value has been recognised by Parliament ordering the Small-pox and Vaccination Report of the Society to be printed, and adopting in the Vaccination Bill of last session, suggestions contained in that Report.

We sincerely hope that the 'Journal of Public Health' will be studied, and meet with support and countenance from all who are called to exercise authority in these matters, as it enjoys the literary aid of many whose names are intimately associated with the cause of hygiène.

Art. VII.—*The Ethnological Exhibitions of London.* By John Conolly, M.D., D.C.L., President of the Ethnological Society. Read at a Meeting of the Ethnological Society, and published at the request of the Members.—*London*, 1855. 8vo, pp. 44.

The various exhibitions of human beings that take place from time to time in the metropolis, and generally draw crowds in proportion to the repulsiveness of their appearance and the degradation of their moral and intellectual powers, are a subject well deserving the attention of the man of science and the philanthropist. Dr. Conolly adverts to these exhibitions under both points of view. He would render them means of general instruction, and points out how much might be learnt with regard to the history of man's development by a careful study and comparison of the characters, habits, and associations of these individuals. He suggests—and the suggestion is one that the Society cannot too early adopt—that members of the Ethnological Society should publish critical notices of exhibitions of the kind alluded to, by which "they might be made valuable even to the public, and especially to young persons, directing their attention to points of real interest, and substituting correct information for the extravagant descriptions by which such exhibitions are usually recommended to the unfruitful curiosity of the people in general." In illustration of the necessity of doing something to guide and direct public opinion in matters of this kind, Dr. Conolly institutes an analysis of several ethnological exhibitions which have of late attracted public attention; he shows, by a reference to the Aztec children, how much false excitement and ignorant credulity might be prevented if men of science or scientific societies had taken the initiative in the way proposed. But although Dr. Conolly puts the intellectual part of the question prominently forward, there is a philanthropic tendency and sympathy expressed throughout, which gives the scientific question a still higher and nobler meaning. He complains that no steps are taken to rescue the untutored savage, who is made the gazing-stock of the curious in these exhibitions, from his debased condition; and he points out how he might be made the means of carrying home the arts and appliances of civilization, the truths and hallowing influences of revealed religion.

Both on scientific and philanthropic grounds we would desire to see Dr. Conolly's lecture extensively circulated and read. It is one of the evidences, now happily no longer rare, of the benefit that accrues to humanity when master minds think it their duty to descend to the consideration of every-day topics.

ART. VIII.—*On Epidemic Diarrhœa and Cholera, their Pathology and Treatment; with a Record of Cases.* By GEORGE JOHNSON, M.D. Lond., Fellow of the Royal College of Physicians, Assistant-Physician to King's College Hospital.—*London*, 1855. 8vo, pp. 294.

OUR readers scarcely require to be reminded that Dr. George Johnson is the representative of the eliminative mode of treating cholera. In the work of which we communicate the title at the head of this notice, Dr. Johnson gives the summary of fifty-four cases of cholera and choleraic diarrhœa in which he has employed castor oil, generally given in doses of half an ounce every half-hour. Of the fifty-four cases, thirty-seven were cholera in a state of greater or less collapse, fourteen of which, or thirty-eight per cent., proved fatal. The first case of cholera given by Dr. Johnson came under his care on the 10th of August, the last on the 16th of September.

The cases are detailed in the first ninety-four pages of the book, and are followed by a disquisition into the pathology of epidemic diarrhœa and cholera. The question of treatment is considered in the third and concluding part. The views upon which Dr. Johnson bases his eliminative treatment are given with clearness and much acumen, showing an intimate acquaintance with the investigations of other writers. The chapter in which he promulgates his theory of the pathology of collapse is probably the most interesting part of the book. Dr. Johnson regards the function of respiration as that which is first and most seriously affected during this stage of the disease. After enumerating the facts bearing upon the interference with the oxygenation of the blood, the author goes on to say:

"We have seen that when death has occurred during the stage of collapse, the minute texture of the lungs is found to be remarkably pale and bloodless; while the larger branches of the pulmonary artery, as well as the right cavities of the heart, are filled with blood, which often escapes in considerable quantity when the roots of the lungs are divided. We have here, then, a positive fact; the blood has been sent into the pulmonary artery, but it has not reached the capillaries of the lungs; it must therefore have been arrested in the minute branches of the pulmonary artery. This, I repeat, is a fact demonstrated by various able and accurate observers, and stated to be of almost constant occurrence. The question thus arises, In what way can this stoppage of blood in the pulmonary arteries be explained? In what way if not thus—that the blood being morbidly changed in its qualities by the action of the cholera poison, is retarded, and finally arrested, in its passage through the minute branches of the pulmonary artery, the muscular coats of the vessels being excited to contract upon their abnormal contents, as we have found reason to believe that the minute branches of the systemic arteries contract upon and impede the passage of blood which contains an excess of carbonic acid; as both the systemic and the pulmonary arteries are probably stimulated to contract upon blood which is charged with urinary excrement, and as the arteries of an animal recently killed oppose in different degrees the passage of cold, astringent, or otherwise stimulating liquids."

In illustration of his views of the pathology of cholera collapse, Dr. Johnson institutes a comparison between this state and the cold stage of ague:

"The embarrassment of the respiratory function," he observes, "during the cold stage of ague is sufficiently obvious, and is usually attended with a short dry cough, which is a common symptom in cholera. Thus the post-mortem appearances, as they are described by various authors, show an accumulation of blood in the right cavities of the heart, and in the venous trunks, with more or less congestion of the lungs. There is, therefore, anatomical evidence to show that, as in cholera, so in ague, the pulmonary circulation has been impeded."

The defective supply of blood to the lungs being assumed as the primary cause of the collapse, the defective oxidation of the vital fluid, and the consequent diminution of temperature and arrest in the secretions, follow as corollaries. The whole of this part of the work is one displaying much knowledge and ingenuity on the part of the author, and it contains sound argument and wholesome doctrine. We should be perfectly willing to accept the main practical conclusion arrived at by the author, if it could be satisfactorily shown that the choleraic poison may be eliminated by exciting the intestinal canal to increased action. Dr. Johnson is of opinion that the success attending his treatment is of a character to justify the conclusion that the purgative action of castor oil removes the poison from the blood and effects a cure. Here we are at issue with him; and although he will probably object to the inferences arrived at by the Council of the Board of Health, whose statistics have established the eliminative plan of treatment as the least successful of all, we see no other mode of arriving at a satisfactory conclusion of such a question than by reference to the results obtained by numerous practitioners.

ART. IX.—*How to Nurse Sick Children; intended especially as a Help to the Nurses at the Hospital for Sick Children.—London,* 1854. pp. 79.

THE untutored empiricism of nurses and mothers has hitherto been generally regarded as ample protection for the healthy or the sick child, and it is altogether a new feature of the present times that attempts are made to give to that empiricism a higher and better character. Those only who have children of their own, or have watched them with parental love, can know the fund of knowledge to be gained from a study of their physical and mental development, from a study of the morbid conditions accompanying the various phases of that development. The young mother, or the still younger nurse, is commonly supposed to understand the child's wants and habits intuitively, and the consequence of such intuitive knowledge is, we fear to say how often, the source of a debilitated constitution or death to the child, anguish and a murmuring spirit to the parent. Nor is the blame to be laid solely at the door of the young mother or the child, but rather at the door of those who, with knowledge at their command, have failed to communicate it. We are not given to reading riddles, and shall not, therefore, attempt to penetrate the anonymity of the little book of which the title heads this notice, but it certainly has the merit of warding off that reproach. Whoever it is written by, the author brings to his task the two great qualifications—a thorough love of children, and a thorough knowledge of their habits in health and disease. There is no pompous condescension to the nurses whom he addresses, nor an arrogant display of that superior knowledge which he (or she) manifestly possesses; it is a simple but feeling account of the wants of the sick child, and of the duties required of its attendant. It is immediately intended by the writer for the nurses of the Children's Hospital in Great Ormond-street, Queen-square, which was opened, mainly owing to the exertions of Dr. West, in 1852. But the book is of a class which meets a great want in society; it helps to fill that want most admirably, and not referring to any temporary or accidental condition, but to circumstances that will make themselves felt as long as the world lasts, may truly be pronounced classical. We do not measure its value by its size, but by its contents. "My object," says the writer, addressing the nurses, in this little book, "is to point out to you some things which a nurse ought to know, but which I believe have never been put down in books before." The nurse's relation to the medical man; the mode of observing the child and interpreting its language; the way to aid the physician in finding out the disease; her reports to him; the points to notice in different diseases, of the brain, the chest, the abdomen; the moral management of the sick room; and other questions bearing upon the nurse's duties, are discussed clearly and intelligibly. We should transcribe the whole seventy-nine pages if we were to quote all we approve; we will not therefore, as that cannot be done, mar the effect by extracting individual passages. Few medical men will regret the hour spent in its perusal; to mothers and nurses it gives invaluable advice in a language that appeals to their heads and their hearts. The medical man should especially welcome a book like this, for everything that tends to raise the character of the nurse strengthens his powers.

ART. X.—*On the Economical Applications of Charcoal to Sanitary Purposes.* A Lecture delivered at the Royal Institution, Albemarle-street. By JOHN STENHOUSE, LL.D., F.R.S. Second Edition.—*London*, 1855. pp. 27.

CHARCOAL possesses a great power of absorbing gases and oxidizing them, which has recently been investigated and practically applied by Dr. Stenhouse to the prevention and cure of disease. Saussure showed, by a series of experiments, that boxwood charcoal was capable of absorbing gases in the ratio exhibited in the following table:

One volume of charcoal took up—	Volumes.
Ammonia	90
Hydrochloric acid	85
Sulphurous acid	65
Sulphuretted hydrogen	55
Nitrous oxide	40
Carbonic oxide	35
Bicarburetted hydrogen	35
Carbonic oxide	94
Oxygen	92
Nitrogen	75
Carburetted hydrogen	5
Hydrogen	17

Dr. Stenhouse has made similar experiments with various kinds of charcoal, and finds that wood-charcoal possesses a marked superiority in this respect over other charcoals, as shown in the following table.

Half a gramme of the following kinds of charcoal absorb the under-mentioned number of centimetres of different gases:

	Ammonia.	Hydrochloric acid.	Sulphuretted hydrogen.	Carbonic acid.	Oxygen.	Sulphurous acid.
Wood	98·5	45·0	30·0	14·0	·8	32·5
Peat	96·0	60·0	28·5	10·0	·6	27·5
Animal	43·5	—	9·0	5·0	·5	17·5

The disinfectant power of charcoal is well shown in the following experiment, an experiment which has been repeated, as we had occasion to see at St. Mary's Hospital, with perfect success:

> "My attention," Dr. Stenhouse remarks, "was particularly drawn to the importance of charcoal as a deodorising and disinfecting agent, about eighteen months ago, by my friend, John Turnbull, Esq., chemical manufacturer of Glasgow. Mr. Turnbull, about six months previously, had placed the bodies of two dogs in a wooden box, on a layer of charcoal powder of a few inches in depth, and covered them over with a quantity of the same material. Though the box was quite open, and kept in his laboratory, no effluvia was ever perceptible, and on examining the bodies of the animals at the end of six months, they were found to be in a very advanced state of decay. Mr. Turnbull sent me a portion of the charcoal powder which had been most closely in contact with the bodies of the dogs. I submitted it for examination to one of my pupils (Mr. Turner), who found it contained comparatively little ammonia, not a trace of sulphuretted hydrogen, but very appreciable quantities of nitric and sulphuric acids, with acid phosphate of lime."

Dr. Stenhouse performed a similar experiment on the bodies of a full-grown cat and two rats, which he allowed to pass through the various stages of putrefaction in his own laboratory, covered only with about two inches of charcoal powder; no smell was perceptible—no inconvenience resulted—and after about eighteen months, almost all the nitrogenous parts had disappeared, and nothing remained but the bones and a portion of the fat, which was also undergoing rapid decay. It appears, then, that charcoal, in addition to absorbing the emanations of putrefying substances, decomposes them, and rearranges the elements into forms perfectly compatible with health. Dr. Stenhouse seized upon the idea thus suggested to construct an air-filter of charcoal, by interposing a thin layer of charcoal powder between two sheets of wire gauze. This has been found to answer perfectly in the justice-rooms of the Mansion House and of Guildhall.

The large air-filter being invented, the invention of a charcoal respirator seemed to follow almost as a matter of course. It is constructed on the same principle as the large filter. "The air is made to pass through a quarter of an inch of coarsely-powdered charcoal, retained in its place by two sheets of silvered wire gauze, covered over with thin woollen cloth, by means of which the temperature is greatly increased." That the charcoal respirator will be of great advantage, wherever miasmatous effluvia exist, in purifying the air that enters the lungs, cannot be doubted; the air passes in with facility, and rapidly absorbs the gases without impeding the access of air to the respiratory passages. Dr. Stenhouse suggests that they might be used beneficially, not only in cases where infection and morbid influences are to be feared, but also in casemated batteries, where two or three volleys fired from the guns almost suffocate the gunners with fumes of sulphide of potassium; the use of the respirators "would enable them to maintain the fire from morning till night with scarcely any inconvenience."

With regard to the question of the comparative utility of the charcoal respirator and Mr. Jeffreys' well-known and admirable wire ventilator, we must remember that they serve different purposes: the latter has stood the test of many years, the former requires to be more employed before its power of warming the air as it enters the lungs can be regarded as satisfactorily settled. For phthisical and bronchitic patients, the uniform climate produced by Jeffreys' respirator is one of the most beneficial influences we are able to offer. Under all circumstances, a charcoal respirator would necessarily absorb the carbonic acid and water passing from the lungs, which at least would not improve the quality of the inspired air. Hence we should not be inclined to employ the charcoal respirator as a substitute for the wire respirator where our object is solely to warm the ingoing air, while the former would be preferable where we are brought into contact with foul and noxious effluvia of any kind which it is desirable to neutralize or to separate from the air that would otherwise waft them to our lungs. Both contrivances may therefore co-exist, and both find their appropriate and salutary application. We owe great thanks to Dr. Stenhouse for his invention, on scientific grounds, and must not forget to draw attention to the liberality with which he has refused to secure to himself pecuniary advantages from an application of natural science to daily uses that promises to become a boon to humanity.

Art. XI.—*On the Diseases of Miners.* By Herbert Mackworth, M. Inst. C.E., Government Inspector of Mines. (Read at the Society of Arts, April 4, 1855.) Pamphlet, pp. 20.

The value of statistics has been shown in nothing more palpably than in determining the question of the average viability of different classes of society, and no facts are more convincing than those which in this manner demonstrate the influence exerted by certain modes of life upon its duration. The premises are attainable; the conclusions may therefore be absolute. Two facts which appear to be incontrovertible are deducible from the statistics of the labouring population—viz., that miners, as a class, are very shortlived, and that they suffer from a larger amount of sickness than other labourers. The average duration of the life of miners and labourers is stated as follows:

Coal miners	26·1.
Labourers' class, undefined	34·0.

Mr. Mackworth, in the paper before us, enters into an examination of the causes to which the brief duration of the miner's life is to be attributed. He is of opinion that the occupation of the miner is not necessarily more unhealthy than that of other labouring classes, but that the diseases which abridge it are due to causes accessible to, and preventible by, man's ingenuity. Mr. Mackworth says, that

> "In tracing out the remedies to be applied to render the cell of the miners a fit place for human beings to pass a large portion of their lives, it is necessary for me to point out, that it requires no other remedy, except in a higher degree, than we are now adopting on the surface; the same rules of ventilation, the same habits of cleanliness will suffice. The miners have reached their present condition, just in the same manner as a portion of any town inhabited by the poor, unvisited by intelligent or professional men, uncared for and never entered by the owners of the property, would infallibly become the stronghold of disease."

The impurity of the air to which Mr. Mackworth especially draws attention, is due to the presence of carbonic acid gas, carburetted and sulphuretted hydrogen, produced by the blasting of rocks, the employment of fires and lights, the white- and fire damp, the excrements and emanations of the men and horses employed in the mines, which, under the present system, are very imperfectly ventilated.

The prevalent diseases among miners are just those which would be expected to result from the inhalation of so impure an atmosphere as they are constantly exposed to. In Cornwall, while 31 per cent. only of the general population die of chest disease, 61 per cent. of the miners succumb to that class of diseases. Pulmonary emphysema, rheumatism, and pneumonia flourish among miners. To effect a proper removal of the foul air to which the abridged duration of life in the mining population is mainly to be ascribed—

> "To effect this requires one simple regulation to be unremittingly carried out—namely, that no man shall work in a stagnant atmosphere; that the working places as now existing, the reservoirs of all the deleterious gases brought along by the air-current, shall have a current sent through them into every part, in sufficient quantity to dilute all the deleterious gases, and deprive them of their power."

The following are the practical suggestions which Mr. Mackworth offers for the sanitary improvements of mines:

"1. That an artificial power and other means of ventilation, under the constant superintendence of appointed persons, should be employed at every mine, so as to produce at all times a regular current of pure air into and throughout the whole of the working-places and parts of a mine past every workman, and so as to dilute and render imperceptible and harmless all noxious gases at the points where they are given off. That the velocity and abundance of the current of air, which must be easily accessible for examination and repairs at all points, should be regulated to the extent of the works, the number of workmen, and the escape or formation of gases and other nuisances.

"2. That dung-heaps, putrescent timber, and similar nuisances should be removed from the mine at least once a month, and the exhalations at all times carefully prevented from mixing with the incoming air.

"3. That a medical officer should examine the sanitary state of the mine at least twice a-year, and report thereon to the owners and to the government.

"4. That there should be an accident-room at every work, provided with necessaries, and inspected once a-week by the medical officer.

"5. That a benefit society should be established at every mine, at the joint expense, and under the joint management, of the proprietors and workmen, to provide medical attendance for the workmen and their families, to support them when sick and wounded, and to pension them off when too old or maimed to gain a livelihood."

We are unable to go more fully into the paper before us, but we have the greater pleasure in drawing attention to the subject of which it treats, and that in a very able and enlightened manner, as it promises to be followed by similar papers, embracing inquiries into the diseases and their prevention among the labouring classes. We understand that the Society of Arts has accepted the self-imposed burden, not, one would have supposed, included among the list of its immediate objects, of instituting investigations in Industrial Pathology. This would have been the duty of other corporations; but since the Society of Arts have undertaken it as a labour of love, we augur well of their results, and shall be happy to welcome more such productions as the one which we have thus briefly adverted to.

Since writing these remarks, a Report by the Industrial Pathology Committee of the same Society has been brought under our notice, which discusses the influence exerted by trades upon the eyes. It is drawn up by Dr. T. K. Chambers, and embraces results obtained from numerous men of science, and others practically acquainted with the subject. The reporter observes, that—

"It appears that the important organ of vision is much less injured directly by handicraft operations than the committee thought. Many of the affections quoted seem due rather to ignorance or neglect of well-known hygienic laws, than to anything essentially connected with the occupation of the workmen. Another subject of congratulation also occurs to them—viz., that all the results complained of seem capable of being met by simple, cheap, and universally attainable means; no alterations of modes of manufacture, no interference of the natural liberty of the master to have work done in the cheapest way, is necessary; but merely such precautions as it is monstrous not to adopt when once known."

We trust, that when a series of these Reports will have been presented to the Society, as appears probable from the concluding paragraph of the one in question, the whole may be published in a collective form, so as to enable us to return more fully to the very important and practical subject of Trades' Health, or Industrial Pathology.

PART THIRD.

Original Communications.

Art. I.

Further Researches into the Functions of the Brain. By Thomas Laycock, M.D., Physician to the York Dispensary, and Teacher of the Theory and Practice of Medicine at the York Medical School.

It is now some years since I extended to the cerebrum the doctrines current as to the reflex function of the spinal cord. During the interval which has elapsed, cerebral physiology has sufficiently advanced to warrant an attempt at extending my views into the more metaphysical and obscure regions of consciousness and thought. By way of retrospect, I may be permitted to observe that when engaged in an investigation of certain morbid conditions of the nervous system, as they were presented to my notice nearly twenty years ago in the wards of the York County Hospital (of which I was for some time the resident medical officer), the imperfect nature of the views then current as to cerebral physiology, and their inadequacy to explain or elucidate functional diseases of the brain, were continually forced upon me. Physiology afforded hardly any clue to the pathology of mental derangement in any of its forms of reverie and somnambulism, whether natural or artificial, or of those varied morbid manifestations of the consciousness, the perceptions, and the will, which are grouped under the terms hysteria, mesmeric phenomena, &c. Mental philosophy and metaphysics were even less instructive than physiology, for the sum of the practical knowledge they imparted, as to the function of the brain in mental operations, might be stated in the words of Reid: "In perception, the object produces some change in the organ [of special sense]; the organ produces some change upon the nerve; and the nerve produces some change in the brain." The nature of that change, and its relations to the consciousness and the will, appeared to be wholly unknown to mental philosophers, and were only discussed when it was sought to establish some vague and profitless hypothesis. Nay, not a few metaphysicians hardly concede so much as the fundamental proposition, that the brain is the organ of mind, and necessary to the manifestation of its phenomena; for they practically ignore the science of cerebral physiology, and investigate the operations of mind as if the brain took no part in them. How dangerous to scientific and religious truth and morals such a fundamental error may be, is in process of demonstration by the proceedings of "spiritualists" and their congeners, who deduce the wildest and most mischievous doctrines from their experimental researches.

Feeling this want of definite knowledge as to the functions of the brain, and its relation to mental phenomena, when investigating cerebral pathology I endeavoured to attain to something better, by adopting the inductive method of inquiry. Facts and experimental researches in abundance were not wanting; and I therefore soon reached this general conclusion, that the brain being a congeries of ganglia, did not differ in its laws of action from the other ganglia of the nervous system; and in particular, that like the spinal ganglia, it was subject to the laws of *reflex* action. It followed, therefore, that although, as the organ of conscious mind, its functions were carried on *with* consciousness, yet as being a series of ganglia analogous to the spinal, its functions might be, and often were, carried on *without* consciousness, or at least independently of the will, and of the accompanying sensations, if consciousness existed. This doctrine having been, in the main points, approved and adopted by eminent physiologists and pathologists (amongst whom my friend, Dr. Carpenter, holds a very high rank), may be considered as established; for, although I still stand almost alone in maintaining that in the so-called *sensational* actions, sensation or consciousness takes no share causally, and is only a coincident phenomenon not necessary to the acts, the main proposition, that cerebral action goes on unconsciously is placed on an irrefragable basis. I would particularly refer to Dr. Carpenter's very lucid demonstration of this part of the doctrine in the fourth edition of his 'Principles of Human Physiology,' §§ 805—845, and his admirable applications of it to various forms of cerebral disorder, whether arising spontaneously or induced artificially.

On one point, however, I am obliged to differ from Dr. Carpenter—namely, that there is an "*essential* distinction, both in their anatomical and physiological relations, between the sensory ganglia and the cerebrum, or hemispheric ganglia." It has been, on the contrary, a fixed and fundamental doctrine with me, that as to reflex action, there is no *essential* distinction of the kind, the differences being, anatomically and physiologically, rather that of species than genus; nay, that there is no essential distinction in the mode of action of *all* organized structures, whether animal or vegetable, considered in relation to the *fundamental psychological phenomenon* of reflex action, *the intelligent respondence to stimuli*. So that the laws of reflex action apply to every form of organism, however lowly, and whether it be a plant or an animal; to every kind of tissue, however simple, and whether it be merely a congeries of cells, or be so highly developed and endowed as the vesicular neurine of the human hemispherical ganglia. Indeed, I need only repeat here what I have previously stated.

> "The doctrine of a molecular organization within organized structures, such as that it shall correspond and be appropriate to given stimuli received by appropriate organs, necessarily constitutes the basis of all inquiries into the laws of action in those structures. And there can be no doubt, such is the magnificent uniformity in the infinite diversity of creation, that the laws of action of the agent and reagent in vital phenomena are as definite as those operating on chemical phenomena, could we but effect a sufficiently minute analysis and induction."*

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* Appendix to Essay on Reflex Function of the Brain, § 3. British and Foreign Medical Review, vol. xix. p. 308.

It is only, in short, on the deductions from this all-comprehensive generalization that the basis for a practical and sufficient system of human psychology can be laid.* It may be stated, then, as an admissible general proposition, and therefore of universal experience, that the cerebrum (the organ of thought) may be put into the same modes of action as occur in the other ganglia of the nervous system, when they are rendered active, independently of the will or the consciousness, by their appropriate stimuli; and (to use Dr. Carpenter's words) may act upon impressions transmitted to it, and convey elaborate results, such as we might have attained by the purposive (or volitional) direction of our minds to the subject, *without any consciousness* on our own parts; so that we only become aware of the operation which has taken place, when we compare the result, as it presents itself to our minds after it has been attained, with the materials submitted to the process. To those who have carefully observed the phenomena of thought in relation to the will and the consciousness, this mode of mental action must be a familiar fact; and to those who have studied the phenomena of reflex action, especially as displayed in the instincts of animals, its dependence upon the cerebral functions must be perfectly obvious and comprehensible. On the one hand, therefore, we have consciousness; on the other, unconscious yet intelligent action. These are the *psychological* phenomena. As the common medium of both, we have the cerebrum, the functions of which, in relation to these phenomena, form, therefore, the *physiological* problems to be investigated.

As a preliminary step, some statement of what is meant by reflex phenomena and of their nature, is necessary. It has long been known, that animals so mutilated as to be deprived of consciousness, and even mere segments of animals, display, when irritants are applied to the integument, or to the special apparatuses, movements of as definite a character as those which are directed by the will, or are under the guidance of sensations. Very numerous experimental vivisections have been made from time to time, to determine the true nature of these movements, especially on cold-blooded vertebrata, in which class of vertebrates they are the most obvious. Whytt was one of the earliest of modern physiologists to institute these experiments. He found that "a frog lives and moves its members for half an hour after its head is cut off; nay, when the body of a frog is divided in two, both the anterior and posterior extremities preserve life and a power of motion for a considerable time." Whytt found, also, that although the brain was not necessary to these movements—for they may be continually excited in headless frogs—they were no longer manifested if the spinal cord was destroyed. Whytt observed similar adapted movements in vipers, and believed that they were necessarily connected with *sensation*.

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* This doctrine has been stated by me on different occasions. In an article on Hysteria (the last of a series), published in the Edinburgh Medical and Surgical Journal, No. 140, July, 1839, I advocated the identity of function of all vital structures, whether vegetable or animal, ganglionic or cerebral. Again, in my Treatise on the Nervous Diseases of Women, 1840 (to illustrate which I first commenced these researches), chapters vi.—viii. inclusive are devoted to the elucidation of this doctrine; chap. viii. being headed, "The instinctive actions in relation to consciousness—the brain subject to the laws of reflex action." At the meeting of the British Association, in York, in 1844, I read the paper on the Reflex Function of the Brain above mentioned; and in the correspondence with Mr. George Combe, which arose out of the views advanced therein, I again reiterated the doctrine, extending it to reflex nutrition and development. "The development, conservation, and reproduction of all organisms," I show, "are regulated by an unerring law of design—a law as generally applicable to living matter as the law of gravity to universal matter." (Lancet, vol. ii. 1845, p. 256.)

"We have no other way," says he, "to satisfy ourselves that an animal is alive or endued with feeling, than by observing whether it shows uneasiness when anything hurts or tends to destroy any of its parts, and an endeavour to remove or avoid it. Since, therefore, the bodies of vipers make just the same kind of motions, when pricked with a sharp instrument, two or three days after losing their head, heart, and other bowels, as if they were entire, we are naturally led to conclude that they are still in some sense alive, and endowed with feeling—i. e., animated by a sentient principle."

This deduction from the phenomena was adopted by the majority of physiologists after Whytt—as Haller, Cuvier, Dumas, Alison, Le Gallois—and was, in fact, the doctrine generally current until Dr. Hall renewed attention to the subject, and made these experimental vivisections the basis of an improved pathology of certain diseases of the nervous system, specially implicating the *motor* system. He argued *that they were wholly independent of sensation*, and successfully; for there are few modern physiologists who agree with Whytt, Haller, and the rest. There was a contemporary of Haller, however, who gave a most lucid and complete exposition of the whole doctrine of the reflex action of the nervous system, carrying it far beyond the views of Dr. Hall, and extending it to the whole phenomena of animal life. This was J. A. Unzer, whose 'Erste Gründe' is still the best work of reference on the subject, and still unapproached by modern physiologists.* Prochaska's 'Commentaries' are but a free summary of Unzer's views, with the more metaphysical and really the more important portion omitted. It was Unzer who first systematically showed the identity of mere reflex phenomena with those that are instinctive and emotional, and explained the share which the states of the consciousness, termed pleasure and pain, have in all these excited acts. He also, of all neurologists, has most successfully made these doctrines elucidate the highest mental phenomena.

The fundamental principles of motor reflex action are these: That there is an apparatus so contrived as to place the individual in relation with the external world, and receive impressions from it in such a way that, whatever in the external world is good for the organism, is sought after and secured, if possible; and whatever is injurious is avoided or repelled, if possible; secured or repelled automatically and mechanically, without the intervention of any sensation, feeling, thought, volition, or act of conscious mind whatever. That the adapting and *quasi*-rational or sentient agent which combines and regulates the movements of the limbs or other organs to these ends is seated, in nervated animals, in the masses of nerve-cells (vesicular neurine) termed ganglia. That the apparatus by which it acts, consists: *a*. Of a special histological arrangement and constitution of the vesicular neurine in each ganglion, in virtue of which it responds to stimuli according to a fixed and predetermined plan; *b*. Of a special histological arrangement and constitution of the vesicular neurine on the periphery of the organism, which, coming into contact with the external world, is influenced according to a predetermined plan, and transmits the changes thus induced to the ganglionic vesicular neurine along conductors—the afferent nerves; *c.* Of efferent nerves (distributed to distant organs) which commence within the ganglionic vesicular neurine. and by the changes within which they, in their turn, are influenced, according to a fixed and predetermined plan, transmitting these influences to the motor system; *d.* Of the muscular system, which, receiving through the efferent nerves the influences originating in the ganglionic vesicular neurine, contracts in part, or as a whole, and in so doing puts in motion the varied mechanism already constructed, so that the external world is acted upon through the latter, intelligently and adaptively, to a distinct purpose and object—the preservation in well-being of the individual or the species. The primary object, therefore, of the reflex function of the nervous system is, psychologically, "*nostri conservatio*," to use the expressive phrase of Prochaska; the essentially necessary means of its attainment is automatic histological action within masses of vesicular neurine, according to a definite arrangement, and a fixed and predetermined series of changes.

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* I had the honour and pleasure to translate and edit this work, together with Prochaska's Commentaries, for the Sydenham Society.

We might rest here, and be content with stating that the cause (or necessary antecedent) to the infinitely varied and exquisitely adapted actions and movements known as reflex, automatic, unconscious, and instinctive, is this definite arrangement and fixed mode of action of the vesicular neurine; but the mind at once perceives the incompleteness of the statement, for it is obvious that there must be a necessary antecedent to the intelligent *action* of the machinery, in the intelligent *construction* of it. If we watch ever so superficially the growth and development of organisms, we are struck by the never-ceasing and ever-varied manifestation of the highest order of intelligence, from the first formation of the primordial cell to the perfect evolution of the entire mechanism of the individual. It is unnecessary to recapitulate illustrations of this general fact. The phenomena it includes have been the source of every variety of speculative philosophy, from Plato downwards; they are the basis of all natural theology; they are the great facts of geology, zoology, and natural history; and are ever connected, in all speculations, with the *instincts*—that is, the intelligent but unconscious *use* of the instruments thus intelligently but (to the individual) unconsciously *constructed.* With the hypotheses and speculations of metaphysical theology and speculative philosophy, the inductive method has no sort of connexion—it is the great *fact* that alone concerns us, that there is inherent in the primordial cell of every organism, whether it be animal or vegetable, and in all the tissues which are developed out of it, an intelligent power or agent, which acting in all cases independently of the consciousness of the organism, and whether the latter be endowed with consciousness or not, forms matter into machines and machinery of the most singular complexity with the most exquisite skill and of wondrous beauty, for a fixed, manifest, and predetermined object—namely, the preservation and welfare of the individual, and the continuance of the species. This *quasi*-intelligent agent thus works with an apparently perfect knowledge of number, geometry, mathematics, and of the properties of matter as known to the human intellect under the term "natural philosophy" or physics—that is to say, with a perfect knowledge of chemistry, electricity, magnetism, mechanics, hydraulics, optics, acoustics—but as far transcending the limited knowledge of the human intellect, as the structures and adaptations of living organisms exceed in beauty and fitness the most finished works of man. Speculation apart, and the fact alone considered that such mental powers, so unconsciously acting, are inherent in every form of organized matter, it need no longer be considered novel or surprising that the unconscious operations of the human cerebrum attain to the perfection they sometimes do attain, or that the blind instincts of animals are so complete, and display so much knowledge of the external world.

The relation between the machines of organisms thus constructed and their actual uses, manifested in reflex phenomena, is too immediate and direct to doubt that the construction and use depend alike upon the same cause. In further developing my views, I shall have occasion to bring forward ample proofs and illustrations of this proposition, but I may here state that, if we were to divide the two classes of phenomena, and assign different causes to each, as has been the custom hitherto, we should only wander away into the hypotheses of speculative philosophy and metaphysical theology, leaving behind us the firm ground of fact and induction, and excluding ourselves from the large and perfectly untrodden field of research which the doctrine advanced opens out to us. I therefore take it as an established principle, that the *quasi*-intelligent agent which operates in the construction of organisms directs the use of the organs constructed.

Having thus traced the intelligent construction and use of organs in living organisms to an unconsciously acting principle of intelligence, as the common source of both, and having identified the results of the unconscious use (or reflex phenomena) with the results of that form of *cerebral* action which is carried on unconsciously,—or, in other words, having shown that the latter are reflex in their nature, it follows, necessarily and obviously, that these reflex *cerebral* phenomena are dependent upon the operation of the same unconsciously acting agent which constructs organs—or, in other words, the unconsciously acting mind of the cerebrum, and the intelligent agent from which constructive and reflex phenomena originate, are identical in their nature and operation. This proposition is the logical and inevitable deduction from the premises; I may add, that it is the logical and inevitable induction from facts, as I shall shortly proceed to demonstrate.[*]

We have, then, an unconsciously acting principle of intelligence operating upon or through matter in three modes. 1. It moulds and compounds matter into living organisms according to a fixed, predetermined, and unchanging sequence of phenomena or plan, having for its object the good of the individual or of the species, forming machines to this end of great complexity and wonderful adaptability out of simple material elements, and arranging the living structures in such a way that these machines act with the greatest precision and fitness to the purpose for which they are constructed. 2. It *moves* and *regulates* these machines according to fixed, predetermined, and unchanging sequences of phenomena, one change necessarily exciting another by sequential association according to a pre-arranged plan, having for its object the good of the individual or of the species. 3. In animals endowed with consciousness, it acts upon the vesicular neurine contained within the cranium, which it has already constructed, according to a fixed and predetermined order of change, one change necessarily exciting another by sequential association; the *results* of which changes, or series of changes, are presented to the consciousness, and constitute, in part, at least, the phenomena of thought. This is a summary of the actual operations of the unconsciously acting principle of intelligence, irrespective of all theory.

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[*] In thus using the terms "unconscious," and "unconsciously acting," I mean them solely to indicate the mental state of the organism itself. An unconsciously acting principle of intelligence is not a new idea, paradoxical as it may appear, for so the soul itself has been designated by modern psychologists. Thus Morell, "The soul, as we have shown, is *prior* to consciousness. It exists *unconsciously* from the formation of the first cell-germ; it operates *unconsciously* throughout all the early processes of life; it acts *unconsciously* even in the greater part of the efforts which subserve our intellectual development."—Elements of Psychology, p. 75. Again—"The same principle which shows itself in the human organization—which gives form and feature to the body—which adapts all the organs to their several purposes—which constructs the nervous system as the great medium of mental manifestation—which implants the instincts and prompts the senses to their appropriate work—this principle rises in due time to a *self-conscious* activity, in which it can recognise its own Divine origin, and aspire towards its own equally Divine destination."—Ibid. p. 77. Consciousness is, in fact, but one form of *manifestation* of the principle of intelligence. I know of no one word which will *exactly* designate the latter; I, therefore, shall merely use that phrase, or that of unconscious agent. With this strict limitation I may even be permitted to use the phrase unconscious *mind*, synonymously with the phrase unconscious principle of intelligence; mind being, when thus used, synonymous with the "soul" of psychologists. The great source of misapprehension, as Morell remarks, is the notion which confounds the human soul with the human *consciousness*.

The next step in this inquiry is, to determine the relations which mind and its operations bear to the unconscious principle and its operations. For this purpose, the threefold division just given will be our best guide, for the operations of the mind may be classed also under three corresponding heads—viz., 1. It designedly seeks to subdue and mould matter to its requirements, using for its designings those mental powers or faculties generalized under the term *intellectual*, and which have a *knowledge* of cause and effect, or of the necessary order of events, as the basis of all their operations. 2. It regulates, by an act of will, the current of its thoughts, and the movements of its own bodily organs in their operation (whether mediately or immediately) on the external world. 3. In these processes of thought and of will it acts upon or through the vesicular neurine contained within the cranium, controlling by its means the action of the muscles, and through it attaining to self-consciousness and knowledge of the external world. The problem to solve is, what are the relations, or rather the phenomena, manifested in common by the two forms of intelligence?

First, as to the unconsciously *constructing* principle and its operations. Its phenomena may be considered from a twofold point of view—i.e., as they are manifested in the body itself, in relation with consciousness simply; or abstractedly, as the results of an intelligent agent, and in relation, therefore, with the intellectual powers or faculties of the mind. In regard to the influence of the *constructive* principle of organisms upon the consciousness, little is known, and, as to the majority, little can be known; for with regard to them, it is not possible to say whether consciousness exists or not. Construction, in the sense I use the term, is not limited to *development*, or the first formation of organs, but properly includes *nutrition* (which, strictly speaking, is a continual reconstruction) and separation. The state of the consciousness in *development*, so far as it is manifested in the developing organism, is clearly a state of pleasure. We know nothing of its existence in embryonic or intra-uterine life; but during the period of growth (in all mammals, at least) the operations of the unconsciously constructing principle are associated with physical enjoyment, or a pleasurable feeling of existence. The same condition is observed, but perhaps in a less intense degree, during the process of continued reconstruction, so long as the objects and intentions of the constructing principle are attained. Should, however, its predetermined plans be interrupted, by an imperfect constitution or supply of the nutrient materials, the general feeling of physical *well*-being is changed into one of *ill*-being. At the same time, special painful feelings are felt, in correlation with the efforts of the constructive principle, to obviate the interruption to its predetermined plans; and the sensations of hunger, thirst, want of air, of exercise, of repose, &c., are induced. With these are associated acts and efforts to attain the means by which the predetermined arrangements (which are those of the healthy state) may again come into operation, constituting the *instinctive acts*, or the so-called reflex phenomena, when directed to the external world; and the operations of the so-called VIS MEDICATRIX NATURÆ, when directed to the working of the inner system of machinery. It is not possible to separate these two classes of conservative phenomena, except in this way—i.e., as to the sphere of action of the unconsciously acting principle of intelligence; in respect to their object and origin, they are identical. The effort to supply fluid to the blood (the instinct of thirst), when it is wanted to carry off by dilution any saline or other ingredient through the skin or kidneys, is not different in its nature from any other effort to depurate the blood, when morbid agents have entered it or are retained within it.

I have stated, that in conscious animals the operations of the unconscious principle of intelligence are associated with a feeling of pleasure or well-being if normal, with a feeling of discomfort or suffering if abnormal. But I wish to include amongst conscious animals only man and the vertebrata; as to other organisms, it is as yet an open question whether they feel at all, or if they do feel, whether they feel both pleasure and pain. The phenomena of consciousness are only known to the consciousness. Doubtless the inferences which a man draws from his own experience, as to the feelings of other men, are in the main correct; and in admitting *mammals* and *birds* to brotherhood with him in respect to physical happiness and suffering, he is not far wrong; but it is not correct to lay down as a proposition, that a manifestation in organisms of the *external signs* of happiness and suffering usually manifested by himself, prove that the *feeling* of happiness or of suffering is experienced by them; or that such manifestations, and none other, are alone proofs. *Articulata* are popularly believed to feel acutely; plants are thought to be devoid of feeling altogether; yet the same class of phenomena are manifested by the latter as by the former, through the working of the unconscious principle of intelligence, the real difference being only in the organs and mode in and by which the phenomena are manifested. There is the same intelligent adaptation to circumstances; the same pre-arrangements for the same great objects; the same efforts for the conservation of the individual and the species under varying circumstances; and therefore fundamentally the same instincts. The difference is in the infinite variety of the means and modes. If we compare our own feelings with those of lower animals, we may reasonably admit that they at least enjoy life; for as to our *viscera*, the organs of vegetative life (which in them are pre-eminent), we have no other state of mind than a dim feeling of pleasant physical existence. When they are diseased or injured, we experience acute pain, not referred to anything external, and certainly more acute in proportion as we ascend from savage or uncultivated life, and much more acute, apparently, than in the lower vertebrata. But it is noteworthy, that the pain hardly dwells in our memories. Perhaps in the articulata there may be a dull sense of pain when injured, but no *memory* of pain; so that there is no *fear* of it; and what is felt is limited to the actual *moment* of injury. As to the vegetable kingdom, it is as reasonable an induction, that its members also enjoy life—possibly a *painless* existence—as that they have no consciousness whatever.

However this may be, it is quite certain, that in all conscious animals endowed with a nervous system, without any exception whatever, the special seat of both conscious and of unconscious mind is in that system, or in some part of it. Here, then, is something more than analogy, for there is identity. But since the development of the nervous system itself is the work of the unconsciously constructing principle of intelligence, and is formed by it with a special adaptation to the uses of the conscious mind, its structure does not fundamentally differ from the organized tissues equally so adapted which are devoid of nerves or nervous system. The contrary opinion is an error, which has broadly separated vegetable from animal organisms, and which has given rise to the hypothesis, that the lowest forms of the latter possess a "diffused" nervous system, microscopically small, or even invisible; it being a notion that the functions of these animals can only be carried on by something of the kind. It is now established, however, that these consist, like the analogous vegetable organisms, of simple cells. It follows, therefore, that the *protozoa* and *protophyta* contitute the *dynamical* types of the essential portion of the nervous system—the ganglionic cells in defined groups—or the vesicular neurine, in which the action is probably direct from cell to cell. The point of importance in vegetables is the division of labour amongst the cells, some secreting colouring matter; others, starch, gum, sugar, oil; and another, the material for reproduction. Still, all combine to a common purpose—the well-being of the plant, and the continuation of the species.

In the higher animals, and in some vegetable organisms, the functions are more specialised, and are carried on by special apparatuses constructed for the purpose. Food is assimilated by one class,—is carried, thus assimilated, to the molecular tissues by another; the results of waste and repair are various, and are carried off by various machinery adapted to the purpose; the germ-cells and sperm-cells are developed also in special tissues—the reproductive organs. There are also weapons for the defence of the organism; apparatus for the prehension of food, and for its mechanical division and preparation previously to assimilation; apparatus for the supply of the oxidizing material; apparatus for the *intus*-connexion of the sperm-cell and the germ-cell, &c. All these require to be combined in action for the attainment of the objects of the organism as a unity, and we have therefore a special apparatus formed for this end, in which that unconscious principle of intelligence, previously (and still, indeed) present alike in all cells, is now specially localized; this apparatus is the nervous system.

The *use* of these various machines and apparatuses, according to a predetermined and fixed plan, is termed *instinctive.*

We have already divided instinct into that which acts consciously, and that which acts unconsciously. Now instinct, in reference to cell-life, may also be divided into the individual and the composite. In the simpler forms of vegetable and animal life the individual existence is *perhaps* typified by the unicellular organisms; it is more certain that the higher animals which are evolved from a single cell are strictly *individuals*—that is to say, indivisible. The composite forms of vegetable and animal life—as yeast, hydras, the diplozoon paradoxon, the various compound entozoa, &c.—are perhaps rather *societies* of unicellular organisms than compound individuals. Be this as it may, it is in the organisms evolved out of a single cell, and in which all the separate organs are co-ordinated to the common object of the organism, that we have the first undeniable example of the *individuum.* Unity manifestly, therefore, precedes consciousness, and is, of necessity, the fundamental or primary idea of the unconscious principle of intelligence. If, then, there be a co-ordinating apparatus, by the operation of which all the separate organs are co-ordinated to the common object of the organism, it necessarily follows that that apparatus must constitute the centre of unity, or of the individual, and therefore the seat of the *ego,* if self-consciousness exist. This has been fixed hypothetically by some physiologists in the medulla oblongata.

Inasmuch as the nervous system, in virtue of its predetermined structure, is the source of the infinitely varied manifestations of intelligence in *action,* and the centre of co-ordination, so also is it the seat of that *great conservative idea,* for the attainment of which co-ordination takes place, inasmuch as the sole object of the entire arrangement is the well-being of the individual or of the species. Since what is true of the whole, is true of every part thereof, it follows that the nervous system is also the seat of all those *quasi*-mental or instinctive powers by which the unconscious mind attains its ends. Now, as the mind has, in summary, the same ends in view, it is absolutely necessary to inquire into the nature of these fixed arrangements of the vesicular neurine on which the instinctive acts depend, and their relations to consciousness.

It has been shown, that in the *construction* of the various necessary apparatus and instruments by which the great conservative idea is carried out into action, there is manifested a profound *knowledge* of numbers, geometry, mathematics, and of every department of natural philosophy; that is to say, all that the human mind knows of pure and mixed science (and, indeed, infinitely more) is *applied* to constructive art. If we investigate the working of the apparatus thus scientifically constructed, we find that they also are all *used* with an apparent similar knowledge. I refer more particularly to those instincts and instinctive actions in which either the *natural* instruments are used, exclusively and primarily, or else secondarily, for the *construction* of other means of conservation of the latter. No better illustration need be given than that familiar to naturalists, of the mathematical knowledge with which the domestic bee, as a formative artist, constructs its comb. The problem for solution is, to construct the cells with the greatest strength, in the least space, and with the least expenditure of material—the daily problem of the human architect. Now this problem is solved by the bee, by selecting the hexagon as the geometrical form; by placing the cells base to base; and by causing the base of each to rest against the point where these partitions meet; thus saving materials and labour, and following out most exactly the principles of solid geometry.

It is a curious mathematical problem, Sydney Smith remarks, in his Lecture on the Faculties of Animals and of Men, at what precise angle the three planes which compose the bottom of a cell ought to meet, in order to make the greatest possible saving, or the least expense of materials and labour. This is one of those problems belonging to the higher parts of mathematics, which are called problems of *maxima* and *minima*. It has been resolved by some mathematicians, particularly by Mr. Maclaurin, by a fluxionary calculation. He has determined precisely the angle required; and he found, by the most exact mensuration the subject could admit, that it is the very angle in which the three planes in the bottom of the cell of a honeycomb do actually meet. Of course, all this knowledge is no part of the consciousness or experience of the insect, yet "it would take a senior wrangler at Cambridge ten hours a-day for three years together, to know enough mathematics for the calculation of these problems, with which not only every queen bee, but every undergraduate grub, is acquainted the moment it is born." I shall presently give an analogous illustration of the application of solid geometry by the unconsciously *constructing* mind to the construction of the *perfect* human form.

The *instinctive use* by the *individual* of the apparatus supplied to it ready made by the unconscious mind, has been always considered as something distinct from the *instinctive construction* of new or more fitting apparatus. From what I have already stated, it follows that there is no fundamental difference in the origin and nature of the two classes of phenomena; one or two illustrations will, however, set the matter in a clearer light. It is matter of common observation, that plants and animals are gradually adapted to any new external circumstances by *structural* changes in the organs of external relation. The leaves, e. g., of the *Ranunculus aquaticus,* differ in structure according as they are above or under the water. If above, they become enlarged and simply lobed; if below, they are more finely cut. If, however, the plant, growing in a moist soil, is not overflowed, then the leaves are so developed, in adaptation to the new circumstances, that a new species, the *Ranunculus hederaceus,* is constituted. The same kind of adaptation to external circumstances is exhibited by almost every kind of animal; the more remarkable and obvious being those in which changes in temperature and climate have to be provided for. Thus, we have hair changed into wool in a cold climate, or wool into hair in a hot; so also the variations in the colouring matter of animals. These facts are familiar to naturalists, and are those which Lamarck has generalized into a system in his 'Philosophie Zoologique.' It is of importance to remember that this instinctive construction is not limited to changes in the leaves, limbs, &c., of organisms, but extends also to the co-ordinating apparatus, so that new instincts are developed in lower animals, and "habits," and new sources of pleasure, in man. To this category may be referred, indeed, every phenomenon of this kind, including the acclimatization of animals and vegetables, the production of varieties by domestic culture, &c.

With the development of new vesicular arrangements, new apparatus, and new instincts, or instinctive actions, there is not unfrequently a repression, suppression, or deprivation of some of those which belong to the original type of the species. It is worthy of notice, however, that they are never absolutely eradicated; for when the appropriate stimuli (long absent from the race, perhaps) are again applied, the corresponding instinct reappears. As an illustration the following may be mentioned. The straw which has been used for bedding the *carnivora* in Wombwell's menageries is sold, and is capable of further use. Straw that had bedded the lions was made into bedding for some horses, and the latter immediately showed signs of alarm on entering the stable, snorting, snuffing, and trembling at the unwonted odour. Now it is certain that for many generations the English horse has had no experience of these his natural enemies, and his instinct of self-defence as regards them never exercised; yet the predetermined arrangement of the vesicular neurine in connexion with the sense of smell and the preservation from violent death was still there, and was duly brought into action so soon as the stimulus to which the arrangement is adapted was duly applied. Numerous similar examples of the persistence of these fixed arrangements might be adduced from the natural history of domestic animals, whether retained in the society of man, or passing again into a wild or half-wild life.*

As illustrative of the common source and nature of the instinctive use and construction of organs, I may mention changes in the colour, form, &c., of animals occurring under the immediate influence of instinct; as when concealment is desired, either to avoid enemies or seize prey. Insects, fishes, reptiles, birds, in numerous instances assimilate their colour to that of surrounding things, or change their colour (as the chameleon) in a moment. The loss and reproduction of limbs under the influence of the instincts belong to the same class of phenomena.

The habits of the solitary wasp, referred to by Sydney Smith, is an apt illustration of another point of view of this matter, inasmuch as it shows instinctive *action* in one form of organism taking the place of instinctive *construction* in others. In numerous animals, as well as in vegetables, the primordial cell is imbedded in a nutrient material contained within a shell or case, the whole constituting the egg or seed. The yolk of the egg (the nutrient part) is not only expressly adapted chemically to the wants of the growing animal, but is also exactly proportioned in *quantity*, so that when it is exhausted, the young being can either obtain food for itself, or is supplied by its parent. In mammals the ovum is placed in the uterus, and is supplied by the circulating system of the parent with nutrient material. In many of the *hymenoptera* the whole business of the active life of the insect consists in the carrying out of these ends of the unconscious principle of intelligence. The construction of the case or receptacle for the ovum, and the filling it with provisions, manifest some of the most singular and interesting efforts of the reproductive instinct. As a special illustration may be mentioned that of the solitary wasp, which supplies to its ovum both a case and a suitable nutrient material. She digs several holes in the sand, in each of which she deposits an egg. Next (I quote Sydney Smith),

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* A sheep farmer has just stated to me an illustration of this principle, which I mention as showing the practical bearing of these views. Complaining of the loss of lambs he had experienced in consequence of the cold spring, I asked why he had not suitable lying-in hospitals constructed for the ewes, and he replied, one reason was, that only the Southdown (the highly-bred ewe) would submit to restraint. The ewe of the *Cheviot* breed, and of the black-faced or mountain sheep, *would* wander away to drop her lamb by herself, and was not easily restrained. The latter also display an impatience of being touched or handled by man, which the more civilized Southdown never manifests. Their semi-wild state on the mountain and moor pasture is clearly the source of these peculiarities.

> "She collects a few green flies, rolls them up neatly in separate parcels (like Bologna sausages), and stuffs one parcel into each hole, where an egg is deposited. When the wasp-worm is hatched, it finds a store of provisions ready made; and, what is most curious, the quantity allotted to each is *exactly sufficient to support it* till it attains the period of wasphood, and can provide for itself."

This instinct of the parent wasp is the more remarkable, as it does not feed upon the food it supplies to the ovum. An analogous instance of constructive development is seen in the economy of bees, when a queen or prolific female is wanted to be developed, and the bees supply certain larvæ with a special kind of food suitable to produce the required effect, the latter not being able to obtain it for themselves under the guidance of their own appetites. In short, it may be stated generally, that bees possess a power in the management of their offspring far beyond the power of man; for, by virtue of their instincts, they can develop them into males, females, or neuters, as the wants of their society demand. Strictly, a hive of bees is analogous to a composite animal, for these remarkable reproductive instincts are nothing else than the means by which the objects of the unconsciously *constructing* agent are specially attained in the individual. That which in vertebrates is secured by the laws of embryonic development, is attained in the hymenoptera (and indeed in *insecta* generally) by the instincts of the individual, or the society.

Another form of instinct remains to be noticed—namely, the adaptive direction of apparatuses and instruments already formed to the attainment of the wants of the individual under *new* circumstances. The class of acts thus caused have been designated *rational*, or adduced as instances of *reason*. They are, I think, not such in the common meaning of the terms. Mr. Gardner records, in his 'Travels in Brazil,' the following instance of apparent reason in a crab, a small species, belonging to the genus *Gelasimus*. It was either making or enlarging its burrow in the sand, and about once in every two minutes it came up to the surface with a quantity of sand enclosed in its left claw, which by a sudden jerk it ejected to a distance of about six inches. Mr. Gardner threw a small shell into its hole, others remaining within a few inches of it. In about five minutes the crab brought up the shell, and carried it to a distance of about a foot from its burrow. Seeing the others lying near the mouth of the hole, it immediately carried them one by one to the place where it placed the first, and then returned to its labour. In this and numerous similar instances, common to all animals, a higher manifestation of the unconscious soul is shown than occurs in those which are in immediate and direct dependence upon fixed arrangements in the vesicular neurine. It is the connecting link between instinct and reason; but it is not a manifestation of the knowing and willing *self*-conscious mind. In man, numerous similar acts are manifested during infancy and childhood.

It is obvious, then, that the unconscious soul, when constructing the co-ordinating apparatus, whether during development or in after-life, writes within it, as it were, its own principles of knowledge; and thenceforward the nervous system acts as wisely and as sagaciously as if endowed with mind, in all those actions which are independent of the will or the reason. The invariable sameness and permanence of the instinct in successive generations (the external circumstances being the same in each generation), and the transmission of acquired instincts and habits (the circumstances being different), constitute a strong argument in proof of the doctrine that they are dependent on special arrangements of the vesicular neurine—an argument confirmed by the numerous vivisections instituted to demonstrate the nature of reflex phenomena, all of which establish the fact, that *integrity of structure of the vesicular neurine* is the essential requisite to reflex movements. These special arrangements I have already designated the substrata of psychical phenomena.* These combinations or masses of nerve-cells are subject to the ordinary laws of quasi-mental action according to a fixed plan, whether they be formed during the life of the individual, or acquired by hereditary transmission; they have equally their appropriate stimuli, their appropriate progressive development, or their retrogressive change; and, singly or in combination, they may lead to the evolution of new masses of vesicular neurine, and new modes of mental action. Whatever may be their course, however, these arrangements of the vesicular neurine correspond in function (sensorial or motor) to the ideas, conceptions, and intentions of the unconscious mind. To the conscious mind of the organism their relation is wholly this—namely, that they enable it to attain to that which it desires, or to avoid that which it dislikes. If the appropriate stimuli be carried to the vesicular neurine and awake it into its proper functional activity, this vital machinery is duly put into operation. The corresponding change in the state of the consciousness is this, that if the stimuli reaching the vesicular neurine be in harmony with the modes of action writ upon it by the unconscious principle of intelligence, and changes follow in harmony with the objects it has in view, a feeling of pleasure is induced; but if the stimuli be not in correspondence with the fixed pre-determined mode of action of the vesicular neurine and with the objects of the unconscious mind, pain or unhappiness results. This is, I think, an accurate general statement of the knowledge we have as to the relations of the inner working of its organ to the consciousness.

Our next step brings us into the field of human neurology and psychology. The unconscious soul of man, acting within the cerebrum, has its substrata—placed there *ab initio*, or constructed anew. What are they? and what are their relations to the consciousness? We shall find that the two forms of mental manifestation have a common origin and a common substratum, and that *the human mind is none other than the unconsciously working principle of intelligence individualized, become conscious of its*

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* On the Reflex Function of the Brain: § 8. The Substrata of Psychical Phenomena. British and Foreign Medical Review, vol. xix. p. 308.

own workings in the cerebrum, and deriving its ideas from its own constructive or material changes in the organ of mind. This proposition I shall now proceed to demonstrate by a series of illustrations. First, as to consciousness itself.

The mind is One—a unity. "The unity of consciousness is at once the deepest, rarest fact of our nature, and the most rigid condition for a complete mental philosophy."*

This unity is to be found in the identity of the conscious and unconscious mind. I have already shown that, as regards the latter, the organism is an *individuum*, and that, therefore, unity is its primary idea and prime object. It is thus the *self*-conscious mind exists; its own existence as an *individual*—as a unit—implies the idea of its existence as a something distinct from everything else. This is its fundamental intuition or conviction. This conviction it retains so long as the co-ordinating apparatus within the cranium duly and normally fulfils its functions; if, however, these be interrupted, then the state of unconsciousness supervenes—or, in other words, consciousness (and therewith *self*-consciousness) is abolished. The exact locality in the encephalon which is the seat of consciousness—or, in other words, the centre of corporeal and mental unity—fixed by some in the medulla oblongata—is still undetermined; but that there is a central point, composed of vesicular neurine, in which the sum total of the functional activity of the organism is felt, and whence there is a reaction (reflex action) upon all the structures which minister to the physical well being of the organism, is as certain as that every organism is *developed* from a common centre—the primordial cell.

Writers use the term *double* consciousness in reference to certain states of the mind in which the individual manifests, as it were, two distinct forms of mental life. A more correct term would be *alternating* consciousness, since it is most probable that the phenomena depend upon alternating independent action of each half of the cerebrum; but whatever may be the explanation, it is certain that the phenomena in question can never establish the doctrine of a *duality* of consciousness. Sir H. Holland appears to have set this point at rest.†

The unity of consciousness implies another fundamental principle—namely, that the varying states in which the latter exists are *successive*, and not contemporaneous. The mind cannot be occupied with two objects at identically the same moment. To assert the contrary proposition (a popular error) is to assert that the consciousness is divisible; whereas its unity implies its *indivisibility*.

"Sensation is not the object of consciousness different from itself, but is the *consciousness of the moment;* as a particular hope, or fear, or grief, or simple remembrance, may be the actual consciousness of the next moment. In short, if the mind of man, and all the changes which take place in it, from the first feeling with which life commenced, to the last with which it closes, could be made visible to any other thinking being, a *certain series of feelings alone*—that is to say, a certain number of successive states of the mind—would be distinguishable in it, forming, indeed, a variety of sensations, and thoughts, and passions, as momentary states of the mind, but all of them existing individually and successively to each other."‡

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* Morell: Elements of Psychology, p. 19.

† Chapters on Mental Physiology: chap. viii., On the Brain as a Double Organ.

‡ Brown: Lectures on the Philosophy of the Human Mind—On Personal Identity.

I know of no inquiry into this part of mental physiology more lucid or more instructive than Sir Henry Holland's, and to his chapter On Mental Consciousness in Relation to Time and Succession, I would specially refer the reader.*

The unity of consciousness implies another fundamental principle—that whatever changes in the vesicular neurine are presented to, or reach, the consciousness, and excite therein feelings, sensations, ideas or thoughts, are accompanied with a conviction of *truth* and *reality* as to the latter, whatever may be the source of the change; that is to say, whether it arise from morbid or healthy cerebral action.

> "When we speak of the *evidence* of *consciousness*," Brown remarks, "we mean nothing more than the evidence implied in the mere existence of our sensations, thoughts, desires, which it is utterly impossible for us to believe to be and not to be; or, in other words, impossible for us to *feel* and *not to feel* at the same moment."

Now, the ideas which are continuously and fixedly thus believed, in all normal states of the mind, are those termed *intuitive* truths, *innate* ideas, &c. They are dependent upon fixed and, in normal states of the cerebrum, unchanging arrangements and modes of action of the vesicular neurine; being such, they are writ upon the organism by the unconscious soul itself, are therefore its fixed and unalterable truths, and are to the human mind the *intuitions of pure reason.*

But what if the cerebral structure be disordered, either as to its vesicular arrangements, or its modes of action? Abnormal states of the consciousness will be induced; but, so long as consciousness exists, the mind will still feel convinced that the representations to the consciousness, which are presented in these disordered modes of action of the vesicular neurine, are real and true. The most common illustration of this fundamental principle is the state of the consciousness in dreaming, in which, as every one knows by personal experience, ideas the most absurd and the most incongruous as to time and space, are fully and indubitably believed. In artificial reverie, induced by the so-called electro-biological processes, an analogous state of the vesicular neurine and of the consciousness is induced; so also in artificially induced somnambulism, spectral illusions (clairvoyance), &c. In these the disordered action of the vesicular neurine is wholly functional and transitory; but in the delusions of the monomaniac they are permanent, and hence it happens that whenever that portion of the vesicular neurine which, in him, is the seat of the morbid action, is brought within the series of changes then being presented to the consciousness, the normal and therefore true succession of ideas is interrupted, and the abnormal and false occupy the mind fixedly, and, for some moments at least, to the exclusion of all others. This morbid presentation to the consciousness comes (like all others) with all the reality of truth, and, in proportion as it is continuous in time, it occupies the mind; for it is only by the constant succession of these changes in associated sequence, that erroneous ideas are corrected. Erroneous states of consciousness probably occur at many moments of our waking lives; not one of our senses is to be depended upon; but there is a pre-ordained mutual control and correction of each other in healthy action, which is destroyed in dreaming and other abnormal states of the cerebrum. The detection of monomaniacal delusions is sometimes difficult, because the patient, being keenly conscious of his infirmities, will conceal them; if, however, by what is termed the association of ideas, the morbid action of the vesicular neurine be brought within the current of his thoughts, he becomes utterly powerless to resist it—as much so as the electro-biologised to resist the suggestions presented to their minds. The formation of these monomaniacal *substrata* is due (as all observation shows) to the fixity of the mind on one idea or class of ideas, at a time when, from morbid changes induced in the vesicular neurine (as by undue mental labour, intense emotional excitement, want of repose, the development of a dormant predisposition, and the like), it is unusually susceptible of the operation of the unconsciously constructing mind; so that the fixed ideas become deeply writ, as it were, on the vesicular neurine, just as acquired instincts, habits, &c.; and are, in fact, as difficult to remove.

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* Chapters on Mental Physiology, &c., p. 46 et seq.

The intuitive conviction of *continuous* existence in time and space, known as the feeling of *personal identity*, has a more complex origin than is usually laid down. It implies two fundamental requisites—namely, a *perception* of the external world and *memory*, together with all their dependent faculties and modes of action. In that state of the consciousness which is a feeling simply of pleasure or of pain, there is no reference to the external world; in the higher state of *self*-consciousness, there is the latter necessarily, because the unconscious mind provides, by its inner vesicular arrangements, for the external world. It not only aims at the well-being of the organism, but provides, by its predetermined plan of construction and action, for the acquisition from without of what is beneficial, and the expulsion or repulsion of what is obnoxious. This is what the unconscious mind aims at; it follows, therefore, that as the conscious mind it desires them. The completion of the desire is accompanied by a feeling of pleasure, inasmuch as that completion is in congruity with the predetermined arrangements of the unconscious mind, which feeling is termed *satisfaction*, joy, pleasure. The desire to attain the good is usually termed *desire*, simply to avoid the evil is termed *abhorrence*. Now just as the unconsciously constructing principle of intelligence adapts the inner vesicular arrangements to external circumstances in plants and in the lower organisms, and so develops new instincts and instruments, so also, during the operations of the conscious mind, it constructs or arranges the vesicular neurine in accordance with its operations. These changes, whenever they are such that they can be presented to the consciousness, will come within the continually flowing series of states of the latter, which constitute the sum of mental existence; and being thus the unconsciously-written record in the vesicular neurine of the successive operations of the mind, constitute the material *substrata of memory*. The substrata, therefore, of acquired instincts, habits, &c., and of memory, are due to a common cause and common mode of action; the former, when transmitted, constitute, in fact, the *memory of the species*; the difference is in the relation of the respective substrata to the states of consciousness, and its relations to the external world.

It is not possible to comprehend the phenomena of memory without the concession of the doctrine, that the mind thus working unconsciously continually constructs or arranges the vesicular neurine of the cerebrum.

In his lucid chapter, "On the Memory as affected by Age and Disease," Sir Henry Holland mentions several interesting illustrations of the general fact—"That, of all the intellectual powers, it depends most on organized structure for whatever concerns its completeness, its changes, and decay," but has strongly experienced the absolute insufficiency of all theories founded on the connexion of memory with organization to explain several of its phenomena. It is, perhaps, in the doctrine I have just advanced that a more satisfactory explanation may be found. These substrata of memory are essential to the feeling of personal identity—i.e., of continued existence in time. The idea of continued existence includes the ideas of the past and the future. It is an intuition that we shall continue to exist, as well as that we have existed. Now this idea of the future is a fundamental idea of the unconscious principle of intelligence—equally fundamental as the idea of unity itself. Its aims and acts are all, without exception, *prescient;* the continued existence—i.e., the existence in time to come of the individual or of the species—is its great object. Hence, the infinite variety of prescient instincts displayed by all organisms, whether animal or vegetable; hence the instinct for continued existence, or love of life, and the universal abhorrence of death; hence it is that "men think all men mortal but themselves." In desire, the idea of the future is necessarily involved, whether it is a good we desire to acquire, or an evil we desire to avoid. The desire realized is the present, often too quickly to become a thing of the past.

Morbid conditions of the vesicular neurine develop correlative states of the consciousness in reference to these fundamental intuitions. *Neuralgia*—i.e., an ache or pain, simply dependent on a morbid state of a nerve or a ganglion of common sensation, and constituting a modification of the primary form of consciousness—is one. *Melancholia* is a higher morbid state in which evil is anticipated, or believed to have occurred; it is, however, precisely analogous to neuralgia in its nature. In the kind of dreams in which everything goes wrong, and in "low spirits," when all kinds of anxious fears are experienced, we have a condition analogous to the condition of the vesicular neurine in melancholia, only in the latter the condition is permanent, in the former it is transient. Melancholia has been termed phrenalgia by Guislain, and in one sense the term is correct; it is a term of doubtful meaning, however, for it may imply that the sources of the states of consciousness grouped under the term are in the mind itself; whereas they spring from morbid modifications of the vesicular neurine. The state of consciousness induced is precisely antagonistic to the aims and objects of the principle of intelligence, which is happiness, and to that experienced in the normal condition of the neurine: hence it is, that things pleasurable naturally become changed in their effects: rightly, therefore, the melancholic Hamlet says of the highest source of natural pleasure—"This most excellent canopy, the air, look you, this brave o'er-hanging firmament, this majestical roof fretted with golden fire, why, it appears no other thing to me than a foul and pestilent congregation of vapours." In the same way it is that, in neuralgia, impressions ordinarily agreeable—as of light, sounds, touches—are the sources of acute pain.

Neuralgia, in its primary and simplest form, is pain only; but there are forms in which there are painful illusive sensations, as of pricking, stinging, burning, coldness, &c.; in these there is a reference to a cause external to the organism. Closely related to these, are the illusions of the hypochondriac as to his bodily sensations, and as to morbid states of his viscera: and in intimate connexion with these latter are those morbid states in which there are delusions as to what may be termed the anatomy and intimate construction of the body or its parts. Thus, melancholic patients will assert that they have no stomach, no bowels, no head, no soul; that they, or some portions of them, are made of butter, glass, or something else easily destructible. They will have delusions as to their personal identity, as to their preservation in general (fear of death, vague apprehensions); or as to their danger from particular sources of injury (suspecting melancholia). Now, just as in neuralgia there is a complete perversion of the predetermined respondence to impressions, so in melancholia there may be a complete perversion of the predetermined instincts and modes of thought; and the trembling melancholic—who expects and dreads his death, flies from the most trivial things, in terror of death at every moment—becomes profoundly suicidal. The transition from a morbid condition of this kind to that in which the active instincts of defence are roused, is a natural and not unfrequent occurrence, so that the suicidal is often a homicidal maniac; or else the nutrient instincts are involved, and the hypochondriacal dread of being poisoned passes into the maniacal determination to take no food, or to take poison. This doctrine of the pathology of melancholia is equally applicable to all forms of the disease.

The preceding illustrations of the relation which the instincts and emotions bear to the vesicular neurine, and through it to the unconscious principle of intelligence, are, I trust, amply sufficient to show the exact correlation between the latter, and conscious mind in all modes of thought and states of consciousness in which the instincts, emotions, and passions are predominant. I will now submit illustrations taken from the domain of the intellectual powers, and will select two points of special and comprehensive importance—namely, reason, or intelligence, itself, and intellectual pleasure, or happiness.

An act of the reason implies a knowledge of the qualities of matter; the primary idea, therefore, of the intelligence, must be the intuitive idea that matter exists. Now the external world, and the qualities of matter in relation to the organism, constitute the study, if the phrase may be permitted, of the unconscious mind; correlatively, therefore, these are the study of the conscious mind. The first rise of the *ego* of self-consciousness is in the perception of that which is not a part of the individual, or external to it. The body is a unity that it may be the more effectually protected from external injurious agents, and secure its well-being and the happiness of the soul which it clothes. The evolution of all the apparatuses and instruments of sense, in particular, has the special end in view of placing the seat of unity and consciousness in instantaneous and intimate communication with the external world, through what may be termed prolongations, or projections outwards, of the vesicular neurine; for the nerves of special sense are virtually nothing else than portions of the grey matter spread out on apparatus suitably constructed for the reception of the influences which matter can exercise upon the vesicular neurine of the cerebrum, itself also specially arranged for being influenced by them. All the nerves, therefore, of special sensation at least (or, in other words, *all* sensory nerves, exclusive of those which minister to pleasure and pain only), have a common function and common principle of action. They may be considered as nerves of touch. This being the fundamental aim and method of the principle of intelligence, it follows that all changes in the consciousness consequent upon changes in the sensorial ganglia are accompanied with the conviction that the sensations arise externally. As to tactile impressions, this may appear of doubtful application; but it must be remembered, that the entire body is external to the consciousness. It is probable, that in a perfect act of perception all the senses co-operate in the act, and erroneous ideas are prevented by that predetermined mutual control and combination to a given end which I have already referred to as part of the function of the vesicular neurine. In morbid states of the latter, as in neuralgia of a stump, the mind refers the seat of pain to a point altogether apart and external to the true seat, because there is no provision for a correction of the impression. In auditory or visual illusions, dependent on cerebral disease, the same result is observed if the person be insane; or, in other words, if the cerebrum be so disordered that the necessary correction cannot be made. This idea of *outness* is fundamental to all perceptions.

The ideas of *power* and of *causation* (or cause and effect) arise in the mind in the same way. We have seen that it is the aim or idea of the unconscious agent, in laying down the predetermined arrangements of the organization, that they shall invariably respond to the same stimuli; this idea is reproduced as a state of the consciousness, and is the idea that they will, for the future, so respond:

"Why is it, then," says Brown, "that we believe in that continual similarity of the future to the past which constitutes, or at least is implied, in our notions of power? A stone tends to the earth—a stone will tend to the earth—are not the same propositions, nor can the first be said to involve the second. It is not to experience, then, alone that we must have recourse for the origin of the belief, but to some other principle, which converts the simple facts of experience into a general expectation or confidence that is afterwards to be *physically* the guide of all our plans and actions. This principle, since it cannot be derived from experience itself, which relates only to the past, must be an original principle of our nature. There is a tendency in the very constitution of the mind, from which the expectation arises—a tendency that, in everything which it adds to the mere facts of experience, may truly be termed instinctive." (Op. cit., vol. i. p. 121.)

When a stimulus or impression has excited the functional activity of any predetermined arrangements of the vesicular neurine, to which it is adapted, the state of consciousness corresponding thereto is correlative with the idea of the unconscious principle of intelligence; now it is the aim of the latter that that effect should be so produced invariably, consequently that which invariably precedes a change in the state of the consciousness is connected in the mind with the idea of a *cause;* hence the idea of *causation.* Thus Brown:

"A cause is, perhaps, not that which has merely once preceded an event, but we give the name to that which has always been followed by a certain event, and, according to our belief, *will continue* to be in future followed by that event, as its immediate consequent; and causation, power, or any other synonymous words which we may use, express nothing more than this permanent relation of that which has preceded to that which has followed. . . . . To know the *powers* of nature is, then, nothing more than to know what antecedents *are* and *will be* invariably followed by what consequents." (p. 120.)

This is, in fact, the foundation of all science. Nature is nothing else than the predetermined arrangements in operation of the great creating and sustaining Intelligence, which it is the duty of man, as "*naturæ minister et interpres*" to know. The faculty by which he ascertains these invariable relations of phenomena to each other, is termed *comparison*.

I could thus go through all our fundamental ideas and all our intuitive truths, and show that in them all the states of consciousness of the self-conscious mind are correlative with the ideas manifested in organization by the unconscious mind; and that it is from the manifestations of the latter in and through the functional activity of the predetermined arrangements in the vesicular neurine, that all thoughts arise into our consciousness. There can be no doubt whatever, whether we consider the deductions to be drawn from observations of the form of men's crania, from the investigations of pathology and pathological anatomy, from the facts of comparative anatomy and zoology, and from the laws of embryology, or whether we consider the general laws of psychology as displayed in the operations of the unconscious mind—that, just as there is a differentiation in the tissues and structure of the body, to secure its well-being and continuance,[*] so also there is a differentiation in the co-ordinating apparatus itself, to secure a knowledge of the external world. The result of this is a constant localization and specialization of function, so that masses of vesicular neurine are progressively appropriated to the mental powers as they are evolved, extent of neurine being correlative, *mutatis mutandis*, with extent of manifestation of the power. In these masses there is the same fixed respondence to the appropriate stimuli, as in the ganglia with simpler endowments; the same correlation between the ideas of the unconsciously constructing mind and the consciously thinking mind; and the same relation between the appropriate respondence to stimuli of the neurine and the states of consciousness known as pleasure and pain. The fundamental modes of action of the human mind and its organs are really, therefore, INSTINCTIVE.

It is a remarkable circumstance, that while metaphysicians and phrenologists have alike almost unanimously advocated or adopted this doctrine, it has never been applied to the elucidation of the nature of mind, by constituting it the starting point of a *comparative* psychology.[†]

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[*] See Dr. Carpenter's Principles of Comparative Physiology, fourth edition, pp. 18—20, 38, for a statement and illustration of this fundamental process.

[†] I subjoin the following rather long extract from Sir W. Hamilton's Note A (p. 761), in his Dissertations, &c., supplementary to his edition of Reid's Works, on account of the vast importance of this doctrine to mental physiology and pathology: "An instinct is an agent which performs blindly and ignorantly a work of intelligence and knowledge. The terms *instinctive belief—judgment—cognition*, are, therefore, expressions not ill adapted to characterize a belief, judgment, cognition, which, as the result of no anterior consciousness, is, like the products of animal instinct, the intelligent effect of (as far as we are concerned) an unknowing cause. In like manner, we can hardly find more suitable expressions to indicate those incomprehensible spontaneities themselves, of which the primary facts of consciousness are the manifestations, than *rational* or *intellectual instincts*. In fact, if Reason can be justly called a developed Feeling, it may with no less propriety be called an illuminated Instinct—in the words of Ovid—

'Et quod nunc Ratio, Impetus ante fuit.'

As to [Reid's use of the terms being] an innovation either in language or philosophy, this objection only betrays the ignorance of the objector. Mr. Stewart (Essays, p. 87, 4to edition) adduces Boscovich and d'Alembert as authorities for the employment of the terms Instinct and Instinctive, in Reid's signification. But before Reid he might have found them *thus*

I will now examine into the source and conditions of *intellectual pleasure* in relation to the cerebrum, taking as a starting-point the doctrine that this organ is the seat of the intellectual instincts. It is necessary to the manifestation of these instincts in consciousness, that is to say, in thought and knowledge, that there be a predetermined arrangement of the vesicular neurine—psychical substrata—corresponding to each, so that when the appropriate stimuli reach it, the corresponding states of consciousness (or sequences—associations—of ideas) may follow. It is necessary to the *perfect* manifestation of these instincts that the aims, conceptions, or ideas of the unconscious mind be writ within the vesicular neurine. Now, we have seen that these are founded upon a profound (perhaps perfect) knowledge of the laws of matter, whether they be physical, chemical, or vital; it is, therefore, a necessary inference that the human cerebrum is, *potentially* at least, the seat of this knowledge; or, in other words, that by a suitable development of the material substratum, through the agency of the unconscious mind, the human mind *may* attain to this knowledge to a greater or less extent, and that the elementary principles of all branches of science may be more or less innate or intuitive. We have seen how the bee is an intuitive builder according to the most correct mathematical formula, in virtue of the same properties which we would assign to man. Now the first instinct of human nature, and perhaps the highest intellectual pleasure, is to seek after and attain to knowledge[*]—knowledge of the world around him, knowledge of himself, knowledge of his relations to his Creator and his fellow-creatures. He is ever endeavouring to know the order of nature, or the causes of things—i.e., what is the necessary antecedent to a consequent; for it is knowledge only which gives him the freedom he continually strains after, and the dominion over matter he would conquer. *Felix qui potuit rerum cognoscere causas* is the sentiment of every man. This general use of the intellectual faculties, and the happiness consequent on the right use, is strictly analogous to that general use of the corporeal organs which constitutes the sum of life, and is, when normally carried on, the source of the feeling of corporeal happiness.

The unconscious principle of intelligence, as a constructive agent, aims not at the good only—*τὸ εὖ*; ever conjoined therewith is the beautiful —*τὸ καλῶς*. In the conscious mind this aim at the beautiful becomes a *desire*, when the vesicular neurine is appropriately evolved. Hence it is that amongst the special intellectual pleasures of which man is capable of feeling, are those derived from the fine arts—namely, music, painting,

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applied by Cicero, Scaliger, Bacon, Herbert, Descartes, Rapin, Pascal, Poiret, Barrow, Leibnitz, Musæus, Feuerlin, Hume, Bayer, Kames, Reimarus, and a host of others; while, subsequent to the 'Inquiry into the Human Mind,' besides Beattie, Oswald, Campbell, Fergusson, among our Scottish philosophers, we have, with Hemsterhuis in Holland, in Germany Petens, Jacobi, Bouterweck, Neeb, Köppen, Ancillon, and many other metaphysicians, who have adopted and defended the expressions. In fact, Instinct has been for ages familiarized as a philosophical term in the sense in question—that is, in application to the higher faculties of mind, intellectual and moral. . . . . In a moral relation, as a name for the natural tendencies to virtue, it was familiarly employed even by the philosophers of the sixteenth century . . . . and in the seventeenth it had become, in fact, their usual appellation."

[*] There is an admirable little work on this subject, to which I would specially refer the reader, and the more earnestly because its value is not generally known—Sir John Forbes' Happiness in its Relations to Work and Knowledge: an Introductory Lecture, &c. Smith, Elder, and Co., Cornhill; or Churchill.

sculpture, architecture, and formative arts generally. These arts being practised by the unconscious mind in the construction of organisms, and in the instincts of lower animals, they present the best subjects for comparison and elucidation. Perhaps the human form may be reasonably assumed as the form the contemplation of which (when perfect) gives the highest intellectual pleasure. It may be considered under two aspects, first, as constructed by the principle of intelligence; secondly, as constructed by man. According to the doctrine I wish to establish, the psychical substrata (the work of the unconscious mind), by and through which the beauty of the human form is felt and perceived, will be correlative with the constructive ideas and conceptions of the unconscious principle of intelligence (or nature, as it is usually designated); so that when the visual impression of a perfect human form reaches substrata perfectly evolved, there is congruity between the latter and the former; and the resulting changes in the consciousness in reference to the visual object are accompanied by that change in the consciousness termed pleasure. The same doctrine applies equally to all artistic impressions derived from the results of true formative art, whether seen in vases and objects of vertû, or in the grander architectural products of human genius; to all æsthetic combinations of colour; to the infinite variety of sweet concords. The recipient senses having an analogous structure, and a common function in relation to consciousness, the ideas that enter the mind through them have a common relation to the feeling of pleasure.

These substrata will also regulate the successive states of consciousness in relation to the objects of intellectual pleasure, and through them, therefore, it is that the mind conceives, either instinctively (as genius), or deductively through knowledge, correct conceptions of those objects; and realizing these conceptions, works matter into artistic forms, harmonizes colour, or combines sounds; which results are perfect accordingly as they approach the model or archetype in the unconscious mind.

The human female, in the perfection of youth and beauty, is to man probably the most beautiful, and the most pleasurable, visual object in creation. Often, doubtless, the artistic feeling of pleasure is associated with the instinctive feeling; but many of my readers will agree with me in the statement that the one is often unalloyed by the other; and that an abstract perception of the beautiful is excited by this example of the artistic perfection of the constructing principle of intelligence. The physiologist can trace visually the formation of that example from the union of sperm-cell and germ-cell, constituting the primordial cell, to its complete evolution at puberty; and he sees nothing more, in any part of the process of formation, than a combination of cells, according to fixed never-varying rules—or, if varying, leading to imperfect results. To him the fundamental form is a hollow spheroid, or ellipsoid, or a combination of such; the fundamental process a constant combination, re-combination, and multiplication of them. Now, the geometrical rules by which these histological elements are finally combined together, or collated, by the unconscious mind into a form of beauty, appear to have been determined by Mr. Hay, of Edinburgh, who has been sedulously labouring for many years past to elaborate the true principles of beauty in formative and decorative art, just as the geometrical rules by which the bee constructs its hexagonal cells have been determined by Maclaurin. These rules are based on a law of harmonic ratio, "identical," Mr. Hay remarks, "with that by which, through the organs of hearing, the mind is æsthetically impressed with one of the most refined and delightful emotions which mere sensation is capable of exciting, and on which are necessarily based the fundamental principles of musical composition." Mr. Hay lays down, as his first position, that the eye is influenced in its estimation of spaces by a simplicity of proportion, similar to that which guides the ear in its appreciation of sounds; and, as his second, that the eye is guided by direction rather than by distance, just as the ear is guided by number rather than magnitude of vibrations. The basis of his theory is simply this:—"That a figure is pleasing to the eye in the same degree as its fundamental angles bear to each other the same proportions that the vibrations bear to one another in the common chord of music." As to these vibrations, we quote Mr. Hay on the sounds of the monochord.

"This is an instrument consisting of a string of given length stretched between two bridges standing upon a graduated scale. Suppose this string to be stretched until its tension is such, that when drawn a little to a side, and suddenly let go, it would vibrate at the rate of sixty-four vibrations in a second of time, producing, to a certain distance in the surrounding atmosphere, a series of pulsations of the same frequency. These pulsations will communicate through the ear the musical note literally signified by C, which would, therefore, be the fundamental note of such a string. Now, immediately after the string is thus put into vibratory motion, it spontaneously divides itself into two equal parts, the vibrations of each of which occurring with a double frequency—namely, 128 in a second of time, and consequently producing a note doubly acute in pitch, although much weaker as to intensity or loudness; that it will then, while performing these two series of vibrations, divide itself into three parts, each of which vibrating with a frequency triple that of the whole string—that is, performing 192 vibrations in a second of time, and producing a note corresponding in increase of acuteness, but still less intense than the former; and that this continues to take place in the arithmetical progression of 2, 3, 4, &c. Simultaneous vibrations, agreeably to the same law of progression, which, however, seems to admit of no other primes than the numbers 2, 3, 5, and 7, are easily excited upon any stringed instrument, even by the lightest possible touch. The musical sounds thus naturally produced are called the Harmonics. . . . . The musical note produced by the vibratory motion of the whole length of such a string is, as I have already stated, called (C), and is, consequently, the fundamental note or tonic to which all that follow in forming a scale must refer. The note produced by half of the string is the first harmonic, and is called the superior octave to the fundamental note."*

Now all solid bodies are referred to plane figures upon the retina, and are bounded either by curves or right lines. If by the latter, their outlines are portions of rectilinear figures; if the former, of circles, ellipses, &c. Each rectilinear plane figure has a curvilinear figure that belongs to it—that is, a figure which may be symmetrically inscribed within it; and since every rectilinear figure may be reduced to a triangle, and a triangle is measured by its smallest angle, so also may curvilinear figures be measured by the angles of the rectilinear figure to which they belong. The theory of the pleasing in form being "that the division of space into an exact number of equal parts will æsthetically affect the mind through the medium of the eye, in the same way that the division of the time of vibration in music into an exact number of equal parts æsthetically affects the mind through the medium of the ear," it follows, that the first step in demonstration is to show the correlation between the fundamental *notes* and fundamental *spaces*. Two straight lines cutting each other—that is to say, a perpendicular and a horizontal line—form at their junction a right angle; and if they be equal in length, and their points be joined by a curved line, equally distant at all points of the curve from the angle of junction, the curve measures one-fourth of a circle, or 90°. The angle (a right angle) is therefore an angle of 90°. This quarter circle corresponds to the monochord in Mr. Hay's theory, and is divided by him in the same numerical ratio that the vibrating monochord divides itself, as just explained; the result being a series of rectilinear and their corresponding curvilinear figures, measured by the angles thus produced, correlative with the fundamental notes. When the parts or vibrations that constitute a musical sound are multiples of the fundamental number by 2, 4, 8, &c., they are called *tonics;* by 3, 6, 12, &c., *dominants;* by 5, 10, 20, &c., *mediants;* by 7, 14, 28, &c., *sub-tonics*. So in plane figures. Divisions by 2, 4, &c., give tonic angles; by 3, 6, &c., dominant angles; by 5, 10, &c., mediant angles; by 7, 14, &c., angles of the seventh degree, or fundamental discord. These angles may be also represented by figures, thus: 90° being taken as 1, an angle of 45° is an angle of ½; 30° of ⅓; 22° 30′ ¼; 18° of ⅕, and so on. There is, therefore, a scale of harmonical angles exactly corresponding to a scale of harmonical notes; this Mr. Hay gives.* The tonic, dominant, and mediant notes produce, when combined, the most beautiful harmony; correlatively, the geometrical figures and forms of which the tonic, the dominant, and the mediant angles are the primary elements, are also the most beautiful of their kind. These views Mr. Hay applies to the Parthenon, to the leaves of trees, to flowers, and to the human form. Illustrations of these are given in the last-quoted work. His views have also reference to the identity of the laws of intellectual pleasure derived through the sense, quoting as to this principle a hypothesis of Sir Isaac Newton, thus expressed: "I am inclined to believe some general laws of the Creator prevailed with respect to the agreeable or unpleasing of all our senses; at least, the supposition does not derogate from the wisdom or power of God, and seems highly consonant to the simplicity of the macrocosm in general."

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* The Geometric Beauty of the Human Figure Defined, &c., 4to, pp. 6, 7. 1851.

To construct the human female form in perfect proportion, Mr. Hay takes the first eleven harmonic angles as they arise consecutively from a division of the right angle, which he adopts as the fundamental angle, and combines them geometrically. First he lets fall a perpendicular line, representing the height of the figure to be constructed, and from this line draws his angles, according to a system only to be understood by a reference to his treatises. The curves of the figure are portions of circles and ellipses, whose angles of inclination are simply those of ½, ⅓, ¼, ⅕, ⅙. The following is Mr. Hay's summary:

> "1st. That on a given line the figure is developed as to its principal points entirely by lines drawn either from the extremities of this line, or from some obvious and determined localities. 2nd. That the angles which these lines make

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* The Orthographic Beauty of the Parthenon, p. 21; also, the Geometric Beauty of the Human Figure Defined; to which is prefixed a System of Æsthetic Proportion. Appendix.

with the given line, are all simple multiples or sub-multiples of some given fundamental angle, or bear to it a proportion admissible under the most simple relations, such as those which constitute the scale of music. 3rd. That the contour may be resolved into a series of ellipses of the same simple angles; and 4th, that these ellipses, like the lines, are inclined to the first given line by angles which are simple multiples or sub-multiples of the given fundamental angle. . . . . Thus there is a perfect harmony of combination in its proportions, associated with as perfect a harmony of succession in its beautifully undulated outline, the curves of which rise and fall in ever-varying degree, and melt harmoniously into one another like the notes of a pleasing melody. When, therefore, we reflect that the scientific investigations of the anatomist have proved, that in the fitness of its parts the construction of the human frame exhibits the closest approximation in nature to a perfect development of mechanical science, and that the similar investigations of the physiologist have proved that the processes by which it is sustained in vital energy exhibit the closest approximation in nature to a perfect development of chemical science, it cannot in any way be surprising to find that, in like manner, and agreeably to a definite and acknowledged law, the beauty of its form discloses the nearest approximation in nature to a perfect development of the science of æsthetics."*

Through Mr. Hay's kindness I am enabled to give a woodcut, with the angles upon it, from a drawing taken by Mr. Houston, R.S.A., of a Scottish female employed in the Royal Scottish Life Academy as a model. All the points of this figure correspond, except the hands, which are a little larger (probably from hard work), and the waist, which has evidently been compressed by stays, with the theoretical figure. The real variation is in the national high cheek-bones and broad Scottish face of the living model. Professors Kelland and Goodsir also assisted Mr. Hay in carefully measuring six living models, the classic statue known as the Medicean Venus, and another as the Venus of Melos. The results corresponded so closely with the theory as to leave no doubt of its accuracy as to the living model, and to render it probable that a similar system constituted the basis of artistic education among the ancient Greeks.†

Fitness, strength, and beauty, are combined in the constructions of the unconsciously constructing principle of intelligence; these are the objects to be aimed at in architectural and the other formative arts. In one of his recent works‡ Mr. Hay demonstrates, by numerous measurements, that one of the most beautiful structures of antiquity, the Parthenon at Athens, was constructed on geometrical harmonies identical with those according to which the perfect human figure is developed or formed. The right angle (90°) is the fundamental tonic; taking this as the key-note, Mr. Hay theoretically re-constructs that grand architectural harmony throughout all its details; and then shows that the actual measurements correspond sufficiently near to the theoretical to demonstrate their identity.

In the application of geometrical ratio to architecture, Mr. Hay has had numerous predecessors; it is in selecting *angular* proportion as the basis of his harmonic system, instead of linear, and in applying his principles to curvilinear as well as rectilinear figures (especially the

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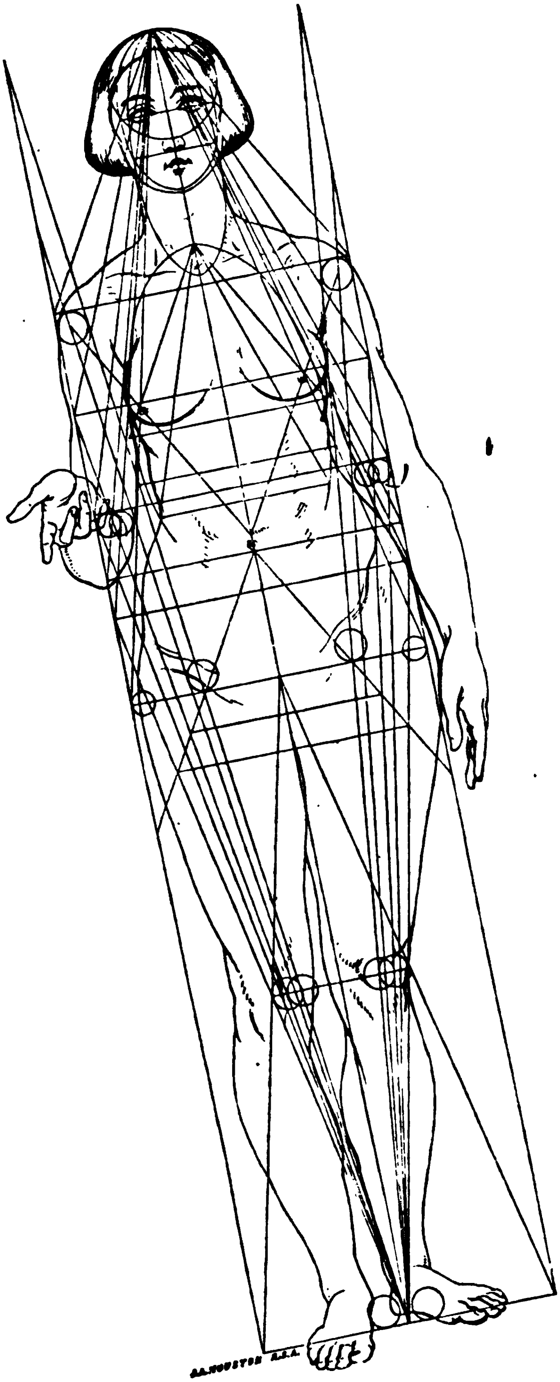
* The Natural Principles of Beauty as Developed in the Human Figure, by D. R. Hay, F.R.S.E., p. 23.

† In another and earlier work (1849), entitled, On the Science of those Proportions by which the Human Head and Countenance, as represented in Works of Ancient Greek Art, are distinguished from those of Ordinary Nature, 4to, Mr. Hay treats fully of this subject.

‡ The Orthographic Beauty of the Parthenon referred to a Law of Nature. 1853.


J.A. HOUSTON R.S.A.

composite ellipse), that he differs from them. Nevertheless, the geometrical harmonies derived from *linear* proportions have an extensive application, especially to Gothic architecture. In these the *three primary forms*—the equilateral triangle, the square, and the pentagon—are fundamental figures.

Mr. Griffith (who has illustrated this theory) terms the governing figure in his system of numerical rectilinear ratio, the *kleis* (κλεὶς, *clavis*, key), but he draws his analogies from chromatics rather than acoustics, and makes his three primary forms analogous to the three primary colours—yellow, red, and blue. The system not only evolves all the ornamental details as well as the ground-plan, but also the greatest strength and elevation; for the same geometrical lines which dictate the latter "indicate the direction of all the thrusts or forces, and their sundry workings."* The ratios in the rectilinear system are the same as in the angular; and curvilinear figures are deducible from the rectilinear.

In another work, published in 1845, entitled 'The Natural System of Architecture,' in which the theory is applied to both Greek and Gothic structures, Mr. Griffith examines and delineates the geometrical proportions of the following Greek temples (amongst others), the Parthenon, Erectheion, the Temple of Bacchus at Teos, of Themis at Rhamnus, and of Theseus. Amongst the Gothic structures are York and five other English cathedrals; and the Temple Church and King's College Chapel, amongst minor examples. Writers since Griffith have also taken up this subject, but on the same principles. We may infer, therefore, that the changes in the vesicular neurine, occurring during consciousness, have a definite relation to geometry and dynamics.

The views just advanced apply exclusively to the absolutely beautiful and true. Pleasure may be derived, however, from that which is relatively beautiful and true; and indeed this is the most common source of our pleasure. All special substrata, acquired either by inheritance or by the external relations of the individual, do modify the states of consciousness by the changes going on within them, when the appropriate stimuli reach them. To the former belong secret, "occult," or mysterious sympathies; to the latter the pleasures of memory. Thus it is, that in a foreign land to hear the familiar language of home is a delight, or even to experience any impressions associated with pleasurable feelings felt at home. It is in confounding these different sources of pleasure, indeed, that the greatest obstacle to a true system of æsthetics and a sound philosophy of morals exists.

Having thus shown the instinctive *nature* and *origin* of our intellectual faculties, I shall now illustrate their instinctive *action*. It has been seen that acquired knowledge is no essential part of instinct generally, neither is it of these faculties when working instinctively. That which *is* necessary is a full development of the psychical substrata appropriate to each, or phrenologically the cerebral "organ." Persons endowed with these, and who have put them into action so as to evolve results, are known by the term *Genius*. Functional *activity* is, however, necessary; that is to say, in all artistic conceptions there must be the power to *represent* either to the eye or the ear. Most men who observe the working of their own minds, are cognisant of a power to conceive far beyond a power to execute; whether it be to clothe their ideas in appropriate language, with due rhythmical cadence (of which poetry is but one form), or in appropriate combinations of musical sounds, or in the visual music and rhythm of sculpture and architecture. Often the power to execute is greater than the power to conceive; thus, persons who know not a musical note, will play on the piano any tune which they have heard once or twice. Mozart is an example of a true musical genius. When only four years old he began to write music in strict accordance with the rules of musical composition, although he had not been instructed in them. In after life he wrote music because, to adopt his own expression, he could not help it. So it was with an eminent English poet:

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* Ancient Gothic Churches, their Proportions and Chromatics. By William P. Griffith, Architect. Part II. p. 21.

> "I lisped in numbers, for the numbers came."

Instances of this kind could be multiplied to an almost indefinite extent.*

An illustration of the instinctive working of the *numerical* faculty may be added, to show that the doctrine is generally applicable. Mr. Roby, a banker at Rochdale, played, sang, composed, and was an amateur painter. His most developed intellectual instinct, however, was his powers of calculation, in which he was superior to Bidder, perhaps the most wonderful calculator this country ever produced. His widow states in his published 'Remains,' edited by her:

> "If a double column, twenty figures in each row, or a cube of six, were placed before him, he would tell the sum as soon as his eye could read the figures. He arrived at the result without going through the ordinary process; he saw it at a glance. If, as was rarely the case, owing to a passing fit of dulness, or a momentary distraction of thought, he failed to see the sum at once, he was rather slow than otherwise in doing it by the ordinary mode."

The preceding series of arguments and illustrations have brought the subject to the point from whence it was commenced—namely, the unconscious or reflex action of the cerebrum. Perhaps enough has been stated to establish these two prime truths,—1. That the unconsciously working principle of intelligence manifested in the construction and instincts of vegetables and of animals, is identical with the unconsciously working principle of intelligence manifested in the construction and functions of the human cerebrum; 2. That the human mind is none other than this unconscious principle of intelligence individualized, become cognisant thereby of its own workings in the cerebrum, and deriving its ideas from its own constructive or material changes in the organ of mind. To demonstrate more clearly the unity of origin and action of the two forms of intelligence, and the application of the doctrine to practical uses, I will now add some further illustrations, taking the intellectual instincts as a starting point.

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* Much knowledge might be gained from a careful observation of the instinctive working of these faculties. The following is an interesting fact taken in connexion with the preceding statements; it is from the Diary of Moore, the poet (edited by Lord J. Russell, vol. ii. p. 235): "Dined at Power's, to meet Bishop, the composer, who is one of the very few men of musical genius England can boast of at present. . . . The omission of the seventh and fourth, he says, is the characteristic of natural music; has often found, when he has been wandering wildly through the mountains of Wales, and has sung away without thinking *what* he sang, that he has invariably detected himself omitting the seventh and fourth." The following entry is also interesting, at p. 341: "Dined with Power, to meet Bishop ——. He mentioned a good story to prove how a musician's ear requires the extreme seventh to be resolved. Sebastian Bach one morning getting out of bed for some purpose, ran his fingers over the keys of the piano as he passed, but when he returned to bed found he could not sleep. . . . . At length he recollected that the last chord he struck was that of the seventh; he got up again, resolved it, and then went to bed and slept as comfortably as he could desire."

The appreciation of the Beautiful, in connexion with the pleasures of sense, is a familiar fact to the moralist and the philosopher. In man, it is first felt rightly with the complete evolution of the system, and when he is become capable of reproducing the species: just as, at the same stage of evolution, his beauty is most perfect. No idea of the unconscious principle of intelligence is more universal than this. In many of the phanerogamous plants the period of formation of the primordial cell (or union of sperm-cell and germ-cell—fertilization of the ovum) is marked by a display of grace of form and beauty of colour, in the appendages to the sexual organs, which it is man's highest ambition to rival successfully. These appendages are formed out of what are the analogues of the male organs. In the insect-world, the brief period of fertilization is also the period of perfect development; in some of these, as the *Lepidoptera*, there is a gorgeous decoration of the animal, and more particularly in the male. In fishes, birds, and mammals, puberty is also characterized by the development of ornaments more or less striking, but more especially on the male; scales brightly coloured, gorgeous feathers (as in humming-birds, and the Gallinaceæ), and horns, manes, beards, are of this kind. In the human female, the hair, the mammæ, and the subcutaneous fat, are undoubtedly analogous structures. The conscious mind displays in man a similar law of action; the gay attire of the lover, and the glories of bridal dress and decoration, are but evolutions of the same great idea of the unconscious mind.

While thus in creation the outward form is æsthetically a unity, so also are minor sources of sensation. Many animals are attracted by *scents* developed during reproductive activity—insects, fishes, and mammals, not excepting man; it is during this period that flowering plants give off their sweets. *Sounds*, of a more or less musical character, are emitted by insects, birds, and mammals, during the same period—perhaps almost exclusively by the males; in this respect the analogy (as to plants and the lower animal forms) is defective. In man, the taste for poetry, music, sculpture, and the decorative arts, is only fully developed with the evolution of the reproductive organs, while it is exalted as to one of these by their special activity. The ballad "to his mistress's eye-brows" of the lover, is the exact analogue of the song of the cicada, or of the male song-bird.

I have given a practical application to these views in an attempt to investigate the nature and origin of hysterical affections, and to this work I would refer the reader.* If the instincts of man, and vegetables, and animals, be collated in reference to the continuance of the species, they will be found to be inseparably connected with every kind of both æsthetic and constructive art, in every form of organism.

Ordinary dreaming, somnambulism, clairvoyance, delirium of every sort, insanity, and other forms of disordered cerebral action, are important changes in the states of the consciousness in reference to the representative faculty. There can be little doubt that these changes have their correlative changes in the vesicular neurine itself, although the demonstration is not easy. In the action of alcohol, chloroform, opium, Indian hemp, &c., on the blood, and, through the blood, on the cerebral tissue, we have, however, an undeniable proof that there are instances in which these changes in the consciousness *do* depend upon changes in the cells of the vesicular neurine, for the invariable connexion of antecedent and consequent is most clearly made out in reference to these. It is a doctrine generally entertained, that narcotic poisons have each a special action upon special portions of the encephalon; but I think there is considerable doubt to what extent, at least, this should be admitted. The difference may be rather in the mode of action than the locality selected; for it by no means follows that these poisons must necessarily affect the vesicular neurine so as to alter the states of the consciousness. On the contrary, it is exceedingly probable (if the proposition I have advanced be granted —namely, that the function of the nerve-cells is only the result of a specialization and evolution of a more general function inherent in all cells), that the latter participate with the former in the changes which the so-called narcotic poisons induce. That this is so with some of them is undeniable, and I will proceed to show this with reference to opium, hoping at the same time to demonstrate the *principles* (in opposition to our empirical knowledge) by which the administration of the drug should be regulated.

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* A Treatise on the Nervous Diseases of Women. Longmans & Co.

The first result of the action of opium on the tissues is to exalt the feeling of corporeal well-being; it is, therefore, congruous with the normal action of those tissues. Its power of actually sustaining the vital powers is well illustrated by the use made of it by messengers and others in the East, both for themselves and their horses, when they have to undergo prolonged labour with little sustenance. Acting upon the organs of self-consciousness and thought, it again exalts the feeling of pleasure in connexion with their action and the states of consciousness arising therefrom. To the wounded spirit it is described, by one who tried it largely, "as an assuaging balm;" and as building, out of the fantastic imagery of the brain, "cities and temples beyond the art of Phidias or Praxiteles—beyond the splendour of Babylon and Hekatompylos." Now this being the action of opium upon tissues wherein consciousness plays, we may infer that it has an analogous action on tissues apart therefrom; and this experience shows to be the case, for there is perhaps no remedy which more facilitates a return to normal action in those tissues when the seat of sloughing wounds, or when the vital reaction is below *par*, than opium. So, also, when the nutrition or vital action of the vesicular neurine is imperfect from like causes, as in asthenic neuralgia, the various forms of melancholia (especially those connected with excessive use or action of the organ), and the asthenic forms of delirium and delirious mania, opium is the most certain medicinal agent. Those who have studied these varied uses of opium empirically, will recognise the justice of these statements as to its widely-different therapeutic applications, and will readily understand that the common link which binds them together in one therapeutic category, is the unity of function of cells in relation to the predetermined arrangements of the unconscious mind. The irritability of a chronically inflamed mucous surface, and the irritability of a nerve or sensorial centre, are not essentially different pathologically; on each, opium acts medicinally in a way also not essentially different. I would call special attention to this point in my system, as one of exceeding value in therapeutics, for if that system be well-founded, we can interpret the so-called vital phenomena by those which involve consciousness, and *vice versâ;* for the latter being nothing more than the workings of the unconscious soul reaching the consciousness through a special apparatus evolved for the purpose, and the workings of the unconscious soul not reaching the consciousness, being vital phenomena, the one can be substituted for the other in our inquiries, so far as the bio-chemical changes in the tissues are involved.

I had intended to have illustrated the nature of the Will (a state of self-consciousness) by an application of these views to the phenomena of motion in organisms, whether animal or vegetable; this must form a subject for further and separate inquiry. As to the doctrines advanced, I may be permitted to say, that they really constitute only a small portion of a general system of mental philosophy, and are therefore of necessity presented in a fragmentary shape. In thus opening out a new and altogether uninvestigated series of related phenomena, I think it right to make some remarks which may be of use in explaining my views and guiding the thinker and observer.

I have constantly made use of the term unconscious principle of intelligence or *mind.* By that term I mean simply to designate that principle of intelligence which is manifested in *all* the phenomena of the universe, so far as they are known, and whether cosmic or organic, in virtue of which all things tend to Good. It is a principle, according to my views, as universally extended, as universally operative, as devoid of personality, and as certain and definite in its laws of action as the force of gravity, and is the primary and essential element of the conscious mind. I term it the *unconscious* mind because to us it so appears to be in its operation in organisms; for although there can be no doubt whatever that it proceeds from the great creative Intelligence, yet the laws of the inductive philosophy forbid us to investigate its relations to the Deity, since these are clearly beyond the reach of philosophical observation and experiment. Like the force of gravity, it is a property of matter, and like it, probably dependent upon an immediate volition of the Deity. Speculations as to its nature and relations have been current in every age, and need not be multiplied now. It has been conceived to be God himself; a doctrine which has constituted the foundation of Pantheistic and analogous systems of theology; or under the term Nature, it has occupied the place of the Deity in Atheistic systems; or in Deistic systems, has been viewed as a special moral agent. In Cosmogony, it has been considered as a *hylozoic* principle animating the world, as if the latter were an animal; or, in relation to natural history and physiology, has been considered as the *anima,* plastic nature (Cudworth), the Archæus, the vital principle, the *vis nervosa,* &c. All these speculations I wish to avoid, preferring to investigate its laws of action through its phenomena: these are twofold: the changes it operates in matter, in reference to the ends it has in view, as manifested by phenomena; and the changes in the states of the consciousness, consequent on those material changes. When these laws have been determined and settled, in part at least, we shall be in a position to determine more satisfactorily than hitherto, the relations of the self-conscious mind to organization, the nature of Truth, and the limits of moral responsibility; or, in other words, to establish psychology, metaphysics, and moral philosophy on a more definite basis.

I have repeatedly used the term psychical *substrata*. By this I do not mean to imply a certain material arrangement of cells or their elements only, but such an arrangement that a fixed order of successional changes, or plan of action, may be impressed upon them. Thus each primordial or embryonic cell has its psychical substrata, in virtue of which there is a continuous series of successional changes in a fixed, predetermined order, and according to a fixed plan. So, also, in those cell-masses (or vesicular neurine) appropriate to special ideas, there are psychical substrata, in virtue of which there is a constant construction of new cells, corresponding to those new states of the consciousness comprised under the general term *development of ideas*, the ideas being developed and the new cells constructed according to a fixed and predetermined law of development. The substrata have *potential* properties—that is to say, they contain the germs of further and indefinite series of future changes, as well as properties in actual use in relation to the external world.

ART. II.

Pathological Observations on the Bodies of Known Drunkards.

Part III. By FRANCIS OGSTON, M.D.

THE series of observations recorded in the two previous parts,[*] though less numerous than could have been wished, may afford a sufficiently broad basis for the support of a few useful deductions. The favourable circumstances under which they were made, yield, it is deemed, a fair security for their accuracy; the details having been borrowed from notes of cases examined *medico-legally*, with the checks against mistake which the general evidence, simultaneously obtained by the legal authorities, was calculated to yield.

1. One of the points which will strike the observer, in looking at the previous parts, is *the extent of chronic change in the various organs* of the individuals whose bodies were inspected. That these changes are far in excess of what could have been reasonably looked for in a like number of persons of the same age and of temperate habits, suddenly cut off while apparently in average health and vigour, will scarce admit of dispute. But for their sudden deaths from other than natural causes, all, or the far greater number of them, might have survived for some time. That the period of their survival, however, independently of these causes, was not likely to have been a very distant one, in the case of many of these persons, may be safely assumed, considering the proclivity to natural disease of a serious kind shown by the state of the principal organs of the economy, to a greater or less extent, in all but one of their number. Thus, the sum of the bodies examined in the two series of observations gives, in 117 individuals, *abnormal appearances*—

In the head in 108, or in 92·3 per cent.
chest in 86, or in 73·5 per cent.
abdomen in 95, or in 81·19 per cent.

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[*] Vide British and Foreign Medico-Chirurgical Review, vol. xiii. p. 502; vol. xiv. p. 507.

Again, the same cases yield *simultaneous morbid appearances*—

In the head, chest, and belly in 67, or in 57·26 per cent.
head and chest in 77, or in 65·8 per cent.
chest and belly in 74, or in 63·24 per cent.

Or, taking the condition of the more important organs separately, diseased appearances are observed—

In the encephalon in 108, or in 92·3 per cent.
respiratory organs in 74, or in 63·24 per cent.
liver in 66, or in 56·4 per cent.
central organ of the circulation (including the pericardium, aorta, and pulmonary artery) in 56, or in 47·86 per cent.
kidneys in 51, or in 43·58 per cent.
intestinal tube in 48,* or in 41 per cent.

2. From a comparison of the two series of observations, *the cumulative effects of long-continued intemperance* appear to be forcibly brought out. The parties classed together in the first paper of the series may be taken as a less extreme order of the intemperate than those arranged in the second paper. This assumption is grounded in part on the results of inquiries made into the previous habits of the different individuals set down as drunkards in the two papers, and partly on the circumstance, that the parties in the second series of observations were known to have reached what may be almost considered as the appropriate termination of lengthened courses of intemperance—viz., death from the direct effects of the poison, independently altogether of the intervention of ordinary disease.

This contrast will be best seen by a glance at the subjoined tables:

TABLE I.

First Series.

Abnormal appearances in the cranium in 65, or 89 per cent.
chest in 48, or 65·7 per cent.
abdomen in 54, or 73·9 per cent.

Second Series.

Abnormal appearances in the cranium in 43, or 97·7 per cent.
chest in 38, or 86·3 per cent.
abdomen in 41, or 93·1 per cent.

TABLE II.

First Series.

Simultaneous abnormal appearances in the head, chest, and abdomen in 32, or 43·8 per cent.
head and chest in 40, or 54·7 per cent.
chest and abdomen in 38, or 52 per cent.
heart, liver, and kidneys in 9, or 12·3 per cent.
heart, lungs, liver, and kidneys in 6, or 8·2 per cent.
liver and kidneys in 13, or 17·8 per cent.
head, pericardium, heart, pulmonary arteries, and aorta in 26, or 35·6 per cent.

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* In 14 cases there were *simultaneous affections* of the stomach and intestines.

Second Series.

Simultaneous abnormal appearances in the head, chest, and abdomen in 35, or 79 per cent.
head and chest in 37, or 84 per cent.
chest and abdomen in 36, or 81·8 per cent.
heart, liver, and kidneys in 14, or 31·8 per cent.
heart, lungs, liver, and kidneys in 11, or 25 per cent.
liver and kidneys in 25, or 56·8 per cent.
head and in the pericardium, heart, pulmonary arteries, and aorta in 25, or 56·8 per cent.

The same result is obtained from a comparison of the states of the principal systems and separate organs of the body, in the two series of cases—as under:

TABLE III.

First Series.

Abnormal appearances in the head in 65, or 89 per cent.
respiratory organs in 41, or 56·16 per cent.
organs of circulation in 31, or 41 per cent.
liver in 30, or 41 per cent.
spleen in 14, or 19·1 per cent.
kidneys in 23, or 31·5 per cent.
stomach in 20, or 27·3 per cent.
intestines in 10, or 13·6 per cent.

Second Series.

Abnormal appearances in the head in 43, or 97·7 per cent.
respiratory organs in 33, or 75 per cent.
organs of circulation in 26, or 59 per cent.
liver in 36, or 81·8 per cent.
spleen in 18, or 40·9 per cent.
kidneys in 28, or 63·6 per cent.
stomach in 24, or 54·5 per cent.
intestines in 8, or 18·18 per cent.

3. The results of the various inspections support, on the whole, the conclusions as to the injurious effects of alcohol in excess, arrived at on *theoretical grounds.*

(1) *The nervous centres* present the greatest amount of morbid change. It has been already noticed that the abnormal appearances within the head, as deduced from both series of observations, yield a per-centage of 92·3 in the whole. This bears out the remarks of Dr. Carpenter,* that

> "From the peculiar power which alcohol has been shown to possess of disturbing the functional activity of the nervous system, and perverting its nutrition, it might be expected that disorders of this apparatus would be amongst the most frequent of the maladies induced by it; and further, that such disorders should show themselves rather in the brain than in the spinal cord, or in any other part."

The stimulating action of alcohol on the cerebral system, and its power of exalting and perverting the mental powers, affords a satisfactory explanation of the close coincidence observable between the results of the inspections of the head in the bodies of so many known drunkards, and those deduced from the examination of the same part in those of confirmed lunatics, as stated in works on insanity. Thus we find in the cases examined abnormal changes in the cranium, the membranes, and substance of the brain, and in the cerebral ventricles, both in number and kind, which, were there any *speciality* in mental diseases, would scarcely have been encountered to the same extent in those under consideration.*

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* Physiology of Temperance and Total Abstinence, p. 30.

The relative frequency of the more usual of the morbid appearances encountered within the head, in the whole of the bodies examined, will be perceived by a glance at the subjoined table.

TABLE IV.

Morbid changes in the cranium in 35, or 29·9 per cent.
dura mater in 28, or 23·9 per cent.
arachnoid in 88, or 75·2 per cent.
pia mater in 33, or 28·2 per cent.
cerebrum in 71, or 60·68 per cent.
cerebellum in 23, or 19·6 per cent.

The separate alterations of the cranium and its contents, in the two sets of cases, bring out the same results as we previously obtained in Tables I. and II. Thus we find,

In the First Series.

Morbid changes in the cranium in 18, or 24·6 per cent.
dura mater in 17, or 23·28 per cent.
arachnoid in 50, or 68·49 per cent.
pia mater in 21, or 28·7 per cent.
brain in 37, or 50·68 per cent.
cerebellum in 14, or 19·17 per cent.

In the Second Series.

Morbid changes in the cranium in 17, or 38·6 per cent.
dura mater in 11, or 25 per cent.
arachnoid in 38, or 86·36 per cent.
pia mater in 12, or 27·27 per cent.
brain in 34, or 77·27 per cent.
cerebellum in 9, or 20·45 per cent.

(2) Following the numerical order, the *changes in the respiratory organs* succeed in frequency those in the nervous centres; such alterations reaching in all to 74, or the per-centage of 63·24. The additional labour thrown upon the lungs when alcohol has entered the circulation after its absorption, and the retardation which takes place in these circumstances of their functional activity from this cause, as well as by the toxical

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* One of the morbid (?) changes noticed in some works on insanity as a result of chronic forms of mental disorder, has been omitted altogether in the cases analysed. I allude to the enlargement of the size, and the wider diffusion of the Pacchionian bodies, which were encountered by me simultaneously with the thickening of the arachnoid, with scarcely any exception. Another appearance which has been brought prominently forwards as characteristic of chronic cases of insanity, though not omitted, has been understated in the above analysis. I refer to the unnatural degree of adhesion betwixt the skull and dura mater. This adhesion is noted (in Parts I. & II.) in 16 of the cases. But this number represents only the instances in which such cranial adhesions were strongly marked. To some extent it was encountered in the far greater number of the inspections.

effects of the alcohol on the medulla oblongata, will alone go far to account for the frequency of morbid changes in these organs in drunkards. The relative numbers of such changes in the two sets of cases (56·16 in the one, and 75 per cent. in the other); and the more serious character of the affections of the lungs in the second than those in the first set of cases, favours this conclusion drawn from physiological considerations. There can be but little room for hesitation, however, in attributing this state of matters, in part, at least, to the operation of other causes but indirectly connected with the action of the poison itself—such as exposure to cold, &c. The mutual reaction between the lungs and the heart, liver and kidneys, where these last are affected, must likewise augment the operation of ordinary morbid causes on the respiratory organs, especially in some of the cases belonging to the second series, where the alterations were of a marked kind.

(3) The alterations of the respiratory organs are succeeded, in numerical order, by the *morbid changes in the liver,* a result which might have been anticipated on purely theoretical grounds. Here, perhaps, as in the case of the brain, the production of the changes met with may be attributed mainly, if not entirely, to the direct influence of the alcoholic stimulus, the effects of which on the liver, as on the encephalon and lungs, were best marked in the class of more advanced drunkards.

Most of the individual changes in the liver are such as might, *à priori,* have been expected. Thus, there was found in all,

The liver enlarged in 32, or in 27·35 per cent.
liver granular in 14, or in 11·9 per cent.
nutmeg liver in 13, or in 11·1 per cent.
fatty liver in 24, or in 20·5 per cent.

One other change was more rarely encountered than might have been counted on—*cirrhosis,* having been present only in 5 of the cases, while 1 of these alone belonged to the more confirmed class of drunkards.

(4) After the changes in the liver come those in *the central organ of the circulation, and the large arteries connected with it.* The changes in the heart and arteries to be met with in drunkards, in such numbers, is attributed by Dr. Carpenter partly to the gouty and rheumatic diatheses generated in such persons by alcoholic fluids, and in part to the direct action of the poison introduced into the blood; while he has not overlooked the effects of antecedent structural diseases in other organs.* That these last play an important part, if not in originating, at least in aggravating, the diseased states of the heart and arteries in drunkards, cannot be doubted, if the amount and severity of these diseases are taken into account.

Here too, as before, the principal morbid appearances were the more numerous in the second class of subjects.

(5) Closely following *affections of the heart,* came those of *the kidneys,* a circumstance which the statements of writers on diseases of this organ give grounds for anticipating, and which need not be dwelt on. That alcohol will escape by the kidneys unchanged, is proved by the fact that it can be tested chemically in the urine. This I have verified in several cases of death by drowning. That this fluid, however, could be found in

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* Op. cit., pp. 74, 75.

the urine in any but the minutest quantities, and in a highly diluted state, I did not consider at all likely till the following occurrence showed its possibility:

A male, forty-seven years of age, while intoxicated, threw himself into the Aberdeen harbour, in December 1850. The body was recovered from the water in less than an hour, and inspected fourteen hours after death. At the inspection, from three to four drachms of urine from the bladder was being heated in an iron spoon over the flame of a candle, to ascertain if it contained albumen, when the flame set fire to the vapour rising from the fluid. This unexpected event was witnessed by my colleague, Dr. James Jamieson, and several medical students who were present. In this case, besides marked hypertrophy of the brain and liver, and dilatation of the ascending aorta, both kidneys were found pale and bulky, with their cortical part attenuated, and the urine highly albuminous.

(6) That *the intestinal tube*, as a whole, should present fewer lesions in drunkards than the brain, lungs, liver, heart, and kidneys, and that these should be chiefly encountered in the stomach, accords with the experience of others, and has been explained by the fact of the rapidity of the absorption of the fluid, and the consequent speedy disappearance of it from the organ which it first reaches on its ingestion. The number of cases in which *the stomach* was seen to have suffered, more or less, was not, however, inconsiderable, reaching a per-centage, in all, of 37·6, and in the second set of cases, to one of 54·5.

The highly congested state of the capillaries, indicated by the deep and uniform reddening of the interior of the stomach on its exposure, differs from the congestion occasionally met with in instances of death by asphyxia. In the latter case, the congestion is usually co-extensive in the stomach and smaller intestines, and at once presents itself to notice on the abdominal cavity being laid open. The appearance in question, on the contrary, rarely* extends beyond the stomach itself, is limited to its inner membrane, and is only noticeable after a short exposure of it to the atmosphere.

The darkened petechial spots, seen through the mucous coat of the stomach in fourteen of the above cases—and termed, for want of a better name, false melanosis—are appearances not unfrequent after deaths from both coma and asphyxia; though, in these instances, it is believed that it is in the case of intemperate persons that they are usually to be encountered in the dead body. By a careless observer, the false might be readily confounded with the true melanotic spots, which in shape and colour, when met with in the stomach, they nearly resemble. The two cases of melanosis belonged to the more advanced class of drunkards.†

The unusually small size of the stomach, in sixteen of the cases in all, was an appearance altogether different from any mere state of emptiness, or natural contraction of the organ. It was, in short, such an atrophy of the whole stomach as this viscus might have presented had its growth been arrested in early life. Thus, in one case, the organ is set down, in the notes of inspection, as only about half the ordinary size; in a second, as not larger than that of the infant at birth; in a third, as barely exceeding the diameter of the duodenum over the greater part of its extent; and in the remainder, as unusually or remarkably small as compared with the rest of the intestinal tube.[*]

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* In one of the above cases, the redness extended from the stomach along the inner surface of the duodenum.

† I have since met with an instance of the same deposit, under the lining membrane of the stomach, on opening the body of a person who had died from exposure to cold. This person was also reported to be of very intemperate habits.

From having met with instances of *softening of the mucous coat of the stomach* in his earlier inspections, the writer was disposed to consider this change as *not unusual* in drunkards.[†] More extended observation, however, it will be seen, has not borne out the truth of the deduction as regards its comparative frequency in such subjects.

4. The changes in the organs of drunkards under review may safely be set down, in general, as instances of *abnormal nutrition, favoured, if not wholly induced,* directly or indirectly, *by the poison* which had previously circulated through the system.

Though much caution is undoubtedly requisite in estimating the extent of organic change clearly attributable to this cause, especially if the changes are to be viewed in their earlier manifestations, and traced to their ultimate consequences, still it may be possible, with little risk of mistake, to class together the more obvious of the appearances in the tissues or organs which point, with more or less clearness, to disordered nutrition as their originating cause.

Amongst these purely nutritive lesions I have placed the *softening of the brain* observed, to some extent, in 21 of the cases; though I have done so with some hesitation, as the tests now employed for the discrimination of the inflammatory from the non-inflammatory form of this affection had not been pointed out at the time when many of these observations came under notice.

Assuming that the excess of the cerebro-spinal fluid met with in 76 of the inspections had existed for some time before death, and that its presence was a consequence of a *diminution in the bulk of the cerebral mass,* we have another indication of defective nutrition of the brain.

If, with Dr. Gairdner, it is considerd that *emphysema* "never occurs unaccompanied by pulmonary collapse, or by one or other form of pulmonary atrophy,"[‡] defective nutrition will afford an explanation of the recurrence, in 38 of the cases, of this morbid appearance.

Decided instances of defective nutrition present themselves in the *attenuation of the walls of the heart,* in 23 cases; *the atrophied stomach* in 16; *the atrophied or contracted intestine* in 11; *the atrophied spleen* in 1; *the cirrhosed liver* in 5; *the atrophied kidneys* in 10; and perhaps, also, in *the softened lining of the stomach and intestine* in 8 cases, in all.

The bodies examined afforded other examples of disordered nutrition in important organs: thus, *the fatty change in the liver* was met with in one form (the fatty liver) in 24; and in another form (the nutmeg liver) in 13.

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[*] In five of these cases there was, at the same time, the central or hour-glass contraction of the stomach present; and in two of them the smaller intestines were also contracted unnaturally.

[†] See Edinburgh Medical and Surgical Journal, vol. xl. p. 292.

[‡] Manual of Pathological Anatomy, by Jones and Sieveking, p. 411; British and Foreign Medico-Chirurgical Review, April, 1853.

The fatty degeneration was observed *in the kidneys* in 12, if not in 16 of the cases, and in *the heart and arteries* in 5.

If it be allowable to consider the marked *absence of the fatty tissue* on the one hand, or its *deposition in mass* on the other, as a fair criterion of deficient or perverted nutrition generally, this tissue will be found wanting over the trunk and limbs in 40, and superabundant in the same situation in 33, of the cases. *Internally*, again, there was noted a large excess of fat deposited in *the mediastinum*, *heart*, and *omentum*, in numbers respectively of 4, 20, and 8 of the cases.

The voluntary muscles were *attenuated*, to any notable extent, in but 5 of the subjects examined, while they were *markedly developed* in as many as 22 of these.

Besides these instances in the fatty and muscular tissues, examples of both defective nutrition on the one hand, and augmented nutrition on the other, were encountered in *the bones of the skull*, in numbers respectively of 9 and 23.

Illustrations of augmented nutrition in *the viscera* of the body occurred in some numbers in our inspections. Thus we had, in all—

Hypertrophy of the brain in 4 cases.
heart in 33 cases.
stomach in 3 cases.
liver in 32 cases.
spleen in 9 cases.
kidneys in 27 cases.

It will be seen that the disturbances of the nutritive functions in various parts of the body almost invariably preponderate in the more advanced class of drunkards. The relative frequency of the more common of these is contrasted in the table below (Table V.):

Table V.

First Series.

Brain softened in 5·48 per cent.
atrophied* in 54·7 per cent.
hypertrophied in 2·7 per cent.
Lungs emphysematous in 23·2 per cent.
Heart attenuated in 12·3 per cent.
hypertrophied in 21·9 per cent.
loaded with fat in 15 per cent.
Stomach atrophied in 12·3 per cent.
softened in 2·7 per cent.
hypertrophied in 1·36 per cent.
Liver cirrhosed in 5·47 per cent.
nutmeg in 5·47 per cent.
fatty in 5·47 per cent.
hypertrophied in 15·06 per cent.
Spleen softened in 1·36 per cent.
hypertrophied in 2·7 per cent.
Kidneys fatty in 1·36 per cent.
atrophied in 6·8 per cent.
hypertrophied in 17·8 per cent.

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* As indicated by the subarachnoidal serum.

Second Series.

Brain softened in 38·6 per cent.
  atrophied in 81·8 per cent.
  hypertrophied in 4·5 per cent.
Lungs emphysematous in 47·7 per cent.
Heart attenuated in 31·8 per cent.
  hypertrophied in 38·6 per cent.
  loaded with fat in 20·4 per cent.
Stomach atrophied in 15·9 per cent.
  softened in 4·54 per cent.
  hypertrophied in 4·54 per cent.
Liver cirrhosed in 2·27 per cent.
  nutmeg in 20·4 per cent.
  fatty in 45·4 per cent.
  hypertrophied in 47·7 per cent.
Spleen softened in 15·9 per cent.
  hypertrophied in 15·9 per cent.
Kidneys fatty in 25 per cent.
  atrophied in 20·4 per cent.
  hypertrophied in 31·8 per cent.

5. The *comparative absence of inflammatory appearances* forms a marked feature in the inspections.

The most unequivocal traces of previous inflammatory attacks are those presented by *the pleuræ* in 49, *the lungs* in 15, and *the pericardium* in 8 of the cases.

How far *the opacity and thickening of the arachnoid* in 65, and *the injected state of the pia mater* in 32 of the cases, are to be regarded as evidence of previous or existing inflammation in these tissues, is at least doubtful, considering the almost entire absence about the brain and its membranes of such other appearances as are, by all pathologists, admitted to be the unquestionable attendants or sequelæ of healthy inflammatory action.

Of an equally, if not more equivocal kind, are *the indurations of the brain* in 39 of the dissections, and for the same reason.

It need scarcely be remarked, that appearances encountered *in the valves of the heart* are, by some of our best pathologists, considered to be the effects of slow nutritive changes of the tissues, unconnected with inflammatory action.

6. So far as they go, the results of the observations, in the cases adduced, bear out the correctness of the commonly-received opinion as to the comparative immunity of drunkards from tubercular affections.

7. In concluding the present contribution towards the elucidation of one of the aspects of a subject of such breadth and importance as the one in question, it may not be superfluous in the writer to state distinctly that, in this and the previous articles, he has had in view the embodiment merely of such facts as he has had under his own notice, in a now somewhat lengthened period of professional acquaintanceship with an unfortunate class of his fellow-creatures. The facts adduced, such as they are, were collected and arranged without any mental prepossessions on his part, and the attempt either to underrate or to exaggerate the effects of alcoholic intemperance it has been his endeavour equally to avoid.

ART. III.

Supposed Case of Ergotism. By THOMAS CAMPS, M.R.C.S., Fenny Stratford.

ERGOTISM, though largely treated of by continental writers, is a disease so little known in this country, that a brief account of the ravages committed by it, together with the attendant symptoms, may, to some readers, form a desirable introduction to the case now submitted to their notice.

Tissot, in a Latin letter addressed, in 1762, to the Royal Society in England,* has given a brief but masterly sketch of the different epidemics believed to have arisen from the use of ergoted grain. The communication exhibits much careful research, and appears to have formed the basis of most historical accounts of this disease, as given by subsequent writers. The following remarks are drawn chiefly from Tissot, and the 'Dictionnaire des Sciences Médicales,'† with some additions from more recent sources.

Ergot is a disease most commonly affecting rye, though by no means confined to this grain. It is frequent on wet soils, and in hot summers when there has been abundance of rain, and is also more prevalent in the spring-sown crops. When only one or two ergots are found in a single ear, they are of considerable size;‡ if more numerous, they are proportionally smaller. As regards its mode of production, Mr. Quekett considers that ergot is merely the body of the grain changed from its healthy condition by the presence of a parasitic fungus. Mr. Bauer, on the other hand, believes that the fungus has no connexion with the ergot, either as cause or effect, and that the origin of the disease is still unknown.§

The train of symptoms produced by the use of this diseased grain was first noticed by Wendelin Thalius in 1596, and from that time the malady appears at intervals to have prevailed extensively in various parts of Europe,|| causing either spasmodic affections, or gangrene of the extremities. The spasmodic form of the disease, as it appeared in some of the districts of Bohemia, in 1736, was witnessed by J. A. Srinc, who alone saw five hundred cases of it. He describes it as commencing with a sense of tingling or itching in the feet; severe cardialgia then came on, and the disease ascended to the hands and head. The tingling sensation, sometimes compared to the bites of ants, was followed by violent contractions of the hands and feet, affecting each particular joint, and described as resembling the pain of dislocation. The patients complained that their hands and feet were burnt, the body being bathed in copious sweats. After these pains—which were intermittent, having sometimes intervals of two or three days—the sufferers were affected with drowsiness,

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* Philosophical Transactions, vol. xlii. part 2.

† Art., Ergot and Ergotism.

‡ Twenty-six lines is the greatest length which the ergot of rye is mentioned as having attained to. I have, however, in my possession, a specimen, gathered in this immediate neighbourhood, which measures upwards of thirty lines. Twelve is the greatest number of ergots described as occurring in a single ear, but in three ears examined by me, the numbers were respectively twenty-four, twenty-two, and twenty-one.

§ Transactions of the Linnean Society, vol. xviii. part 3; see also Lancet, No. 14, 1851.

|| Even so recently as 1854, thirteen cases of this disease are mentioned as having been admitted into the hospital at Lyons. Medical Times and Gazette, Dec. 16th, 1854.

giddiness, indistinctness of vision, and staggered in walking. Some became maniacal, some melancholy, and others comatose. Those who had reached their fifteenth year were very liable to epilepsy, and of these the greater part died. An enormous appetite generally accompanied this train of evils. Spots appeared on the feet of one, resembling the bites of fleas, which remained to the end of the eighth week. The faces of many were extensively covered with these spots. In those who recovered, the disease rarely abated before the third week, whilst in many it continued for one or two months.

The other variety of ergotism—that accompanied by gangrene of the extremities—is thus depicted by Langius, as it was observed by him in some of the cantons of Switzerland in 1715 and 1716. After excessive lassitude, more or less protracted, and unaccompanied with fever, the extremities become painful, cold, and rigid. Benumbed, and almost insensible, the limbs were yet capable of movement, though with difficulty. The patients were afflicted with grievous internal pain, which was greatly increased by heat, whether of the bed or atmosphere, but abated somewhat when they were exposed to a cooler temperature, though even then it was scarcely tolerable. The pain extended by degrees from the toes to the legs and thighs, and from the fingers to the arms and shoulders, till, sphacelus supervening, the affected parts, dead and black, dropped from the trunk or the adjacent members.

Throughout the progress of the disease, the general health appeared to be but little affected, with the exception of slight febrile heat from an increase of pain, and copious sweats, extending from the head to the pit of the stomach. Those with whose food ergot had been sparingly mixed, were only affected with a sense of weight in the head, and drowsiness, sometimes followed by transient intoxication.

In 1747, a fatal epidemic of this kind raged in Sologne, destroying the greater number of the inhabitants. Salerne, a physician practising at Orleans in 1748, saw, in the Hôtel Dieu of that city, a child ten years of age, which had lost the whole of both the lower extremities, the thighs being detached from their articulations without hæmorrhage. Her brother lost the leg and thigh of one side, and the leg of the other. Both died within a month from the commencement of the disease. De Vetillart, who in 1770 published a work on the treatment of gangrenous ergotism, gives the following example of its virulence:

> "A poor man, of Noyen, in Maine, seeing a farmer sifting his rye, begged the refuse to make bread with. The farmer cautioned him as to the danger of using it, but hunger prevailed over fear. The siftings, composed chiefly of ergot, were ground and made into bread. In the space of a month, the poor man, his wife, and two of his children, perished miserably. A third, an infant at the breast, who had eaten panada made with this flour, escaped death, but was deprived of both legs, and became deaf and dumb."

Instances similar to the above are frequently to be met with in the continental writers. In our own country, a disease, believed by Tissot and other eminent physicians to be precisely similar to gangrenous ergotism, occurred in a family at Wattisham, in Suffolk.* This case was described in the 'Philosophical Transactions' of 1762, by Dr. Wollaston and the Rev. J. Bones. A narrative of the occurrence was also published in a separate form by Dr. Wollaston, and in the parish register of Wattisham is a detailed account of the striking circumstances that attended it.

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* See also an interesting case in the Philosophical Transactions for 1757, abridged in the Gentleman's Magazine for October, 1758.

Dr. Wollaston states, in his communication, that the husband, wife, and six children were affected with this disease; the eldest being a girl of fifteen, and the youngest an infant of four months old. They were all healthy at the time of the attack. It commenced with severe pain, which in most of the cases attacked the left leg first, or as some of them described it, the leg and foot. The pain was so violent, that the whole neighbourhood was alarmed by the cries of the sufferers. In the course of a few hours the toes became much swelled, and after four, five, or six days, the pains abated, and the foot began to turn black, appearing at first covered with spots, as though it had been bruised. At this period, in those affected in more than one limb, the other began to be attacked with similar pain. The swelling and discoloration gradually extended upwards, till, finally, the mortified parts separated from the bone, and, after a lengthened period, the bone itself was detached from its connexions. All, with a single exception, were affected about the same time, in the month of January, the weather being then warmer than usual. In the case of the father, the disease assumed a milder form, the fingers only becoming discolored and contracted, and the nails of several of them falling off. For a long time afterwards, he continued to complain of darting pains in the legs, hands, arms, and back. One poor girl, attacked in the left foot, stood upon the other for three weeks, leaning against the chimney, till that foot also becoming affected, she took to her bed. During the whole time of this calamity, the family appeared in other respects well. They ate heartily, and slept soundly when the pains abated.

At the termination of the disease, the father had recovered, with the exception of two fingers, which remained in some degree contracted. The mother, aged forty, had lost the right foot at the ankle joint, and the left leg a little below the knee; her hands, and part of her arms, remained with but little sensation, the fingers being also contracted. Mary, aged fifteen, had lost both legs below the knee, and was then dead. Elizabeth, aged thirteen, had also lost both legs below the knee. Sarah, aged ten, was deprived of one foot, and two of the toes of the remaining foot. Robert, aged eight, had both legs off below the knee; and Edward, aged four, had lost both feet at the ankle joints. The infant was weaned as soon as the mother was attacked, but became ill, and died in the course of a few weeks. It appeared to suffer violent pain, and the legs became black before death. Dr. Wollaston states that, with the exception of the mother, "the rest of the family seemed well. One poor boy, in particular, looked as healthy and florid as possible, and was sitting on the bed, quite jolly, and drumming with his stumps."

No cause could be assigned for this dire visitation, except that of the family having lived for about a fortnight on bread made from wheat which had been badly harvested. Mr. Bones says, in reference to it,

> "This wheat they have bought of the farmer whom I lodge with, who tells me, that last year he had some wheat *laid*, which he gathered and threshed separately, lest it should spoil his samples. Not that it was mildewed, or grown, but only discolored, and smaller than the other. This damaged wheat he threshed last

Christmas, and then this poor family used no bread but what was made of it, as likewise did the farmer's own family, and some others in the neighbourhood. A labouring man of the parish, who had used this bread, was affected with a numbness in both his hands for about four weeks from the 9th of January. His hands were continually cold, and his finger-ends peeled. One thumb, he says, still remains without any sensation."

Dr. Wollaston also says that the corn alluded to was very bad. "It was wheat that had been cut in a rainy season, and had lain on the ground till many of the grains were black, and totally decayed."

I have entered more into detail on a few points of the above narrative, as they appear to have a direct bearing upon the facts about to be submitted to the notice of the reader. In apologising for the very imperfect manner in which the following case is drawn up, I must observe, that the patient lived at some distance from me, and that when he first came under my care a malignant epidemic of scarlatina, not extending however to the village in which he resided, so occupied my time and thoughts as to prevent me from giving that attention to him which I should otherwise have done. A reference to the subjoined notes will show the exact resemblance subsisting between ergotism as described by continental writers, and the form of disease assumed in the present instance.

James Golding, of Newton Longville, Bucks, labourer, twenty-five years of age, about five feet nine inches in height, well formed, with dark brown hair, grey eyes, and, when in health, of florid complexion, had been working in the neighbourhood of London since Michaelmas, 1853, and was employed in digging drains for some houses about to be erected. A little after Christmas he was attacked with pneumonia, which his medical attendant informs me was severe in character, affecting both lungs, and requiring general and topical bleeding, tartar emetic, and mercury, to subdue it. Whilst improving under this treatment he imprudently exposed himself to cold. The urine now became albuminous, and the patient's condition in all respects more unfavourable. Shortly after, he was removed by his friends to his native village, and put under my care on the 26th of April, 1854.

I found him reduced to a state of great debility, much emaciated, and complaining chiefly of difficult breathing and a sense of exhaustion. The left lung appeared to be extensively hepatized posteriorly, much more so than the right, which also showed well-marked signs of disease. I was unable to detect any morbid sounds in the heart, nor were any observed by such of my medical friends as examined the case. The urine was albuminous, but by no means to a great extent, and the mouth suffering, though not severely, from the effects of the mercury. The pulse, as might be expected, was feeble. No febrile action was present, and at that time there was nothing to lead me to anticipate the remarkable symptoms about to be developed. As no indication for active medication existed, it appeared the safest plan to watch the patient carefully, but in his present state to pursue only the expectant plan of treatment.

In the course of ten days or a fortnight after his return home, he began to complain greatly of pain and numbness in the left leg and foot.* A small patch of eruption showed itself on the calf of the leg (much resembling lichen in appearance, except that it was darker in colour), very slightly raised above the skin, and not mingled with any vesicles.[*] There was no heat of surface, though the patient complained of a sense of burning, accompanied with formication; and there was nothing in the appearance of the limb to account for the constant and intense pain, which continued day and night, without being relieved by any form of opiate, though given in large doses. In fact, so completely did opiates fail to mitigate the pain, that they were abandoned as useless. After a few days, the foot and lower part of the leg became cold, nearly void of feeling, and were evidently in a state of approaching gangrene, which soon showed itself unequivocally. The parts became black, and so shrivelled as to give the idea of nothing intervening between the skin and the bones beneath it. At this time, and throughout the greater part of his illness, his extreme aversion to warmth was very remarkable. He complained that heat applied to any part of the body aggravated his pains; and if, on a cold day, any additional covering was laid upon him whilst sleeping, he wakened almost instantly and threw it off.[†] I generally found him, in the coldest weather, lying in bed with only an old cloak thrown over him.

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* Primum symptoma erat torpor crurum, tum dolor cum levi tumore sine inflammatione, et cità successione, frigus, livor, sphacelus, membri excisio. Tissot's letter: Philosophical Transactions, vol. xlii. part 2. 1762.

When sphacelus had taken place, the affected limb became easier; but about this time the right leg and foot were affected in a precisely similar manner, and, in succession, both hands, the ala of the right nostril, and a small portion of the upper part of the helix of each ear. In the earlier part of these attacks he suffered greatly from spasmodic contraction of the hands and feet, which was not constantly present, though generally returning at short intervals.[‡] The pulse in frequency differed but little from the standard of health.[§] There was no febrile action,[‖] except at the time when vesications appeared where the line of demarcation was about to be formed. Though not complaining of the head, the mental powers were evidently much enfeebled. The memory was greatly impaired, and his attendants described him as talking all kinds of nonsense.[¶] As soon as the pains abated, he began to perspire profusely, slept well,[**] and had a voracious appetite.[††] The bowels continued regular, with the exception of about a fortnight, when he had severe diarrhœa. When signs of approaching gangrene became evident, wine was given, and bark in its different forms, as long as the patient was willing to take it.

The specific gravity of the urine, which for a considerable time was examined weekly, was 1·011 when he first came under my care, but after a short time it was reduced to 1·006, from which it afterwards varied but little. The colour was generally pale, and it was either neutral or faintly acid, depositing little or no sediment. The albumen continued to diminish, till eventually it almost disappeared. From its low specific gravity no sugar could have been anticipated, nor did it at any time show traces of it. At an *advanced* period of the disease the presence of urea was clearly ascertained, though I am unable to say whether this element was normal in amount.

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[*] Maculæ in alterius pedibus effloruere, pulicum morsibus similes, quæ in octavam hebdomadam perdurant. Quorundam facies turpiter fœdata est maculis. J. A. Srinc, quoted by Tissot, as above. See, also, a case quoted in the Medical Times, March 4th, 1854.

[†] "Dolore interno cruciabantur atrocissimo, qui ex atmosphæræ aut lecti calore enormiter augebatur." Langius, as quoted by Tissot.

[‡] "Cette maladie," dit M. Bordot, "commence le plus ordinairement par une sensation incommode aux pieds avec une sorte de titillation, ou de fourmillement dans ces parties. Ces symptomes sont bientôt suivis de contractions violentes spasmodiques des membres," &c.

[§] "Pulsus sanorum similis, nullo excepto." J. A. Srinc, as quoted by Tissot.

[‖] Langius describes the disease as commencing, "absque ullâ febre."

[¶] "Obliviscuntur se . . . . mentis minime compotes" J. A. Srinc.

[**] "Sub morbi decursu reliquæ corporis partes satis bene valebant." Langius.

[††] "Universa hæc malorum ilias pedisequam habet bulimum." J. A. Srinc.

The eruption, which constitutes an interesting feature of the disease, continued to make its appearance at intervals throughout the attack, till within a month of the present date, Nov. 12th, 1854. It was most abundant on the knees, shoulders, elbows, and the skin covering the lines of the tibia and ulna. The face had many spots on it: they were observed on the nose, the upper part of each ear, and even on the glans penis. Indeed, no part of the body appeared to be wholly exempt from them. The eruption was accompanied with intense itching. Its duration was uncertain; sometimes disappearing in a few days, and at others continuing for many weeks. The spots generally appeared in small patches, varying in form. They differed from petechiæ in colour, being of a redder tint, and were slightly elevated above the level of the skin. On dying away, they left in some places merely a dark stain; in others, desquamation of the cuticle took place, or incrustations of a yellow colour were formed, adhering for a lengthened period; whilst occasionally, as on the nose and ears, slight sloughing of the parts affected took place.

No fœtor was perceptible from the gangrenous portions of the limbs till their separation commenced. It was then horribly offensive. On this account, after the soft parts of the *left* leg had separated to such an extent as to leave a large portion of the bone exposed, it was thought best to remove the limb at this point. This was done by sawing through the bones close to the granulating surface, which was effected with difficulty, from their extreme density. The walls, where divided by the saw, presented a polished surface resembling ivory. It was found needful to apply lint to the bone, which bled freely. I was not allowed to retain the foot after its removal, but on separating a portion of the posterior tibial artery it was found, contrary to expectation, pervious. The bones of the *right* leg soon after became denuded a little above the ankle, and the foot appeared about to separate at its articulation with the leg, the ligaments being completely exposed. Amputation, in the ordinary method, has always proved fatal in the gangrene of ergotism,* and in the present instance I have no doubt that it would have been so. The poor fellow laid constantly upon his back, with the knees drawn up, and after the *left* leg had been removed he found much difficulty in placing the limb in a different position, nor did the sore go on so well as it had previously done. On this account, the right foot was detached by cutting through the ligaments, leaving about an inch of denuded bone above it. The advantage of this plan of treatment was very obvious. The limb remained in its customary position, and the sore healed rapidly to within a short distance of the projecting bone. With the stump in this state, the removal of the remainder of the bone at a future period will be attended with little risk, should it be deemed expedient. It is worthy of remark, that though the patient has kept his bed for so many months, and during the greater part of the time has lain upon his back, yet no bed-sores have occurred, with the exception of a very small one on the sacrum, which might have taken place in a healthy man confined for as long a time to the same position.

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* "Quatuor mortui sunt post amputationem, gangrænâ ad truncum usque serpente." Noel, surgeon to the Hôtel Dieu, Orleans. "Amputatio mortem citat." Cl. Salerne, quoted by Tissot.

The condition of this unfortunate being at the present time (November 12th, 1854), is as follows:—His general health appears to be good, and he has evidently gained flesh, eats and sleeps well, and is generally free from pain. The pulse is natural, and no morbid sounds can be detected in the heart. His strength appears to him to be restored, but his mutilated condition forbids him to test the truth of his opinion in this respect. There is no tendency to sweating, and the specific gravity of the urine is 1·017. It has an acid reaction, and shows no traces of albumen. The memory, both as regards recent and more remote events, is still feeble, which has added much to the difficulty of drawing up a connected statement of his case.*

The small sores on the nose and ears have perfectly healed. The thumb and forefinger of the left hand have separated at the middle of the first phalanges, and the stumps are cicatrized. It is remarkable that, in the right hand also, the thumb has separated, and the forefinger is about to separate, at the same points. The remaining fingers have all been detached at the joint between the first and second phalanges, leaving the former completely denuded for half their length. These subsequently became covered with granulations. Owing to the carelessness of the attendants, the fingers have been allowed to unite at these points. In the left leg, the bones of which were sawn through about seven inches below the tubercle of the tibia, the stump is still far from healed. The sore on the right leg has granulated as low down as the commencement of the epiphysis of the tibia. At this point, separation will probably take place. The flexors of the leg have contracted to such an extent as to render it impossible to straighten them.

Such are the outlines of this remarkable case. Two questions are naturally suggested by it. Were the above symptoms those of ergotism? and if so, How was the poison introduced into the system? I use the term "ergotism" as designating merely a particular train of symptoms, without reference to the cause producing them, as the case of the Wattisham family renders it probable that there are other diseased states of grain capable of producing all the symptoms caused by ergoted rye. It would be difficult in Golding's case to point out a single symptom that is wanting to complete its resemblance to the pictures of gangrenous ergotism as drawn by those most familiar with its features. It is remarkable, however, in him, that some symptoms of the CONVULSIVE form of this malady—viz., the spasmodic twitching of the limbs, the profuse sweating, and voracious appetite—should also have been present. His case, though bearing some resemblance to those of mortification arising from obstruction in the larger arterial trunks, will be found, on careful comparison, to differ essentially from them. Assuming, then, what appears most probable, that it was a case of ergotism, it remains to consider from what cause it could have originated.

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* "Omnes ægri ab initio fere imbecilli, historiam morbi tradere nesciunt." Salerne, quoted by Tissot.

Rye, as before observed, is most liable to ergot in a wet season, or when sown in the spring. Wheat is also liable to the same disease, and its production is influenced by similar causes. It is at all times less abundant in this grain, and from its appearance being less striking than in rye, it is often overlooked, or not recognised when seen.* I learn from two millers living in low and damp districts, that they have always observed it more or less in the wheat grown in wet seasons, and one of them stated that he had never had a load at his mill of the last year's crop (1853) without finding ergot amongst it. The crops of 1853 were exposed to an unusual amount of rain, the ground having been saturated with it for a longer period than could be previously remembered.† A larger proportion of wheat was spring sown; ergot was more frequent in it than usual, and in some situations much of it was LAID for a long time before it was reaped. It will be remembered that it was LAID wheat which proved so injurious to the family in Suffolk; it had lain on the ground without germinating, its shrivelled appearance and small size showing that it had never ripened, whilst its discolored condition was probably indicative of the change that had converted it into a deadly poison.

There is no reason to suppose that rye was used in the bread eaten by Golding, either before or after his return home, nor did he at any time suffer from deficiency of food, but the condition of the grain consumed by him at some period of his illness was probably the chief agent in the production of the disease from which he suffered. Should the small amount of the noxious principle thus introduced into the system be objected to as inadequate to produce such serious effects, the following considerations may, perhaps, tend to modify this opinion.

In the absence of a surer guide, it is allowable to avail ourselves of the glimmering light which analogy supplies, and in this point of view the action of iodine and arsenic upon the human system are most instructive. In endeavouring to secure their specific effect upon the absorbents and the skin, we know that, within certain limits, the smaller the dose the more readily is our object attained, and that when given to such an extent as to irritate the mucous surfaces, their absorption is impeded. It is also known that these substances may be retained for a considerable time in the system, and that they are occasionally detected in the urine several weeks after the use of them has been discontinued. The poisonous principle of ergot has been found by Mr. Wright in the blood of animals to which it had been administered,‡ and a recent case mentioned in the 'Gazette des Hopitaux'§ proves that it is capable of producing gangrene in the human species months after the use of the bread that contained it has been abandoned.

Whatever may be the effect of ergot given in large doses or in a concentrated form, the more we examine the phenomena connected with its action, the more probable does it appear that, in GANGRENOUS ergotism, the poison enters the blood, and produces its specific action upon the capillary vessels.[*]

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* The *bearded* varieties of wheat appear to be most liable to ergot.

† Dr. Tytler's remarks clearly prove the noxious properties of rice grown in a very wet season. Morbus Oryzeus, by Dr. R. Tytler. Calcutta, 1820.

‡ Edinburgh Medical and Surgical Journal. 1839–40.

§ Quoted in Medical Times. March 4th, 1854.

Looking at the important function performed by the kidneys in purifying the blood, and in not a few instances separating from it any extraneous matter that may have entered the circulation, are we not justified in supposing that the same office is also discharged by them in cases of ergotism? But if from disease the kidneys cease to exercise their depurative function, or perform it to a very limited extent, must not disease ensue from this cause, unless counteracted by an increase in some vicarious secretion?

In the case under consideration, the state of the kidneys renders it more than probable that their office was imperfectly performed. The pulmonary and cutaneous systems were at the same time equally defective in their respective operations, and the vital powers too much enfeebled to struggle against any morbid influences.[†] We are ignorant of the amount of ergot required to produce gangrene in the human species, but it is certain that some individuals are more liable to be affected by it than others,[‡] as we observe with respect to the action of mercury and other remedies. After the most careful inquiry, I am unable to discover anything in addition to what has been already stated, that can throw any light on the origin of this disease in my unfortunate patient. As respects the disease itself, it would seem that we must either regard it as a case of ergotism, or admit that a train of symptoms precisely resembling this malady may arise from causes of which at present we are wholly ignorant.[§]

In conclusion, I would beg to call the attention of my medical brethren to the quality of the flour in use during the prevalence of unusual forms of disease, or whenever symptoms having any resemblance to the mildest forms of ergotism occur in individual cases. This vigilance is peculiarly requisite amongst those of the poorer classes who,[||] in seasons of scarcity, purchase flour at a low price, or who are supplied with bread furnished by contract to parishes, &c. Rye is not likely to be used, except in

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[*] In gangrenous ergotism we have no evidence that the poison acts exclusively on the cerebro-spinal system. Even when sphacelus has made some progress, the power of motion continues in the affected limb, nor is sensation entirely wanting. Courhaut remarks, "Mais les troncs nerveux conservent pendant quelque temps leur sensibilité, leur couleur naturelle, leur forme, même au milieu des parties gangrénées qui les environnent. Cela est d'autant plus évident qu'on observe, dans une partie très-proche de celles qui ne sont pas encore privées de la vie, que les nerfs sont sensibles à environ un pouce de distance de la partie saine."

[†] The different effect of opium on the horse in health and disease, forms a striking illustration of this remark. Mr. Clark states, that two ounces of opium given to a healthy horse produced scarcely any effect; but adds, "I must here again emphatically remark, that when the animal is in a state of debility, as after inflammatory attacks, severe purging, bleeding, or other evacuations, he cannot withstand its action, but the whole system, especially the alimentary canal, rushes into a most violent and fatal inflammation." (A Reformed Pharmacopœia for Horses, by Bracy Clark, F.L.S.)

[‡] Aliæ observationes demonstrant nonnullas personas facillime gangræna corripi." Tissot's Letter.

"Si on observe l'action de l'ergot d'après les tempéramens et la constitution des individus, on remarque qu'il agit plus promptment sur les individus cacochymes, sur ceux qui ont des obstructions dans les viscères, sur les scrofuleux, sur ceux qui ont des ulcères, et sur les personnes affectées de scorbut. Les individus robustes et vigoureux résistent pendant longtemps à son action."—Traité de l'Ergot du Seigle, par J. F. Courhaut. 1827.

[§] A very remarkable case of gangrene of the extremities, occurring in a child three years of age, the cause of which was wholly unknown, is recorded by Mr. Solly, in the Medico-Chirurgical Transactions for 1839–40.

[||] The poor appear at all times to have been the chief sufferers from ergotism, owing, in part, to the inferior quality, and larger amount of the grain consumed by them. To this, neighbourhoods where it is grown, and not to a great extent even there,[*] unless bread is high in price. But in unfavourable seasons, and when the wheat in the poor man's allotment-ground is used soon after it is harvested, danger may sometimes arise from this source.

A poison capable of producing convulsions, gangrene of the extremities, and death in healthy individuals, must surely have symptoms arising from its use in diminished doses, the careful investigation of which is alike due to science and philanthropy.

Postscript, May 24th, 1855:—James Golding is now in perfect health, fat and florid. The sores on the extremities have healed, with the exception of the right leg, from which the lower end of the tibia is not yet detached, though its separation is proceeding.

ART. IV.

A Case of Osteoid Cancer. By WILLIAM SEDGWICK.

THE case which forms the subject of the present communication belongs to a small and imperfectly-recognised class of disease, which appears to have escaped the notice of pathologists until Müller, in the year 1843, published a memoir on the subject, which contains nearly all that is at present known of the history and pathology of these rare and curious formations. The disease is described by him under the name of the "Osteoid Tumour," or "Ossifying Fungous Growth," and he seems disposed to consider it as cancer. Lebert states, that although the disease is constitutional, it is not to be classed with cancer; and accordingly, in his arrangement of tumours into the benign and cancerous, he places "osteoids" in the former division. This opinion, however, of the distinguished French pathologist can hardly be allowed to have weight, as the disease seems to be unknown in France. In this country it is recognised as malignant, and as such is described by Stanley, in his work 'On Tumours in Bones,' under the name of the "Malignant Osseous Tumour;" and by Paget, in his recently published 'Lectures on Surgical Pathology,' as "Osteoid Cancer."

A pedler, aged 19 years; native of London; below the average height; pale, and much emaciated. Both parents appear to have been healthy, and no relation has suffered from cancer. In January, 1850 (at the age of sixteen), he struck his left knee against a flag-stone, and felt pain and stiffness in the hamstrings in consequence; he did not attach much importance to this at the time, thinking the pains were "growing pains." Soon after the accident, he noticed that the knee was swollen slightly on the inner side; after a week, the swelling had extended to the outer side

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should, perhaps, be added, the want of what sometimes appears to act as an antidote—viz., a sufficient supply of other food. Indeed, the extent to which the action of a poison may be modified by the latter circumstance is very remarkable. The yew, as is well known, is a rapid and fatal poison to the horses and cattle that browse upon it. Eight ounces of the leaves given alone proved fatal to a horse within an hour. "But eight ounces of the yew plant, with twice as much of oats, did not kill, or produce any sensible inconvenience; and the same result took place in three or four experiments of Professor Viborg." Reformed Pharmacopœia for Horses. See also Romberg, Nervous Diseases, Syd. Soc. Ed., vol. ii. p. 45.

[*] I am informed by an intelligent corn-merchant, that the imported rye is generally more or less ergoted. As an article of food it is by no means popular in this country. In 1847, some thousands of tons, in the shape of meal, were introduced from the Continent, but the speculation proved a complete failure, and it was sold eventually as food for pigs.

also, and the pain in moving the joint obliged him to give up his occupation, and have medical advice. The knee was leeched with some apparent benefit, but the swelling, after the first five or six weeks from the time of the accident, rapidly increased, and his health became so impaired as seriously to alarm his friends, who removed him, on March 12th, 1850, to the University College Hospital. The tumour at this time presented the appearance of an elongated, rounded enlargement, extending from the upper extremity of the tibia to the commencement of the lower third of the femur; it had a firm and somewhat elastic feel, and the outline of the patella was scarcely perceptible. The knee was kept semi-flexed, and intense shooting pains were sometimes felt in it.

Comparative Measurement of the Two Limbs.

	Sound limb.		Affected limb.
Over patella	11⅝ in.	...	14⅝ in.
4½ inches above patella	11 in.	...	15⅛ in.

On March 13th, Mr. Quain amputated at the commencement of the middle third of the thigh: several arteries had to be tied, and the venous hæmorrhage was considerable. He recovered favourably, and left the hospital in June.

The following is the record of an examination made immediately after the operation:

> "The tumour is an osteoid, affecting the lower end of the left femur; it is of a greyish-red colour, full of large vessels, and permeated throughout, more or less, by bony matter; the cartilages of the patella and femur are sound. Under the microscope, the tumour presents a trabecular-looking structure, consisting of bony meshes, and cells apparently of three different forms: 1st, numerous cells, with sub-cells; 2nd, some flattened cells, with polygonal nuclei; 3rd, spiral-shaped cells, some with fine extremities bifurcated."

After travelling about the country as a pedler for nearly three years, he returned in good health to London in March, 1853, and obtained occasional employment at turning a mangle; but this occupation he was soon obliged to give up, owing to his chest becoming affected; and he now noticed, for the first time, that when he coughed the expectoration was slightly streaked with blood. Having been induced to consult me, the following examination was made:

June 6th.—*General Condition.*—Anæmic; emaciation considerable; skin cool and moist; tongue clean, paler than natural; perspiration at night rather profuse. There is troublesome cough, with occasional slight expectoration of a whitish-looking mucus. Bowels habitually costive. Urine of a reddish colour, diminished in quantity, acid reaction to litmus: odour very urinous: on standing, a copious cloud of mucus collects at the bottom of the vessel; microscopic examination: numerous and very beautiful acicular crystals of uric acid, in radiated clusters; numerous mucous corpuscles; some epithelium scales; vibriones.

Physical Examination of Chest.—Dulness on percussion over the greater part of the left side, slight resonance towards the apex only; behind, the dulness extends for somewhat more than an inch to the right side of the spine, from the fifth to the eighth dorsal vertebræ inclusive, which are deviated to the right; percussion of the spine at this part is painful: elsewhere, on the right side, there is resonance. The respiration generally, on the left side, is distant and bronchial; on the right side, exaggerated, except near where the spine deviates, at which part the respiration is distant. The expansion of the right side is more complete than of the left; on the left side, behind, there is only elevation, not expansion. The local fremitus on the right side is increased; on the left side, slight above, and absent in the lower half. The heart's action is heard over the whole of the left lung, and across to the right side behind, where the spine deviates: a slight systolic murmur is heard at the base; the apex of the heart beats more to the left, and slightly lower than natural.

A tumour is situated in the outer part of the right clavicle, and shows very prominent and distinct, resembling a flattened hen's egg: it feels hard to the touch, rounded, and even; painful on firm pressure, but not on slight pressure. It measures 3 inches in length, by 1⅘ inch in breadth. It appears to go somewhat deep down towards the apex of the lung. Indistinct pulsations can be felt in it, chiefly towards the outer margin, and a *bruit* can be heard in the tumour itself, which seems to be independent of any that may be communicated from the subclavian artery. Considerable pain is complained of in the tumour on raising the right hand to the head. The projection forwards of the tumour produces flattening below the right clavicle.

The abdominal region presents nothing abnormal.

The stump is well formed, with a somewhat broad glazy cicatrix: near the inner end of the femur there are two lumps, the larger of which is about the size of a small walnut, and is painful on pressure. There are occasional startings in the stump, which he describes as being more frequent and troublesome than they were, and he still has at intervals the referred sensation as if the toes were on.

With reference to the tumour in clavicle, he states that when he was at Worcester, in March, 1853, shortly before his return to London, he "wrenched" the right shoulder in throwing up a heavy bar of iron into a cart; the shoulder felt very stiff the next morning, and for some days afterwards he could not move the right arm without suffering pain.

On June 13th, I saw the patient, in consultation with Dr. Jenner, and it was then remarkable how much the central dulness of the right side behind had increased during that short interval, showing the rapid progress the disease was making at this time. The general condition of the patient was less favourable; he was weaker, and more anæmic; night-sweats profuse, and some diarrhœa. He was ordered cod-liver-oil, and a mixture of sulphuric acid with opium.

June 20th.—Better: the diarrhœa has ceased. He is able now to take exercise. Slight pain is complained of in the lower half of the left side of the chest.

June 27th.—Pain in the lower part of left side more severe, and slight pressure causes considerable distress. The left intercostal spaces appear more retracted than on the right side, especially the upper ones, as low down as the fifth rib.

In the early part of July the patient had decidedly improved; the lips were redder; the cough and night sweats abated; and he was able to go out almost daily for two or three hours at a time. Towards the latter part of this month a new group of symptoms made their appearance, which occasioned much distress; these were connected with *difficulty of* *deglutition*, of which he first complained to me on July 20th, but he had felt uneasiness in swallowing for a few days previously. He had no difficulty in swallowing fluids, but when he attempted to swallow solid food, he felt that there was some obstacle low down, which appeared to him to be close to the entrance into the stomach. I may here remark, that the difficulty of deglutition subsequently increased so as to interfere with his taking sufficient nourishment, and that after continuing severe for about four or five weeks, it gradually diminished towards the end of August, and that early in September he ceased altogether to complain of it. Whilst the dysphagia continued, great tenderness was complained of on pressure at the end of the ensiform cartilage.

The following examination was made at a period about midway between the first examination (June 6th), and his death (Oct. 7th):

Aug. 1st.—General appearance improved; cough severe early in the morning, but almost absent during the rest of the day; expectoration, a glairy, gelatinous kind of mucus, mixed with shreddy-looking matter, and streaked sparingly with blood.

Physical Examination of the Chest.—Increased dulness on percussion on the left side, with still slight resonance at the extreme apex. On the right side generally, extra resonance, except behind in the interscapular regions, from the spine of the scapula downwards to the lower angle, and the vertebral column; in the space so marked out, there is dulness on percussion. The vertebræ at this point, from the sixth cervical to the eighth dorsal, deviate to the right. Deep percussion on the right side in front, from the lower border of the fifth rib, to an inch below the false ribs, gives dulness. Respiration on the left side is scarcely perceptible on either calm or deep inspiration; on the right side, respiration generally is exaggerated, except in the interscapular region, where it has a tubular character. Vocal fremitus on the right side is well marked, except in the interscapular space; on the left side, it is absent. A loud blowing murmur is heard at the base of the heart, prolonged, and obscuring the second sound. This murmur is also heard in the carotids. There is a distinct systolic murmur in the clavicular tumour.

About this time he suffered much from severe shooting pains in the clavicular tumour, and its form, which had previously been oval, now underwent a change; and by irregularly enlarging on the outer side towards the acromion, subsequently assumed a cordate form, with the apex directed backwards and inwards. The following table of monthly measurements shows the progressive increase of this tumour:

	Length.		Width.
June 6th . . .	3 inches.	...	$1\frac{4}{5}$ inches.
„ 24th . . .	$3\frac{3}{4}$ „	...	$2\frac{3}{5}$ „
July 25th . . .	$4\frac{3}{5}$ „	...	$2\frac{4}{5}$ „
August 18th . .	$5\frac{1}{2}$ „	...	$3\frac{3}{10}$ „
September 24th .	$5\frac{4}{5}$ „	...	$4\frac{1}{2}$ „

From about the 10th of August to the end of the month, he suffered more pain than usual in the chest, especially at mid-sternum, and on the left side about the heart; the dysphagia was also greater at this period than at any other.

On examining the chest on September 7th, there was dulness on per cussion over the whole of the left lung, and over all the interscapular space on the right side. No respiration was heard in the left lung. Along the inner border of the right scapula, a fulness was noticed, elongated in form, in which a *bruit* could be heard.

The character of the expectoration during September was altered and peculiar. It now sometimes, but not always, presented the appearance of a glairy, pale, whitish-brown sputa, mixed with little clots of blood, resembling black-currant jelly. A feeling of tightness and pain at mid sternum was complained of just previous to this kind of expectoration appearing, and after spitting up "only phlegm" once or twice, some of this kind of sputa was spat up, and he felt better for a time in consequence.

In the following month, head symptoms made their appearance. It was on August 27th that he first drew my attention to his head, when he complained of great pain at the vertex and forehead, and photophobia, so that it was necessary to have the room darkened; the pupils, which acted readily, were a little more contracted than they should have been for the small amount of light in the room. Whether these symptoms passed off after this, or my attention was not directed to them, I have no further mention in my notes of anything bearing on the subject till September 17th, when the patient informed me, that on the previous afternoon, he was attacked with what he called a "tight fit," which he described as a "sensation of numbness," or "a kind of sleepy feeling," in the right arm; this continued about a quarter of an hour. Soon after this, he had convulsive movements of the right side, which commenced in the right arm. These convulsive attacks continued to appear till very near the time of his death. At first they did not come on more than once or sometimes twice a-day; but they soon increased in frequency and severity, lasting about two minutes, and being most troublesome at night. During the attacks, the right arm would be thrust violently forwards and retracted, its movements being rapid and uncontrollable, causing him great pain, and leaving him very exhausted. Towards the end of the afternoon of October 5th (two days before death), five "fits" of a more severe character than usual came on in quick succession, affecting the whole of the right side; the face during the convulsive attacks was much distorted, being drawn to the right side, and "the mouth quite twisted." It was observed, that whenever the fits were coming on, he seemed to be aware, for some time previously, of what was about to happen. He expressed himself as having curious, indescribable feelings, sometimes for two or three hours before the convulsive action commenced.

Throughout this long illness there was the same hope of recovery, and unwillingness to be persuaded of the fatal character of the disease as in phthisis, to which disease the general symptoms bore considerable resemblance. In September he suffered much from occasional severe diarrhœa, which was checked, but only for short intervals, by a mixture of catechu, sulphuric acid, and opium. The emaciation, which was arrested for a considerable time, advanced rapidly during the last three or four weeks of life, when well-marked hectic set in, with profuse sweatings, from an early hour in the morning till about noon; during the rest of the day, the skin was without any moisture, and generally very cold. About ten or twelve days before death, the right hand and foot became œdematous, and the skin over these parts was somewhat bluish; no œdema was observed elsewhere. No delirium was noticed beyond a "little rambling" during the night preceding his death. The patient lay, throughout the illness, on the left side, doubled up.

He died on Oct. 7th, at 7·40 P.M. At 1 P.M. he had eaten a moderately good dinner, consisting of roast mutton and potatoes, and he afterwards took, as usual, a little wine and water. He slept for some time after dinner, as he had lately been in the habit of doing; and about 5 P.M. had some tea. He expressed himself as feeling comfortable, and free from pain. Shortly before death he swallowed a spoonful of wine and water, complained of feeling very cold, and asked his mother to feel his hand, and whilst she held it he ceased to breathe.

*Post-mortem examination seventeen hours after death.**—Rigor mortis considerable, great emaciation, the right hand and right foot œdematous. *Head:*—Close to the margin of the sulcus, on the upper aspect of left hemisphere, corresponding to commencement of posterior lobe, a slight eminence is perceived, differing in colour from the adjacent convolutions, firm, and rather elastic; on cutting the surrounding brain-substance, a small rounded *tumour* is readily enucleated, of a form irregularly globular, inclining to oval; size about one inch by three quarters, and appearing as if transversely cleft into two unequal parts; the white substance about the tumour is more opaque, yellowish, custard-like, and softer than natural. On closer inspection this tumour is found to be intimately connected with the pia mater, numerous small vessels running on its surface, which are evidently continuous with those of the pia mater. The tumour itself is lobulated on the surface, the lobules varying in size, many of them are oblong, and imperfectly defined; between the lobules run the larger vessels, from which minute branches pass into the surface of the lobules, and as the colour of the tumour is delicate grey, the surface of the tumour bears a close resemblance, when seen through a simple lens, to the surface of the fœtal brain, the resemblance being rendered more close by the transparency of the mass. On opening the tumour, its surface is found to be covered by a layer of loose cellular tissue, highly vascular, and closely resembling the pia mater. One or more small cysts are now opened, from which escapes about half a drachm of transparent, slightly yellowish fluid, of the consistence of synovia, highly glutinous, and capable of being drawn out into threads eight or ten inches in length. *Under the microscope* this fluid presents numerous beautiful specimens of cancer cells—i.e., large spherical cells, of most delicate outline, transparent and large nuclei with well-defined outline, and large bright dots. On dividing the tumour, it is found that the cysts are limited, or almost so, to the surface; they are three or four in number, and contain fluid similar to that described. The remainder of the tumour is soft, opaque, cream-white, streaked with red, friable (ordinary encephaloid).

Right Clavicle.—The inner fifth of this bone is of about natural size, the outer four-fifths are involved in a double bony tumour, irregularly rounded and uneven on the surface. On section, the tumour is found to be bony throughout, intimately connected with the structure of the clavicle, and blocking up the medullary cavity. The outline of the clavicle is still distinctly traceable, the compact tissue of the shaft of the bone contrasting with the porous structure of the tumour.

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* It is with much pleasure that I acknowledge the prompt and courteous manner in which Mr. Quain favoured me with his notes and observations on the earlier history of the case; and also the assistance received from Dr. Jenner and Mr. Tomes in the microscopic examination of the diseased parts.

Cavity of the Thorax.—On the right side, the lung is somewhat retracted, and does not project beyond the bony part of the ribs; laterally there are some slight adhesions, and posteriorly adhesions so firm as to prevent the removal of the lung entire; these latter are due to the intimate connexion of the pulmonary pleura at this part with a hard and bony-feeling mass, corresponding with the fourth, fifth, and sixth ribs, and the adjoining vertebræ, and very firmly connected with both. In the removal of this tumour, the periosteum is stripped off, leaving the surface of the bones bare, but otherwise healthy. On the lower lobe of this lung are some nodular deposits, sub-pleural, varying in size from a large walnut to a hazel nut, and of a firm and grisly consistence, and near the base is a mass about two and a half inches in length by one inch at the broadest part, and one inch in depth, which, on section, is found to be an osseous tumour, with the bony matter in a granular form. The tissue of the lung around these deposits, and elsewhere, is crepitant and healthy.

The large osteoid tumour contained in this cavity is coarsely nodulated on the surface, and covered with dense fibrous tissue; it measures four and a half inches in length, five inches in width, and two and a quarter inches at the deepest part. Posteriorly, it is irregularly flattened, anteriorly rounded, and at its upper part covered with large nodules, some of which are movable. On section it exhibits an apparently bony structure, which towards the centre of the tumour is very dense and compact; some of the nodules referred to are not osseous.

Under the microscope the central portion of the tumour presents the following appearances:

Scattered irregularly over the field are a very large number of opaque bodies, varying infinitely in size and form, yet all in appearance essentially identical. Some of these bodies have the closest resemblance to, and probably are, lacunæ; from a few of these project processes which seem to be canaliculi; one of these lacunæ measures $\frac{1}{200}$th by $\frac{1}{100}$th of an inch in diameter, the same lacuna with its canaliculi measuring $\frac{1}{100}$th by $\frac{3}{200}$ths of an inch; the canaliculi are therefore evidently very short, they are also less clearly defined than are the canaliculi of ordinary bones. Others of these bodies resemble in form the spiculated nuclei that are seen occasionally in enchondromata; others again are more or less spherical; while some are most *bizarre* in outline.

As to size, the smallest measures about $\frac{1}{250}$th of an inch in diameter; the largest $\frac{2}{100}$th by $\frac{3}{200}$th of an inch.

Here and there are some bodies which resemble those last described, but their outline is less distinct; they seem made up of agglomerated granules.

In some parts, the bodies above-mentioned are closely packed together, accumulated into heaps of irregular shape.

The matrix in which the bodies are situated is transparent; has at some places an imperfectly marked fibrous appearance, and generally is somewhat granular; a few channels, circular in outline, pass through the matrix.

Where the opaque bodies (lacunæ?) are situated in the most fibrous-like matrix, they are generally oval in form, their long axis being situated so as to correspond with the direction of the fibres. The long diameter of one of these bodies is $\frac{7}{200}$ths, the short diameter of the same body $\frac{1}{100}$th of an inch. Where the imperfectly-defined fibres make a curve, there these long lacunæ are also curved.

Left Side.—The left lung at its upper part extends as far as the left margin of the sternum; it is firmly adherent in front, and quite distends this side of the chest. In proceeding to remove the lung, several large spaces, more or less circumscribed, were opened into, containing a thick and somewhat greenish purulent fluid (empyema), the odour of which was sickly and purulent, but not fœtid; the total amount exceeded two pints. The tissue of this lung is brittle, and breaks down easily on pressure; on section from base to apex, it is seen to be riddled with cavities, varying considerably in size, and filled with a semi-fluid, greenish-yellow, cheesy-looking matter: under the microscope, this is found to consist chiefly of cells about the size of pus cells, with single and double nuclei, a few having triple nuclei; some cells a little larger than pus cells; and ciliated epithelial cells (columnar).

The *Pericardium* is thickened, and throughout firmly and closely adherent.

Heart somewhat increased in size; muscular structure firm and of good colour, lining membrane and valves healthy; the left auricle is reduced in size, from being closely surrounded and pressed upon by cancerous masses.

Posterior Mediastinum.—Occupying the posterior mediastinum, and displacing the parts contained in it, is a bony tumour, about the size of a fist, irregularly nodulated on the surface. This tumour *above* completely fills up the space between the bifurcation of the trachea; *in front* are the heart and great vessels; *behind* is the œsophagus, lodged in a broad groove on its posterior surface, to which it is bound down by firm cellular tissue. On section, the tumour presents the same granular bony appearance as the smaller one described near the base of the right lung, and seems to be made up of several altered bronchial glands. The descending part of the arch, and the thoracic aorta, to the extent of about three inches, are closely surrounded, except on the inner side, where it is in relation with the œsophagus by an elongated, irregularly-shaped, cartilaginous-looking mass, which on section is found to contain scattered, radiating, bony spiculæ towards the centre. The aorta and intercostal arteries embedded in this tumour do not appear to have been compressed by it. The coats of the aorta at this part are very thin, being less than half the ordinary thickness; its lining membrane is smooth and apparently healthy.

The Trachea, for about one inch above the bifurcation, is somewhat pressed upon posteriorly by a cartilaginous mass, forming an appendix to the mediastinal tumour. The right bronchus, for about three inches, is closely surrounded, except in front, by the bony tumour, which, on slitting up the tube, is seen to project internally. The mucous membrane is much injected, and covered with a reddish, viscid secretion. The left bronchus is completely surrounded by a mass of enlarged and diseased bronchial glands; at its commencement it is blocked up with a greenish cheesy-looking mass, which assumes, a little further on, a cartilaginous appearance, and subsequently becomes calcareous. The œsophagus, as already described, lies in a groove on the posterior surface of the mediastinal tumour, and is partly overlapped by the cartilaginous mass surrounding the descending aorta. It appears healthy; muscular fibres of good colour; lining membrane very pale. The vena azygos is partly blocked up with an elongated cartilaginous-looking mass, which is loose in its interior; at one point, the coats of the vein have been perforated by a narrow band connecting the mass within with a similarly elongated mass outside the vein.

The *Peritoneum* and *Viscera* of the *Abdomen* and *Pelvis* appear healthy on careful inspection.

Stump.—The cicatrix and distal end of the femur appear healthy. On sawing off about an inch of the bone, the compact tissue is found to be much thinner than usual, medullary cavity occupying the rest. The cavity is filled with red, soft, and apparently healthy medullary matter. The two swellings noted during life on the anterior aspect of stump are bulbous enlargements of the cut extremities of nerves.

Remarks.—As osteoid cancer has only been recognised within the last twelve years, and our knowledge of the disease is, from the extreme rarity of its occurrence, necessarily very limited, a few general remarks are all that can be offered on some of the leading points of interest in connexion with the present case.

The primary appearance of the disease at an early age in the femur, with its subsequent development in the cavity of the thorax, has been observed in so large a proportion of the cases recorded, that it may be looked upon as one of the most characteristic features of the disease. As an alleged exception to this, Müller cites a case in which the primary tumour was loose at the inside of the right thigh, between the vastus internus and sartorius muscles; *an osteoid tumour was found in this case, after death, in the middle of the right femur;* and there are two others alluded to by Paget, the particulars of which are not given.

There is no instance on record of an osteoid tumour occurring in the brain; in the present case the recurrent disease, when it attacked that organ, assumed the character of ordinary encephaloid. The co-existence of these two (distinct?) forms of cancer, is a point of considerable importance in the pathology of the disease, and favours the opinion which Paget seems disposed to entertain—viz., that osteoid tumours may be looked upon "as examples of fibrous or medullary cancers ossified."

The diagnosis of the tumour on the clavicle offered at first some difficulty, owing to its so closely resembling, both in situation and character, the callus consequent on fracture, which there were some grounds for suspecting, from the injury sustained at Worcester; but the previous history of the case, the chest symptoms, and the rapid growth of the tumour itself, were subsequently sufficient to indicate its true character. The disease in the chest appears to have been secondary to this affection of the clavicle.

Respecting the essential cause of osteoid cancer, nothing definite is known, and from the small number of cases yet observed, and from the regular but unaccountable sequence of the symptoms, it is difficult to say what circumstances would be best calculated to give rise to a disease of so peculiar and fatal a character in a young subject. It may be readily supposed that the tendency to the excessive secretion of bone would be more likely to occur before than after the age of thirty; but this osseous diathesis is not malignant, and it is probably only on those rare occasions, when it co-exists with a predisposition to cancer, that we obtain the conditions favourable to the development of the disease. It is remarkable that in France, where pathology is so much cultivated, no example of osteoid cancer has hitherto been met with; should further observation confirm this, it may be the means of supplying us with some clue to the subject, by showing how far certain peculiarities of diet or habits of life may tend to produce the disease. In many of the cases on record, injury, usually of a slight character, has been assigned as the exciting cause; such was the case in this instance, both previously to the femur and to the clavicle becoming affected; and it is probable that in some of those cases where no exciting cause is assigned, there has been local injury, which has escaped notice.

Osteoid tumours appear to enlarge by fibrous deposition on the surface, and in those connected with the soft parts the deposit generally occurs in the form of nodules, which may or may not become ossified. In the case under notice, as in others in which the aorta has been found imbedded in one of these tumours, the calibre of the vessel is not perceptibly diminished, showing that the enlargement has been effected in this manner; for were it otherwise, from the rapidity with which the increase takes place, death would quickly ensue, from obstruction to the circulation. Whether the osseous ingredient is always a subsequent deposit, is uncertain, but it seems to have been so in most of the morbid growths in the present instance; and in that surrounding the aorta there is exhibited, on section, a good illustration of the mode in which ossification may commence in one of these thoracic tumours, a process analogous to what takes place in ordinary ossifying cartilage: the bony matter, which is in very small quantity, having been deposited in detached radiating points, near the centre of the nodulated fibrous tumour.

Lastly, the generally rapid progress of the disease has been cited as an evidence of its malignant character. Paget relates a case in which the patient died three and a half months after its first appearance; and in the one observed by Müller, at Berlin, the duration of the disease was less. The instances on record of patients having survived beyond the first year, are very few; and in some cases an operation has appeared rather to hasten the fatal termination. The present case is an exception to this, presenting not simply an example of a patient living for nearly four years after the first appearance of the disease, but also affording an encouraging proof of the success which may occasionally attend an operation for its removal. The immunity from the disease which this patient enjoyed for a period of three years after amputation of the thigh, is the most important feature in the case; for however attractive many of the facts related may be to the pathologist, it is chiefly with reference to the success attending the operation that it will be considered valuable by the surgeon, who will hardly fail to regard it as an instance both of life being prolonged and health restored by the removal of the limb in which the disease was first seated, and apparently localized.*

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* In a case of osteoid cancer of the right femur, which I have had the opportunity of seeing at University College Hospital, under the care of Mr. Quain, since writing the above, secondary bony deposits were found under the pleuræ, in the lungs, diaphragm, and omentum. The patient was a man, aged fifty-five years, who had had his right thigh broken by a sheep running against it about eleven months previously. No symptoms were noticed in this case during life indicating any affection of the chest.

PART FOURTH.

Chronicle of Medical Science.

REPORT ON THE PROGRESS OF ANIMAL CHEMISTRY DURING THE YEARS 1852-3-4.

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(*Concluded from No.* 30, *p.* 552.)

The Blood.—On this fluid, comparatively few memoirs calling for any lengthened notice have been published.

In connexion with its *analysis*, we may mention the following papers:

1. **Vierordt**:[1] A New Method of Analysing the Blood, Chemically and Microscopically.
2. **Welker**:[2] On Certain Modifications of Vierordt's Method.
3. **Funke**:[3] On Certain Sources of Error in Vierordt's Method.
4. **Schmidt**[4] (of Dorpat): On Vierordt's Method of Analysing the Blood.
5. **Zimmermann**:[5] On Blood-Analyses.
6. **Du Bois-Raymond**:[6] A Criticism on Blood-Analyses.
7. **Vierordt**:[7] On Blood-Analyses.
8. J. **Vogel**:[8] A Simple Method of Determining the Amount of Chlorides in the Blood.

Our limited space prevents us from noticing the contents of the first seven papers, none of which are of any special interest; we give, however, Vogel's method of determining the chlorides—it is as follows: A measured volume of blood is treated with nitric acid, and a graduated solution of nitrate of silver is then added from a burette, till a filtered specimen of the solution is no longer rendered turbid by nitrate of silver. In this way, the amount of chlorides in the blood may be tolerably accurately determined in a few minutes, without previous incineration or the use of the balance.

Amongst the memoirs bearing on the blood generally, we may mention Lecanu's[9] "Nouvelles Etudes Chimiques sur le Sang;" a memoir by Hétet,[10] in which he maintains that alkaline sulpho-cyanides exist in the blood, which take up oxygen in the lungs with the development of a scarlet colour, which oxygen they again give off in the capillaries; Bruck[11] "On the Colour of the Blood;" and Williams's philosophic memoir "On the Blood—its Chemistry, Physiology, and Pathology," now in the course of publication in this review.

The blood of the spleen has been very fully examined by Mr. Gray,[12] who was assisted in his chemical researches by Dr Noad. We give a tabular view of his most important results:

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[1] Arch. für Physiol. Heilkunde, vol. xi. pp. 26—73, 327—332, and 547—558.

[2] Fechner's Centralblatt, p. 218. 1853.

[3] Schmidt's Jahrbücher, vol. lxxiv. p. 1; and vol. lxxviii. p. 5.

[4] Zeitsch. für rat. Med., new series, vol. ii. p. 293.

[5] Arch. für Physiol. Heilkunde, vol. xi. p. 278; and vol. xii. p. 155.

[6] Zeitschr. für rat. Med., new ser., vol. iv. p. 44; and vol. v. p. 101.

[7] Ibid., p. 401.

[8] Corresp.-Bl. des Vereins für gem. Arbeiten zur Förderung der Wissenschaft. Heilkunde, p. 44. 1853.

[9] Journal de Pharm., vol. xxii. pp. 244, 337; and Comptes Rendus, vol. xxxiv. p. 11.

[10] Comptes Rendus, vol. xxxxiv. p. 410.

[11] Zeitschr. für Wissen. Zool., vol. iv. p. 373.

[12] Op. cit. pp. 136—177.

Table showing the average results of 111 *analyses of aortic, jugular, and splenic venous blood* (*horses*).

	Aortic.		Jugular.		Splenic.
Clot	159·50	...	141·00	...	95·12
Ash in ditto	1·04	...	0·86	...	0·71
Serum	840·50	...	859·00	...	904·88
Specific gravity of ditto	1032·5	...	1031·14	...	1032·24
Water	789·14	...	793·42	...	829·81
Albumen	42·00	...	54·40	...	63·00
Fibrin	2·26	...	4·15	...	6·32
Fat in ditto	·04	...	0·05	...	0·22
Globules	157·20	...	136·80	...	88.58
Oily matters	·30	...	·35	...	·64
Crystalline fat		...	·49	...	·92
Alcohol extract	2·42	...	1·61	...	3·27
Water extract	6·64	...	8·73	...	7·24

The following are the chief chemical peculiarities of splenic venous blood: there is a very considerable diminution of the blood-corpuscles, an increase of the iron, albumen, and fibrin, and a deep reddish-brown colour of the serum.

Lehmann[13] has suggested a new method of determining the *average quantity of blood* in the body of an animal: "If," he observes, "we know how much sugar the blood may, under the most favourable conditions, contain, without its appearing in the urine, and if we determine how much sugar the blood may normally contain on an ordinary diet, we may be able to calculate the quantity of blood contained in an animal by ascertaining the quantity of sugar which must be introduced by injection into the jugular veins, or by some other method, in order to make it pass into the urine. We know, from v. Becker's investigations, that about 0·5 per cent. of sugar may exist in the blood without passing into the urine; and that, further, after the use of saccharine roots, the blood contains 0·67 per cent. of sugar. Now, I find from my own, as well as from Uhle and v. Becker's injections of sugar (grape-sugar), that when 0·2 of a gramme (rather more than 3 grains) of sugar was injected into the blood of rabbits of the ordinary size (1200 grammes' weight), the urine indicated the presence of sugar in twenty-five minutes. If, now, we assume that, in a rabbit weighing 1 kilogramme, 0·15 of a gramme of sugar injected into the blood will saturate it to such an extent, that if there were any additional quantity of sugar it would appear in the urine, a rabbit of this weight will contain 95·8 grammes of blood."

We should altogether exceed our allotted space were we to enter into the consideration of the various contributions which have been made during the last three years to the chemical pathology of the blood. The results of the recent labours of Becquerel and Rodier in this department are collected in their 'Traité de Chimie Pathologique;' and we must refer those of our readers who wish to pursue this subject further to the admirable reports "On the Pathology of the Blood," which Professor Vogel has published in Canstatt's 'Jahresbericht.'

Amongst the memoirs of most general interest in this department, we may mention the following:

1. VERDEIL:[14] A Comparison between the Amount of Urea in the Blood in Albuminuria and in Normal Blood.
2. SCHERER:[15] An Examination of the Blood in Leucæmia.

Verdeil has submitted normal blood and the blood of a person with albuminuria to the same kind of analysis, and has arrived at the result, that the latter contains twenty times more urea than the former. The following was his method of procedure: The blood is coagulated on the water-bath, after being previously acidulated with a few drops of acetic acid. The clear liquid, separated by filtration from the coagulum, is then evaporated on the water-bath, and when it is reduced to one-tenth of its original volume, it is treated with alcohol of 36°, till any further addition induces no further turbidity. After standing for twenty-four hours, the fluid is again filtered and evaporated; a little water, acidulated with sulphuric acid, is then added, and the fatty acids which are thus liberated are removed by filtration. We now neutralize the fluid with carbonate of baryta, and evaporate to dryness in a vacuum. The dry mass is then extracted with cold absolute alcohol, which dissolves the urea. On adding to the alcoholic solution double its volume of ether, a precipitate is formed, and the urea in a state of perfect purity remains alone in solution in the mixture of alcohol and ether, from which it may be obtained in a crystalline state by evaporation.

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[13] Physiological Chemistry, vol. iii. p. 533.

[14] Gaz. Méd., p. 191, 1853; and Comptes Rendus de la Soc Biol., vol. v. p. 13. 1853.

[15] Verhandl. der Phys.-Med. Gesellsch. in Würzburg, vol. ii. p. 321. 1852.

The following are the most important conclusions which Scherer draws from his analysis of leucæmic blood. In addition to the ordinary constituents, it contained:

1. A substance allied to, if not identical with, gluten.
2. A peculiar organic matter, probably forming an intermediate link between the albuminous and gelatinous groups.
3. Hypoxanthine, the substance formerly detected by Scherer in the spleen, and recently found by Gerhard in ox-blood; it was present in mere traces.
4. Formic, acetic, and lactic acids.

CHYLE AND LYMPH.

The *Chyle* of the cow has been submitted to chemical analysis by Lassaigne.[16] The fluid was obtained for him by M. Colin, of the Veterinary School at Alfort, by inserting a silver canula into the thoracic duct. Several pints of the fluid were thus obtained. The chyle was alkaline, very fluid, opalescent, and of a reddish tint; a transparent jelly was formed in it after twenty-four hours' rest. In 1000 parts of this chyle there were:

Water	964·0
Fibrin	0·9
Albumen	28·0
Fat	0·4
Chloride of sodium	5·0
Carbonate, phosphate, and sulphate of soda	1·2
Phosphate of lime	0·5

It appears, from the researches of Bidder and Schmidt,[17] that the *quantity* of fluid which passes daily from the thoracic duct into the general circulation is about equal to the quantity of blood, and averages about one-fifth of the whole weight of the body. From their experiments on the lower animals (dogs and cats), they calculate this quantity for an adult man at 13 kilogrammes (or about 28·6 lbs.), of which only 3 kilogrammes (or 6·6 lbs.) are true chyle obtained directly from the digested food, while the remaining 10 kilogrammes (or 22 lbs.) are true lymph. Hence the quantity of lymph formed and conveyed into the blood in twenty-four hours amounts to one-seventh or one-eighth of the weight of the whole body.

THE MILK.

The following memoirs have appeared on this subject:

1. Vernois et Becquerel:[18] Du Lait chez la Femme dans l'état de Santé et dans l'état de Maladie. Mémoire suivi de nouvelles recherches sur la composition du lait chez la vache, la chèvre, la jument, la brebis et la chienne. Paris, 1853. pp. 198.
2. Moleschott:[19] Chemical and Microscopical Notices of the Milk.

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[16] Journ. de Chim. Méd., p. 348. 1853.

[17] Verdauungssäfte und Stoffwechsel, p. 284.

[18] Reprinted from the Annales d'Hygiène Publique et de la Médecine Légale.

[19] Arch. für Physiol. Heilkunde, vol. xi. p. 696.

3. **Schlossberger**:[20] On the Reaction of Fresh Milk, and on the so-called Hexenmilch (Witches' Milk).
4. **Doyère**:[21] The Milk Studied Physiologically and Economically.
5. **Doyère et Poggiale**:[22] On the Presence of an Albuminous Substance in the Milk which Polarizes Light to the Left.
6. **Wildenstein**:[23] An Analysis of the Ash of Woman's Milk.
7. **Girardin**:[24] On the Occurrence of Albumen in the Milk.
8. **Guillot**:[25] On the Milky Fluid in the Breasts of Young Children.
9. **Joly et Filhol**:[26] Remarkable Cases of Milky Secretion.

Of these various memoirs, by far the most important is that of Vernois and Becquerel. The first part treats of the milk of healthy nurses, and the physiological influences which may affect it; the second part is devoted to the milk in disease; the milk of the cow is then considered in some detail, and afterwards the milk of the other animals whose names are recorded in the title-page.

The first part commences with the determination of the composition of human milk in its normal or physiological state.

The following table gives the composition of healthy milk, calculated for 1000 parts. It is based upon eighty-nine observations.

Healthy Human Milk.

	Mean.		Maximum.		Minimum.
Density	1032·67	...	1046·48	...	1025·61
Water	889·08	...	999·98[27]	...	832·30
Solid constituents	110·92	...	147·70	...	83·33
Sugar	43·64	...	59·55	...	25·22
Casein and extractive matters	39·24	...	70·92	...	19·32
Butter	26·66	...	56·42	...	6·66
Salts yielded by incineration	1·38	...	3·38	...	0·55

The next table shows the influence which the age of the nurse exerts on the milk.

	15 to 20.	20 to 25.	25 to 30.	30 to 35	35 to 40.	Normal state.
Density	1032·24 ...	1033 08 ...	1032·20 ...	1032·42 ...	1032·74 ...	1032·67
Water	869·85 ...	886·91 ...	892·96 ...	888·06 ...	894·94 ...	869·08
Solid constituents	130·15 ...	113·09 ...	107·04 ...	111·94 ...	109·06 ...	110·92
Sugar	35·28 ...	44·72 ...	45·77 ...	39·53 ...	39·60 ...	34·61
Casein and extractive matters	53·74 ...	38·73 ...	36·53 ...	42·33 ...	42·07 ...	39·24
Butter	37·38 ...	28·21 ...	23·48 ...	28·64 ...	22·33 ...	26·66
Salts yielded by incineration	1·80 ...	1·43 ...	1·26 ...	1·44 ...	1·06 ...	1·38

Hence the age of the nurse does not seem materially to affect the specific gravity or the relative amounts of water and of solid constituents in the milk. The individual constituents seem to differ more from the normal scale between the ages of fifteen and twenty than afterwards. On the whole, the period at which the milk most nearly approaches the physiological standard is from the twentieth to the thirtieth year.

The next table exhibits the influence of the age of the milk from the first month to the end of the second year, and shows that no definite law can be deduced regarding the augmentation or diminution of the specific gravity, which apparently reaches its maximum in the seventh, and its minimum in the twelfth month.

The influence of the constitution of the nurse is next considered, and a very unexpected result obtained.

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[20] Ann der Chem. und Pharm., vol. lxxxvii. p. 317.

[21] Ann. des Sc. Nat., troisième série, vol. xxvii. p. 193.

[22] Comptes Rendus, vol. xxxvi. p. 430.

[23] Journ. für Pr. Chem., vol. lviii. p. 28.

[24] Comptes Rendus, vol. xxxvi. p. 753.

[25] Ibid., vol. xxxvii. p. 609. [26] Ibid., pp. 271.

[27] There is obviously an error in this number.

	Nurse with strong constitution.	Nurse with weak constitution.
Density	1032·97	1031·90
Water	911·19	887·59
Solid constituents	88·81	112·41
Sugar	32·55	42·88
Casein and extractive matters	28·98	39·21
Butter	25·96	28·78
Salts yielded by incineration	1·32	1·54

We here find that the milk yielded by the nurse with the weak constitution very closely corresponds with normal milk, while that of the strong woman presents a great deficiency of sugar and casein.

There is very little difference in the chemical characters of the milk of a woman with her first child and of that of a woman who has borne several. On the whole, the milk of a *primipara* approximates most closely to the normal mean.

Pregnancy is commonly held to exert a deleterious influence on the nurse. The authors have only one analysis bearing on this point.

	Three months' pregnancy.	Normal state.
Density	1030·67	1032·67
Water	860·97	889·08
Solid constituents	139·01	110·92

The main points of difference are the augmentation of the solid constituents, especially the butter. An observation on the cow's milk at the third month of gestation gives similar but less marked results, in so far as the augmentation of the butter is concerned.

The amount of the development of the mammary glands exerts no definite influence on the composition of the milk; and the same may be said regarding the longer or shorter stay of the milk in the breasts. (In the cow and the ass, however, we find that the milk which is last drawn contains a great excess of butter; this is no doubt due to the cream rising in the udder, just as it does when the milk is placed in any vessel.)

The influence of menstruation is next considered. In 89 cases, there was suspension of the menses in 79 cases, and they were present ten times; in 3 of these latter cases, the milk was analysed during menstruation.

	Suspension of menstruation.	Menstruation regular.	During menstruation.	Normal state.
Density	1032·24	1031·94	1031·48	1032·67
Water	889·51	886·44	881·42	889·08
Solid constituents	110·49	113·56	118·58	110·92
Sugar	43·88	41·68	40·49	43·64
Casein and extractive matters	38·69	43·58	47·49	39·28
Butter	26·54	26·98	29·15	26·66
Salts yielded by incineration	1·38	1·32	1·45	1·38

The most striking point here is the regularity of the modifications impressed upon the amounts of the abnormal constituents. During actual menstruation the milk differs considerably from the normal type.

The nature of the food influences, as is well known, the milk of the lower animals; woman's milk is, however, less affected by this cause than might have been supposed. In the following table, I. represents the milk of a very well-fed nurse, and II. that of a woman with a very deficient supply of nourishment:

	I.	II.	Normal milk.
Density	1031·27	1030·45	1032·67
Water	876·49	895·69	889·08
Solid constituents	123·51	104·31	110·92

This portion of the work concludes with the consideration of the influence which the quantity of the milk exerts upon its chemical characters. In 89 cases, the milk was abundant and escaped with facility 60 times, and was scanty in the remaining 29.

The general inference arrived at is, that milk which flows abundantly approaches more nearly to the normal standard than scanty milk.

The authors analysed the milk in 18 cases of acute disease (enteritis, pleurisy, colitis, typhoid fever, &c., &c.), and in 27 cases of chronic disease (chronic ophthalmia, chronic pleurisy, chronic enteritis, pulmonary tuberculosis, syphilis, &c., &c.). The mean results are given in the following table:

	Acute diseases.	Chronic diseases.	Normal state.
Density	1031·20	1031·47	1032 67
Water	884·91	885·50	889·08
Solid constituents	115·12	114·50	110·92
Sugar	33·10	43·37	43·64
Casein	50·40	37·06	39·24
Butter	29·86	32·57	26·66
Salts	1·76	1·50	1·38

Moleschott's observations are more of a microscopical than of a chemical nature. He mentions 2 cases in which stall-fed cows secreted *acid* milk.

The reaction of fresh milk has been specially investigated by Schlossberger. He combats the idea that the reaction of milk generally is alkaline, and shows that this is strictly true only for human milk, that the milk of the domesticated herbivorous animals is very irregular in this respect, and that the milk of carnivorous animals is probably as normally acid as human milk is alkaline. In the Stuttgard Maternity the milk was tested 385 times: it was never acid, 45 times neutral, and in all the other cases more or less decidedly alkaline. In 272 observations in the "Clinique" of Professor Breit, a slight acidity was twice observed, and Schlossberger explains these exceptional cases by supposing that the nipple was not perfectly freed from some rancid butter with which it had been rubbed on the previous day; in some few cases it was doubtful whether it was neutral or alkaline, but decided alkalinity was exhibited in the great majority of cases. For the corresponding data in relation to the milk of the cow, the sheep, the mare, the dog, and the cat, we must refer to the original memoir.

Passing over the two next memoirs, we give the result of Wildenstein's analysis of the ash of normal human milk. After the deduction of the carbonic acid and carbon, he found:

	Per cent.		Per cent.
Chloride of sodium	10·73	Phosphoric acid	19·00
Chloride of potassium	26·33	Phosphate of iron	0·21
Potash	21·44	Sulphuric acid	2·64
Lime	18·78	Silica	a trace
Magnesia	0·87		

The milky fluid which is often secreted by the breasts of infants has been independently examined by Guillot and Schlossberger (it is the "Hexenmilch" of the latter chemist). According to Guillot, it has the same composition as woman's milk, exhibits a neutral or alkaline reaction, becomes acid on exposure to the air, and separates into a serous and a creamy portion. Schlossberger was able to collect about a drachm from the breast of a male infant. It had the appearance of watered milk, was decidedly alkaline, exhibited under the microscope the normal milk-corpuscles, but no colostrum or pus globules. It exhibited a strong reaction of sugar (by Trommer's, Moore's, and Pettenkofer's tests). It did not coagulate on heating, but flakes were separated both by acids and by rennet. In 100 parts there were contained (according to an analysis by Hauff, who examined it at Schlossberger's request)—Water, 96·75; fat, 0·82; casein, sugar, and extractive matters, 2·83; ash, 0·05.

THE FLUIDS OF THE EGG.

An elaborate memoir on this subject has been published by Valenciennes and Frémy,[28] who have analysed not only the eggs of birds, but those of fishes, reptiles, and various invertebrate animals.

THE SWEAT.

This fluid has been recently examined with great care by Favre,[29] a French chemist, and by Schottin,[30] a pupil of Lehmann's. Their most important observations are condensed in the following remarks:—

Favre, who asserts that he has operated on 40 litres (or 8·8 gallons) of sweat, maintains, that after prolonged sweating the secretion becomes neutral, and finally alkaline; Lehmann, however, was unable to confirm this observation; it is to be regretted that Favre has not stated how he collected this enormous quantity.

The solid constituents amount, according to Schottin, to 2·26%, while according to Favre they do not exceed 0·443%. In these 2·26% of the solid constituents of normal sweat, Schottin found 0·42 of epithelium and insoluble matters. In 100 parts of the ash of the sweat he found 31·3 parts of chlorine, combined with 28·2 of sodium and 11·1 of potassium; the ratio of the potassium to the sodium in the ash was as 15·7 : 27·5.

In the ash of the sweat from the feet he found 4·1% of phosphate of lime, and 1·4% of phosphate of magnesia and oxide of iron. Moreover, in two closely coinciding analyses of the ash of sweat from the feet and arms, he found 5·5% of insoluble and 94·5% of soluble mineral constituents.

The organic acids of the sweat were never strictly investigated until Schottin undertook the examination of this fluid: he has demonstrated with the greatest certainty the presence of *formic* and *acetic acids* in it. Lehmann considers it singular that the formic acid should preponderate so much as seems to be the case, over the other volatile acids; the acetic acid was in far smaller quantity, and butyric acid was present in mere traces.

For a long time the presence of *lactic* acid in the sweat was regarded as an accepted fact; but Lehmann failed in detecting any trace of it in the sweat either of puerperal women or of persons suffering from gout or rheumatism; and it was unquestionably proved that this acid was not present in the sweat collected by Schottin. Favre, who seems to have entirely overlooked the presence of volatile acids in the sweat, maintains, however, that he has not only demonstrated the existence of this acid by the exhibition of its zinc-salt and elementary analysis, but that he has determined the actual quantity of the lactates of potash and soda in the sweat at 0·0317%.

Favre further believes that he has discovered a new nitrogenous acid in the sweat, to which he has given the name of *hydrotic* or *sudoric acid.* From two elementary analyses of its silver-salt he assigns to it the formula $C_{10}H_8NO_{13}$.

With regard to the presence of *urea* in the sweat, Favre regards it as a normal constituent, and he thinks that it is upon its presence, or that of a similar substance, that the readiness with which the fluid becomes alkaline depends; but notwithstanding the most careful search, Schottin failed in detecting it, either in the normal sweat generally, or in the sweat of the feet, which so soon becomes alkaline. Schottin, however, made the interesting observation, that in *uræmia* (especially when occurring in cases of cholera) considerable quantities of urea pass into the sweat.

We sometimes find the bodies of persons who have died from cholera coated with a thin bluish layer, which on closer examination is found to consist of a fine powder, composed, for the most part, of urea.

Lehmann was unable to detect any trace of *sugar* in the sweat of a diabetic patient, who, contrary to the general rule, perspired very copiously in a hot summer.

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[28] Journ. de Pharm., troisième série, vol. xxvi. p. 321.

[29] Arch. Gén. de Méd., cinquième série, vol. ii. pp. 1—21.

[30] Arch. für Physiol. Heilkunde, vol. xi. pp. 73—104.

Landerer,[31] however, declares that he has recognised the presence of sugar, "with all certainty," by means of Heller's test (Moore's potash-test), in the sweat of a diabetic patient.

Schottin has instituted several very admirable experiments on *the passage of several matters into the sweat*, and from these it would appear that benzoic acid, and also succinic and tartaric acids, pass very rapidly and unchanged into the sweat. Iodide of potassium was not detected in the sweat until it had been taken for five days (half a drachm daily). When salicin was taken, neither this substance itself nor any of its known products of decomposition, could be detected in the sweat. Quinine, taken to the amount of twelve grammes, did not pass into the sweat. After the ingestion of much sugar of milk, neither a saccharine matter nor lactic acid appeared in the sweat.

[Want of space compels us to defer those points connected with the chemistry of the urine, which are not included in the paper on Urology, in a previous part of this number of the Review.—EDITOR.]

We now proceed to notice the most important contributions to our knowledge of the chemistry of the SOLIDS OF THE ANIMAL BODY; and commence with the consideration of *Bone* and *Cartilage*, including the *Teeth*.

All the substances have been very elaborately discussed by Schlossberger,[32] in a monograph extending over upwards of 100 pages, to which we must refer those of our readers who take a special interest in the chemistry of these tissues. The most important contribution to our knowledge of the osseous tissue, is a memoir by Flourens, which, as it was not fully published till the beginning of the present year, will be postponed to our next Report.

If we exclude those memoirs which are of a microscopical rather than of a purely chemical character, the only papers on *Cartilage* to which we need refer, are one by Hoppe[33] "On Chondrin, and certain Products of its Decomposition;" and one by Virchow[34] "On the Gelatin extracted from the Sheaths of Tendons and from the Inter-vertebral Cartilages." A brief analysis of the former (the more important) of these papers will be found in Lehmann's 'Physiological Chemistry,' vol. iii. p. 496.

Joy[35] has published analyses of the inner and outer parts of the tooth of the Narwhall, and Fricke[36] of Fossil Ivory.

Zollikofer's[37] "Contributions to our Knowledge of *Elastic Tissue*" have been already noticed in this Review. (See vol. xi. p. 279.)

The only addition to our knowledge of the *horny tissues*, is an analysis by Diez[38] of rhinoceros' horn. After deducting the ash, it yielded C 50·55, H 7·36, N 15·68, O 26·03, S 0·38; this amount of nitrogen is intermediate between that of gelatin and chondrin; the chemical relations of this substance resemble those of gelatin.

Piria[39] has shown that both leucine and tyrosine may be obtained from horn by the action of sulphuric acid and prolonged boiling.

In connexion with *muscular tissue*, we may especially notice the researches of Marchal,[40] Lassaigne,[41] Thiel,[42] Echevarria,[43] Schottin,[44] and Grohé.[45]

[31] Arch. für Chem. und Mikrosk., p. 11. 1853.

[32] Erster Versuch, &c. Stuttgart, 1854.

[33] Journ. für Pr. Chem., vol. lvi. p. 129.

[34] Verhandl. der Phys.-Med. Gesellsch. zu Würzburg, vol. ii. p. 281.

[35] Ann. der Chem. und Pharm., vol. xxviii. p. 365.

[36] Ibid., vol. cx. p. 100.

[37] Ibid., vol. lxxxii. p. 162.

[38] Ann. der Chem. und Pharm., vol. xc. p. 303.

[39] Ibid., vol. lxxxii. p. 251.

[40] Comptes Rendus, vol. xxxiv. p. 591.

[41] Journ. de Chem. Méd., troisième série, vol. ix. p. 155.

[42] Ann. der Chem. und Pharm., vol. lxxxi. p. 370.

[43] Ibid., p. 373.

[44] Arch. für Physiol. Heilk., p. 622. 1852.

[45] Ann. der Chem. und Pharm., vol. lxxxv. p. 233.

Marchal attempted to determine the relative nutrient value of different kinds of flesh, and for that purpose ascertained the amount of solid matter and fat in 100 parts of flesh, from which all extraneous matters, as cellular tissue, bone, &c., had been removed. The following are his results:

	Pig.		Ox.		Sheep.		Hen.		Calf.
Water	69·7	...	72·5	...	73·6	...	73·7	...	74·4
Dry muscle, free from fat	24·3	...	25·0	...	23·4	...	24·9	...	22·7
Fat	6·0	...	2·5	...	3·0	...	1·4	...	2·9

Lassaigne has determined the relative quantities of water, organic matters, and chlorides in fresh and salted pork, and in bacon.

Thiel has analysed the ash of salted beef and of salted pork, and Echevarria that of fresh pork.

Schottin's "Investigations regarding the Quantity of Water in the Muscles in different Pathological Conditions," are of considerable value. He determined the quantity of water in the muscular tissue in forty-one cases, and in most of these he also determined the quantity of water in the serum of the blood drawn from the jugular vein. The interval that elapsed between the death of the patient and the section, and the disease which caused death, are also noted in his tables.

We extract a few of his results, from the higher and lower portions of his table.

Muscle-water.		Water of serum.		Cause of death.		Interval previous to examination.
822·04	...	928·37	...	Pulmonary tubercules.	...	10 hours.
816·04	...	923·00	...	Ditto.	...	12 „
814·25	...	909·09	...	Pneumonic infiltration of right lung.	...	9 „
809·10	...	901·26	...	Atrophy of the liver and general dropsy.	...	10 „
792·79	...	902·13	...	Pleuritic exudation.	...	15 „
790·00	...	894·55	...	Typhus in the stage of infiltration.	...	6 „

The largest quantity of water found in 1000 parts of muscle was 822·04; the smallest 730 and 729 in two cases of advanced cholera.

The following are the conclusions at which he arrives:

1. That the quantity of water in the muscles is proportional to that occurring in the serum, whatever may be the morbid conditions present; and that this proportion does not change for twelve or fifteen hours after death.

2. That in acute transudations the quantity of water in the muscles is diminished proportionally to the duration of the transudation.

3. That in chronic transudations through the serous membranes, the quantity of water in the muscles, although not below the normal proportion, is certainly not augmented.

Grohé's experiments have reference more to the *muscular juice* than to the actual muscular tissue itself. Moleschott[46] having maintained that he found oxalate of urea, together with other oxalates, in the muscular juice of frogs whose livers had been some days previously extirpated, Grohe repeated this experiment, and found that neither urea nor oxalic acid exists in this fluid, and that the crystals supposed by Moleschott to consist of oxalate of urea, are in reality composed of creatine, creatinine, and nitrate of potash.

The *Nervous Tissue* next claims our attention, and here we find several memoirs deserving of a careful notice. We may especially mention—

1. Von Bibra's[47] Comparative Investigations of the Brain of Man and the Mammalia. (This is an expansion of his memoir "On the Brain" in the 'Ann. der Chem. und Pharm.,' vol. lxxxv. p. 201.)

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[46] Arch. für Phys. Heilk., vol. xi. p. 493.

[47] Vergleichende Untersuchungen über das Gehirn des Menschen und der Wirbelthierre. Mannheim, 1854.

2. Von Bibra:[48] On the Spinal Cord and the Nerves.
3. Schlossberger's Memoir on the Nervous and Muscular Tissues (not yet completed) in his 'Erster Versuch,' &c.
4. Hauff and Walther's[49] Comparative Investigation of the Quantities of Water and Fat in the Brain (conducted under Schlossberger's superintendence).
5. Schlossberger:[50] On the Brain of the Infant.
6. Schlossberger's[51] Farther Communications on the Chemical Characters of the Cerebral Substance.

Von Bibra's first memoir is divided into nine sections, which treat respectively of—

I. The relative proportions of water, fat, and solid constituents in the brain of man and animals.
II. The fats of the brain.
III. The water-extract of the brain.
IV. The inorganic constituents of the brain.
V. The amount of phosphorus in the brain.
VI. The grey and white substance of the brain.
VII. The brain in insane patients.
VIII. The brain in the embryo and in extremely young animals.
IX. The weight of the brain as compared to that of the body.

I. From a very large number of analyses (he determined the amount of fats, water, and solid constituents in more than 100 cases in the human brain, in 138 other mammals, in 75 birds, and in 13 amphibians and fishes) he draws the following conclusions regarding the *first head.*

1. Within certain limits the quantity of fat is constant in the brain of man, as also in that of other animals.

2. Diseases of the general system, and even such as induce a diminution or disappearance of the fat in other parts, do not occasion a diminution in the amount of the brain-fat.

3. Fattening an animal appears to exert no special influence on the amount of fat in the brain.

4. The brain in other mammals contains less fat than the human brain. Where the opposite is the case, it appears to be induced by the ratio of the weight of the brain to that of the body—that is to say, the smaller quantity of cerebral substance is compensated for by a larger quantity of fat.

5. The brain in birds contains less fat than the brain in mammals.

6. The brain in amphibians and fishes contains a trace less fat than that of birds.

7. In man, other mammals, and birds, the medulla oblongata contains the largest amount of fat.

8. The quantity of fat in the hemispheres is both relatively and absolutely greater in man than in the mammals, and in the latter than in birds.

9. The whole quantity of brain-fat in old men is a little less than that in adults in the prime of life.

10. The water and solid constituents (the fat not being included) fall and rise in their amount in all classes of animals with the augmentation or diminution of the fat, the albuminous matters being liable to the greatest variations.

11. It is not definitely established that the brain in mammals contains a larger mean quantity of water than the human brain; it would appear as if in this class of animals the smaller quantity of fat is compensated for by the albuminous substance rather than by water.

12. In birds, on the contrary, the amount of water in the brain is unquestionably larger than in man or other mammals.

[48] Ann. der Chem. und Pharm., vol. xci. p. 1.

[49] Ibid., vol. lxxxv. p. 42.

[50] Ibid., vol. lxxxvi. p. 119.

[51] Ibid., vol. xc. p. 381.

Von Bibra believes that the analyses, from which the preceding conclusions are drawn, establish beyond all question the importance of the fat in relation to the functions of the brain.

II. The brain-fats seem to have been submitted by him to a very careful investigation. The following are his chief conclusions. The brain-fats consist of cerebric acid and cholesterin, and of a series of fatty acids which possess very different properties and very diverse fusing points. These fatty acids are not the same in different brains even of one and the same species; and it would seem probable that in the living organism they are undergoing perpetual decomposition, passing into one another, and taking a share in the cerebral functions. They contain no nitrogen or sulphur, and those which solidify below —12·5° C., contain no phosphorus. His cerebric acid agrees very well with the acid described by Frémy: von Bibra however finds as a mean of five analyses only 0·52% of phosphorus (the extremes being 0·49 and 0·55%), whereas Frémy fixed this constituent at 0·9%. He found that in adult men the brain-fat contains 20 or 21% of cerebric acid, and from 30 to 33% of cholesterin, while the remainder is made up of the above-noticed fatty acids and their salts. The cerebric acid is rather more abundant in the brain of man than in that of the other larger mammals. The grey substance of the brain contains the least cerebric acid, a mean quantity of cholesterin, and an excess of the other fats. The white substance contains more cerebric acid and cholesterin than the grey, and consequently less of the other fats. Although all these constituents of the brain-fat are found in the smaller mammals as well as in birds, amphibians, and fishes, and likewise in young infants and in the embryo, yet the quantity of cerebric acid seems to diminish as we descend the animal scale, and to be smaller in the infant and fœtus than in the adult.

III. His examination of the water-extract was not very satisfactory. He found,

1. That the water-extract of the brain, both of man and other mammals, was entirely devoid of all those crystallizable bodies which have as yet been found in other parts of the organism.
2. That lactic acid was certainly present, and probably also another non-volatile acid, in addition to volatile acids.
3. That, besides albumen coagulable by heat, there were present various modifications of albuminous substances which were not precipitated from their solutions by boiling; and that at least two nitrogenous substances were present, one of which was soluble in water alone, the other in water and alcohol.

IV. From a large number of analyses of the mineral constituents of the brain he deduces the following conclusions:

1. The inorganic constituents of the cerebral substance are the same as we meet with in other organs and in the formative fluids.
2. This qualitative condition holds good in all the classes of the vertebrata.
3. The ratio of the potash to the soda is nearly intermediate between the ratios occurring in the ashes of flesh and blood respectively.
4. Sulphates are almost entirely absent, and the quantity of the chlorine is very variable.
5. In man and other mammals the medulla oblongata contains more earthy phosphates than the other parts of the brain.
6. The amount of inorganic constituents is greater in the brain of birds than in that of man or other mammals.
7. The brains of amphibians and fishes contain more inorganic constituents than those of the other classes of animals.
8. The amount of earthy phosphates is moreover greater in the brains of amphibians and fishes than in the other classes of animals.

V. We quote the following determinations of the amount of phosphorus in 100 parts of human brain-fat.

Man aged 59 years. Bright's disease.

The medulla oblongata	1·65	The corpora striata	1·65
The cerebellum and pons Varolii	1·83	The optic thalami	1·54
The crura cerebri	1·76	The corpus callosum	1·54
The hemispheres	1·83	The mean for all the parts being	1·68

In a girl aged 19 years the mean quantity was 2·53; in a man aged 65 years, who died from marasmus senilis, 1·72; in a man aged 80 years, who died from old age, 1·93; and in a man aged 25 years, 1·89.

In three cases of insanity, the patients being men of the respective ages of 36, 38, and 52 years, the per-centage of phosphorus in the brain-fat was 1·75, 1·93, and 1·87.

Von Bibra draws the following conclusions from his numerous analyses:

1. The amount of phosphorus in the brain-fat is very nearly the same in man, in other mammals, and in birds. With the exception of a single case, that of the chamois, in which it amounted to 3·40, it never exceeded 3·0%, and it never sunk below 1·0%, except in Falco nisus, in which it was as low as 0 72%.
2. The phosphorus in the brain-fat of insane persons does not exceed the mean amount; nor does extreme old age modify the quantity.
3. The brain in very young persons, and in the embryo, presents no peculiarity in this respect.
4. The fat of the grey matter contains rather more phosphorus than that of the white substance of the brain.

Von Bibra believes that the phosphorus of the brain belongs to one of the brain-fats, and in part unquestionably to the cerebric acid, and that consequently its amount varies in different brains with the amount of fat: there is, however, no reason to believe that there is any special connexion between the intelligence and the amount of phosphorus.

VI. The grey and white matter of the human brain were separately analysed by von Bibra. We quote his analysis in the case of a man aged thirty years, who died from pulmonary phthisis:

	(*a.*) Grey substance of the hemispheres.	(*b.*) White substance of the corpus callosum.	(*c.*) White substance of the med. oblongata.
Fat	6·43	20·43	14·67
Water	83·57	69·19	71·55
Solid constituents (exclusive of fat).	10·00	10·38	13·78

This brain-fat was again analysed, and found to be composed as follows:—

	(*a.*)	(*b.*)	(*c.*)
Cerebric acid	2·64	20·72	24·70
Cholesterin	34·74	37·07	47·06
Other fats	62·62	42·21	28·24

Hence it follows that the grey substance contains less fat than the white, and that the fat is here replaced by water: and further, that the cerebric acid, and to a certain extent the cholesterin, preponderate in the white substance.

VII. In the analyses of various parts of the brain of three insane persons, he was unable to detect any striking chemical peculiarity.

VIII. We give his analyses of the brain of the human embryo at different stages, and of that of a child aged six months:—

	At 10 weeks.	At 12 weeks.	At 14 weeks.	At 18 weeks.	At 20 weeks.	At 21 weeks.	At 37 weeks.	Child.
Fat	1·26	0·99	1·53	1·06	1·07	1·23	3·06	6·99
Water	85·10	86·71	86·24	86·90	86·03	85·93	87·90	82 96
Solid constituents (exclusive of fat)	13·64	12·30	12·28	12·04	12·60	12·84	9 04	10·04

From these and similar observations on the lower animals (dogs, cats, pigs, horses, goats, and cows), it appears that the amount of fat in the brain of the fœtus is far less than in that of the adult individuals, the difference being made up by an excess of water. The great and sudden augmentation of fat towards the end of fœtal existence, and shortly after birth, is a fact of much physiological interest.

The last section of von Bibra's first memoir pertains rather to anatomy than chemistry, and therefore requires no notice in the present place.

Von Bibra's second memoir commences with the determination of the fat, other solid constituents, and water of the spinal cord. In connexion with the mammalia, there are recorded analyses of the cervical, dorsal, and lower portions of the cord of a man aged 44, of a woman aged 40, of two cats, of two dogs, of a fox, of a horse, of two pigs, of a roe-deer, of two sheep, of two oxen, of a hare, of a rabbit, and of a rat. (In the last case the whole cord was analysed, instead of three special portions.) There is likewise an analysis of the cord of a four months' human fœtus, and of a puppy a day old.

The following table gives the relative quantities of fat in the brain and spinal cord in different mammals:

	Brain.	Spinal cord.		Brain.	Spinal cord.
Man	14·44	25·45	Roe-deer	10·95	18·72
Cat	13·10	22·46	Sheep	14·09	20·96
Dog	15·07	24·33	Ox	16·50	25·60
Fox	12·87	22·41	Hare	10·81	22·55
Horse	16·42	25·00	Rabbit	8·85	21·15
Pig	15·50	24·45	Rat	9·71	18·03

The spinal cords of various birds were also analysed, and here, as in the case of mammals, the cord was found to contain a great excess of fat over the brain.

The fat of the spinal cord, like the brain-fat, seems to vary considerably in the spinal cords of animals of the same species; we take for example the male and female human cord:

	Man.	Woman.
Cerebric acid	23·7	30·6
Cholesterin	54·2	32·8
Mixture of fatty acids	22·1	37·6

The following table has reference to the per-centage of phosphorus in the fat of the spinal cord, as compared with that of the brain:

	Phosphorus in fat of cord.	Phosphorus in fat of brain.
Man	1·32 1·21	1·68, 2·53, 1·72, 1·93, 1·89
Horse	1·78	2·11
Ox	1·25, 1·37, 1·35	2·06
Pig	1·35	
Goose	1·80	2·17

We give his two analyses of the inorganic constituents of the human spinal cord:

100 parts of fresh tissue gave . 8·25 ... 8·36 of dry residue, free from fat.
100 parts of fresh tissue gave . 0·35 ... 0·36 of ash.
100 parts of dry residue, free
from fat, gave . . . . . 4·18 ... 4·30 of ash.

In 100 parts of the ash there were contained—

Phosphate of potash	39·25	52·77
„ soda	24·82	22·30
„ lime	21·00	16·10
„ magnesia	13·67	7·00
Iron	1·26	1·83

The ash was similarly analysed in the case of the ox, the pig, and the horse; and in all these cases the phosphate of potash was found to be the preponderating constituent. Traces of chlorides were always found in the ash, but seldom in appreciable quantities. No sulphates could be detected. (It is probable that the larger quantities of chlorides which were sometimes found in the brain-ash were due to the tissue being not quite freed from blood.)

From these researches of von Bibra it follows that the qualitative composition of the spinal cord is the same as that of the brain; with regard to its quantitative composition, it contains more fat, but less albuminous matters and less water than the brain: the fats of the brain and spinal cord are, however, qualitatively the same, but the quantity of cholesterin in the fat of the spinal cord considerably exceeds that in the brain-fat. Phosphorus occurs less abundantly in the former than in the latter, owing, probably, to the excess of cholesterin in the former. Finally, the salts in the solid residue of the cord, after the abstraction of the fat, are much the same as those of the brain.

Schlossberger's treatise being still unfinished, and von Bibra having gone over much the same ground as Hauff and Walthér, we proceed to the two memoirs standing last on our list.

Schlossberger's analysis of the different parts of the brain of a still-born child give the following results. (We quote the mean numbers deduced in each case from several analyses, and, by way of contrast, we append the corresponding mean numbers obtained by Hauff and Walther for the brain of the adult.)

	Grey substance of the hemispheres.	**Corpus callosum.**	**Corpus striatum.**	**Thalamus opticus.**
Water	88·9	89·6	88·2	87·4
Fat	3·7	3·8	4·5	4·4
		In the adult:		
Water	86·2	70·6	80·1	78·3
Fat	4·3	15·4	10·0	8·2

Hence we see that the infant brain differs from that of the adult in the corpus callosum being as rich in water as the grey substance, and in the quantities of water and of fat being very nearly the same in different parts of the organ.

In his last memoir, Schlossberger gives the result of his analyses of the healthy brain of an aged person (a woman, aged 74). There were found:

(*a*) In the *cortical or grey substance* of the cerebrum:

In the right hemisphere—

Water, 87·55. Ether-extract (fat), 4·01.
„ 87·68. „ 3·99.

In the left hemisphere—

Water, 88·26. Ether-extract (fat), 4·21.
„ 88·57. „ 3·76.

(*b*) In the *corpus callosum*:

Water, 73·50. Ether-extract (fat), 12·71.
„ 74·33. „ 12·21.

The main result of this investigation is, that there is a diminution of fat in the brain of the aged—a conclusion at which von Bibra had independently arrived a short time previously. There seems, also, to be a slight augmentation of water. Hence the brain in old age seems to approximate in its chemical characters to the brain in infancy. On what the decrease of fat in the brain of the aged depends—whether these fats are less copiously formed in old age, or whether they are absorbed from the textural elements (the primitive tubes) of the brain, or whether they are in part decomposed—is a question that will probably never be satisfactorily answered.

Schlossberger's analyses of the brain-ash are less elaborate than those of von Bibra. The following are his results :—

1. The observations were made by Lassaigne on a *morbid* brain, that the ash of the grey or cortical substance has an *alkaline*, and the ash of the white substance (the corpus callosum) an *acid* reaction, holds good also in the *normal* brain of man and the higher animals. Moreover, the degree of alkalinity and of acidity is liable to variations; thus, for instance, the ash of the corpus callosum of a calf aged one month was very slightly acid, while the corresponding ash from a man aged seventy-four years exhibited a strong acidity.

2. While the grey substance burns tolerably readily into a pure whitish grey ash in oxygen gas, this process is hardly possible with the white substance, in consequence, doubtless, of the great preponderance of the phosphorus-compounds in the latter.

3. It is probable that the determination of the whole collective ash is not very certain, in consequence of the extraordinary heat required for the perfect combustion of the brain-tissue.

The *nerves* have been submitted to chemical examination by von Bibra.[52] After giving various histological and micro-chemical details, he records the following results of the analyses of portions of various nerves of the human subject. As these are the only analyses of the kind on record, we give them more fully than we should otherwise do.

Sciatic nerve of a child aged six months.

Fat	6·06
Water	64·55
Solid constituents, exclusive of fat	29·39

The fat consisted, for the most part, of margarin and olein, with a little cholesterin, and dubious traces of cerebric acid.

A woman aged thirty-six years (phthisis).

	Crural nerve.	Brachial nerve.	Sciatic nerve (upper part).	Ditto (lower part).
Fat	38·72	30·20	26·37	24·25
Water	44·99	50·27	59·00	58·42
Solid constituents, exclusive of fat	16·29	19·23	14·63	17·33

The fat of the sciatic nerve contained much cholesterin, 8% of cerebric acid, and an olein-like fat.

100 parts of fresh tissue yielded	0·76 of ash.
100 parts of dried tissue, freed from fat, yielded	4·22 of ash.

And in 100 parts of ash there were contained:

Phosphate of potash	23·63	68·17 soluble salts.
Phosphate of soda	17·73	
Chloride of sodium	26·81	
Phosphate of lime	18·08	31·83 insoluble salts.
Phosphate of magnesia	12·27	
Iron	1·48	
	100	

The following analysis is especially interesting. The case is that of a woman, aged seventy-eight years, who had been paralysed for two years and a half on the left side. The upper analyses refer to the paralysed, the lower to the healthy side.

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[52] Ann. der Chem. und Pharm., vol. xci. p. 19.

	Crural nerve.	Brachial nerve.	Sciatic nerve (upper part).	Ditto. (lower part).
Fat	70·80	17·55	50·56	43·00
Water	15·00	59·04	32·40	39·30
Solid constituents, exclusive of fat	14·20	23·41	17·04	17·70
Fat	—	—	44·77	43·51
Water	—	—	39·16	41·51
Solid constituents, exclusive of fat	—	—	16·07	14·98

Perhaps the most remarkable point in the analyses of the nerves given in this memoir, is in the enormous differences in their amount of fat. Rejecting the case of paralysis altogether, in which 70·8% of fat was found in the crural nerve, we have a maximum of 38·72% in the crural nerve of the woman aged thirty-six years, and a minimum of 3·9% in the brachial nerve of a man aged eighty-seven years. In all the analyses of human nerves (but not in the case of the horse) the crural contained more fat than any other nerve. The largest quantity of cerebric acid occurred in the optic nerve, which contained 28·57% of that substance, much cholesterin and the brownish-coloured solid fatty acids occurring in the brain. The amount of phosphorus in the different nerves is mainly dependent on the quantity of cerebric acid which they contain.

From researches which we have not space to notice, it appears that the nerves of the mammalia contain less fat than those of man.

We shall conclude the second great division of this Report by a notice of

EXUDATIONS AND MORBID STRUCTURES.

We begin with the *pus*. The most important contributions to our knowledge of this fluid are:

1. PETREQUIN:[53] New Researches and Experiments on the Composition of Pus.
2. BRACONNOT:[54] On the Blue Colour which Pus sometimes Assumes.
3. HIFFELSHEIM and VERDEIL:[55] On Blue Pus.
4. GIBB:[56] On Sugar in Pus.

The titles of these papers, for the most part, afford a sufficient indication of the nature of their contents. Lehmann[57] records that a student of his has recently found the most essential biliary constituents (glyco-cholate and tauno-cholate of soda) in the pus from a large abscess in the leg of a jaundiced person.

Kletzinsky[58] has published a long memoir "On the *Contents of Ovarian Cysts*," and has likewise given a notice[59] "Of the *Contents of an Hepatic Cyst containing Echinococci*;" and Dr. Douglas Maclagan[60] has analysed the fluid from a *cyst in the thyroid body*.

Various memoirs on other allied subjects by Kletzinsky and Landerer may be found in Heller's 'Archiv' for 1852 and 1853.

Amongst the memoirs of this nature, we must not omit to mention those of Hoppe,[61] "On the Comparative Investigation of Transudations and the Serum of the Blood;" and of Grohé[62] "On the Pathological Exudations occurring in the Cavities of the Pleura and Pericardium." They are each deserving of a careful study, and as the Journal in which Hoppe's memoir originally appeared does not circulate widely in this country, we may mention that a full report of it is given by Scherer in his "Report on the Progress of Pathological Chemistry in the year 1853" (Canstatt's 'Jahresbericht'). Grohé's memoir is devoted for the most part to the consideration of the conditions under which urea and sugar appear in exudations occurring in the cavities of the pleura and pericardium.

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[53] Gaz. Med. de Strasbourgh, No. 6, 1852, quoted in Canstatt's Jahresbericht.

[54] Journ. de Chim. Med., p. 454. 1852.

[55] Comptes Rendus de la Soc. Biol. 1853.

[56] Gaz. Méd. de Paris, p. 659. 1852.

[57] Physiological Chemistry, vol. iii. p. 159.

[58] Heller's Archiv für Chem. und Mikrosk., pp. 173, 183, 266, 294. 1853.

[59] Ibid., p. 221.

[60] Monthly Journal of Medical Science, third series, vol. vii. p. 523.

[61] Deutsche Klinik, p. 404. 1853.

[62] Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. iv. p. 147.

We have already extended this Report so far beyond the limits originally contemplated, that we can only afford a very brief space for the last great division of the subject.

THE THEORY OF ZOO-CHEMICAL PROCESSES.

The general subject of the *metamorphosis of the animal tissues* is admirably treated in the third volume of Lehmann's 'Physiological Chemistry.' The works of Bidder and Schmidt, and of Bischoff, to which we have so frequently alluded in the preceding pages, and which have already been reviewed in this Journal, may also be consulted with advantage; nor should we omit to mention a memoir by Ranke[63] on this subject.

Amongst the memoirs on special agents as affecting the metamorphosis of tissue, we may refer to:

1. BÖCKER's[64] Investigations regarding the Action of Water.
2. L. LEHMANN:[65] On the Action of Sitz-baths of the Temperature of from 50° to 59° F.
3. L. LEHMANN:[66] On the Action of Sitz-baths of the Temperature of from 66° to 112° F.
4. SCHEFFER:[67] On the Nutrition of Animals deprived of Water.
5. FALCK and SCHEFFER:[68] On the Metamorphosis of Matter during the Deprivation of Water.
6. BÖCKER's[69] Investigations regarding the Action of Tea on the Human Economy.
7. J. LEHMANN:[70] On Coffee as a Drink in a Chemico-Physiological point of View.
8. BÖCKER:[71] On the Action of Beer on the Human Economy.

As it would be impossible to do justice to these memoirs, some of which are very long and elaborate, in a Report like the present, we shall shortly bring their contents before the readers of this Review in a separate article.

The only contribution of any importance to the subject of *Respiration* is Baumert's[72] "Chemical Investigations regarding the Respiration of the Pond-Loach" (*cobitis fossilis*). This fish, as has been long known to naturalists, possesses both a branchial and an intestinal respiratory apparatus. Baumert has compared the respiratory products with those of the tench and gold-fish, which breathe only in the former manner. The results of the inquiry may be seen in Lehmann's 'Physiological Chemistry,' vol. iii. p. 373.

There are a few memoirs on *Digestion* and *Nutrition* which we ought to notice here, but we must postpone the consideration of them to our next Report. The most important papers on the former of these subjects have been already referred to in our remarks on the Digestive Fluids.

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[63] Ranke, Ueber den thierischen Stoffumsatz: Journ. für Pr. Chem., vol. lxxxvi. p. 1.

[64] Verhandl. der kaiserlichen Leopoldinisch-Carolinischen Akademie der Naturforscher, vol. xvi. part 1, pp. 309—408.

[65] Archiv des Vereins für gemeinschaftliche Arbeiten, vol. i. p. 521.

[66] Ibid., vol. ii. p. 1.

[67] De animalium, aqua iis adempta, nutritione. 1852.

[68] Archiv für Phys. Heilk., vol. xiii. p. 1. 1854.

[69] Archiv des Vereins für gemeinschaftliche Arbeiten, vol. i. p. 218.

[70] Ann. der Chem. und Pharm., vol. lxxxvii.

[71] Archiv des Vereins für gemeinschaftliche Arbeiten, vol. i. p. 544.

[72] Ann. der Chem. und Pharm., vol. lxxxviii. p. 1.

REPORT ON MATERIA MEDICA.

By Edward Ballard, M.D.,
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I. *On the Mineral Waters of Mont Dore: Presence of Arsenic.* By Prof. Thénard. (Comptes Rendus, 5 June, 1854.)

In a memoir read before the Academy of Medicine of Paris in 1848, MM. Chevalier* and Gobley announced the presence of arsenic in the mineral waters of Mont Dore, Rayat, Hauterive, Provins, Vichy, Saint Mart, Bains, Plombières, and Bourbonne, but did not estimate its quantity, or determine the form in which it existed in them. M. Thénard, ignorant of these observations, re-discovered it in the waters of Mont Dore by evaporation, and application of Marsh's test; and believes he has determined that it is there in combination with soda as an arseniate, and not as an arsenite. He determined the amount of the mineral in the several springs.

It is the water of the *Madeleine* that is used for drinking. In a litre he found 0·53 milligrammes of arsenic = 0·812 milligrammes of arsenic acid =1·253 milligrammes of arseniate of soda.

The waters of *Saint Nectaire* are situated about twenty kilometres from Mont Dore, and are derived from various springs. In that of *Mont Cornador* he found 0·57 milligrammes of arsenic in the litre = 0·873 milligrammes of arsenic acid = 1·346 milligrammes of arseniate of soda. In the *Gros-Bouillon*, 0·61 milligrammes in the litre of arsenic = 0·934 milligrammes of arsenic acid = 1·441 milligrammes of arseniate of soda. In the *Boëte* spring, 0·82 milligrammes in the litre of arsenic = 1·256 milligrammes of arsenic acid = 1·935 milligrammes of arseniate of soda.

The waters of La Bourboule, situated about four kilometres from Mont Dore, upon the Dordogne, are derived from several small springs. The one employed for baths was alone examined, and was found to contain an amazing quantity of arsenic. From a single litre, reduced to some centilitres, 8·5 milligrammes of arsenic were obtained = 13·02 milligrammes of arsenic acid = 20·09 milligrammes of arseniate of soda.

These quantities of arsenic in the water drunk, employed in baths and in vapour (which Thénard found also to contain the mineral), cannot fail to be operative.

II. *Chemical History of the Mineral and Thermal Waters of Vichy, Cusset, Vaisse, Haut-rive, and Saint Yorre.* By M. Bouquet. (Comptes Rendus, 14 Aug., 1854, p. 326.)

The following are the most important points referred to by the author:

The mineral waters which emerge from natural sources at Vichy, as well as others around the town, have all the same origin, and the chemical differences of composition they present depend on the loss or gain of certain principles during their sojourn in or passage through the tertiary strata in their ascent.

The gases which they spontaneously give off are neither oxygen nor nitrogen: in the greater number they consist entirely of carbonic acid; in some instances, traces of sulphuretted hydrogen are evolved.

The Vichy waters contain carbonic, sulphuric, phosphoric, arsenic, boracic, hydrochloric, and (in some special instances) hydrosulphuric acids. They contain, also, silica, protoxide of iron, protoxide of manganese, lime, strontia, magnesia, potash, soda, and a bituminous organic substance. M. Bouquet has not discovered in them fluorine, iodine, bromine, lithia, or alumina.

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* See also an historical notice, by M. Chevalier, of the discovery of arsenic in mineral waters, with a table of the arseniuretted waters of France. He attributes the first positive announcement of the presence of arsenic to M. Tripier, in 1839, though Robert Boyle suggested the possibility of the occurrence in 1685. (Bulletin de l'Acad. Imp. de Méd., Jan. & Feb. 1855.)

The quantities of some of these principles, such as soda and the sulphuric and hydrochloric acids, identical in several instances, are always nearly the same; those of other principles vary. The variations in the carbonic acid appear to be proportional to the temperature of the waters; those of other principles are doubtless accidental. The proportion of potash they contain is large, and in several of them about 0·200 grammes in the litre.

The quantity of arsenic is not to be neglected: it is as much as 0·001 gramme in the litre for the non-ferruginous waters, and 0·002 for the ferruginous.

The spontaneous solid depositions are divisible into three groups. The first, amorphous, or presenting the Arragonitic texture, consists of carbonates of lime, magnesia, strontia, manganese, etc.; the second, crystalline, is of the same general composition, but contains appreciable quantities of sesquioxide of iron and arsenic acid; and the third, pulverulent, ferruginous, and gives on analysis 5 to 8 per cent. of arsenic acid.

Some authors have sought to explain the energetic medicinal properties of the Vichy waters by the chemical reactions they can produce in the economy, and consequently, have almost exclusively referred their therapeutical action to the predominant salt—the bicarbonate of soda. The precision of this view is far from being demonstrated, for besides this salt, others—such as the arseniates—must necessarily take a part in the therapeutical operation of the water.

III. *On the Action of Sitz-baths of the Temperature of* 12°—7·7° *Reaumur.* By Dr. L. LEHMANN. (Vierordt's Archiv, Band i. Heft 4, p. 521; and Schmidt's Jahrbucher, 1854, No. 8, p. 164.)

THE chief points to which attention was directed in the very careful and interesting series of observations recorded at full in this communication, were the influence of the cold Sitz-bath upon the temperature of the part immersed, with the amount of heat abstracted from the body; its effect upon the pulse and respiration, and on the metamorphosis of tissue, as measured by the alterations in the weight of the body and the quantity of the urine, and of its several constituents.

Two modes were adopted to determine the loss of heat which the body underwent from immersion. The first consisted in thermometrical observations upon the part immersed, and the second upon the water of the bath. With the former object the thermometer, at 29° R., was applied prior to the bath, to the anterior part of the perineum for five minutes, and the temperature noted; and then, after the bath, and before being dried, the temperature was again taken in the same manner, and in the same situation, the difference being received as the loss of heat due to the cold water. In the second method, the temperature of the Sitz-bath, containing 45 lbs. Prussian, was taken before and after use by means of a thermometer at 10° R. For comparison with this, and to obviate error arising out of other sources of heat, a second similar quantity of water was placed near it in the bath room, and its temperature taken simultaneously with the former. The difference of the two observations on the unused water being assumed as the increase due to the warmth of the room, was therefore deducted from the total increase of temperature in the used bath, and the remainder was taken as the increase due to the heat imparted to the latter by the body. In making observations upon the pulse and respiration, it was necessary to separate the influence which would be exerted by the mere removal of the clothing in a cool room. The experimenter, therefore, after undressing and seating himself as for the bath, noted the number of the pulse and respirations, each during half a minute; and then sitting in the bath, which covered about a fifth of the entire body, counted them at the end of five, ten, and fourteen minutes.

In order to render the conditions of all the experiments as nearly similar as possible, the time selected for them was the six hours between six or seven A.M., and twelve or one P.M., during which time no food or drink was taken. At the commencement of each experiment, having undressed and passed all the urine contained in the bladder, he was accurately weighed. The bath lasted fifteen minutes, and was sometimes repeated once or twice within the six hours, the rest of the time being occupied in the laboratory or in out of door occupations. At the end of the six hours the urine was again all passed and preserved, and he was again accurately weighed. The quantity of urine and fæces passed were also weighed, and being deducted from the total loss of weight of the body, gave the quantity of the insensible perspiration. These observations were also made on eight separate occasions without the use of the bath, with a view to ascertain the normal loss of the body under the circumstances of the experiment. The difference of the two series of experiments was ascribed to the influence of the bath.

The results of all the observations, experiments, and analyses of the urine, are elaborately given in several interesting tables. The following may be given as a summary of the conclusions to be drawn from them:—

The loss of temperature of the part immersed was, on the average, 6·6° R. In one experiment, in which the water of the bath was flowing so that fresh water was constantly brought in contact with the body, the loss was 9·4° R. The average increase of temperature of the bath, after use, was 1·6° R., a loss on the part of the body which would demand for its restoration an expenditure of material by oxidation corresponding with 0·292 Loth (about 61½ grains) of carbon. From the commencement of the bath, the frequency of the circulation and the respiration were altered in an inverse proportion to each other, the pulse becoming less frequent, and the breathing remaining unaltered or being increased in frequency. Thus, before one of the baths, the pulse and respiration were 80 : 17, but at the end of ten minutes in the bath, 64 : 16; in the first instance, to one respiration the pulses were $4\frac{12}{17}$, and in the second 4. On another occasion, before the bath, the pulse was 79, and the respiration 11, while at the termination of the bath the pulse was 65, and the respiration 17; in the first instance, to each respiration the pulses were $7\frac{2}{11}$, in the second $3\frac{14}{17}$. This effect is most remarkable during the first five or ten minutes, but lessens subsequently. The sensation of cold gradually becomes less from the beginning to the end of the bath, and is regarded as the probable cause of the augmented respiration. In consequence of the increase of the respirations, and diminution of the rapidity of the circulation, determinate volumes of blood become more fully impregnated with oxygen than under ordinary circumstances; and this again gives rise to a more active metamorphosis of the organs, &c., through which the heat removed by the bath is restored. The observations upon the weight of the body, urine, &c., correspond with this view, inasmuch as they show clearly an increased consumption of material in the body. Thus, the loss of weight by the body while fasting and using the bath was 61 per cent. more than while fasting without bathing, and the quantity of urine passed was 70 per cent. greater under the former than under the latter circumstances. The quantity of mere water evacuated by the urine was increased about 71 per cent. by the bath, and the solid matter of the secretion was also augmented—the urea, uric acid, fixed salts, and chlorides. The sulphuric and phosphoric acids, the earthy phosphate, the volatile salts, and extractive matters, appear, however, not to be increased by the bath. The urea was increased, in the observer's own instance, about 29 per cent.; for whilst without the bath 0·121 gramme of urea was evacuated for each kilogramme of the weight of his body, 0·178 gramme was given out when the bath was used. The increase of insensible perspiration is calculated at 48 per cent. (but this evidently includes the loss by the pulmonary exhalation).

IV. *On the Adulteration of the Oxides of Zinc of Commerce.* By Mr. Redwood.
(Pharmaceutical Journal, January 1855, p. 301.)

Mr. Redwood has pointed out the several sophistications to which this important drug is subjected by erroneous methods of preparation, and the rarity with which the pure medicine, as furnished by the process of the 'Pharmacopœia Londinensis,' is met with; his inquiries, made within the last few weeks, having convinced him that the *true oxide of zinc* was rarely kept by pharmaceutical chemists, but substituted by the carbonate, basic sulphate, or basic chloride. He believed this due to a prejudice for an oxide entirely free from colour, whereas a perfectly white smooth powder gives presumptive evidence that the preparation is not oxide of zinc at all. The oxide of zinc, as prepared according to the 'Pharmacopœia Londinensis,' is of a *yellowish white* colour; sometimes nearly, but never quite white. Besides the true oxide, the so-called oxides met with in commerce are the following:—1. *Carbonate of Zinc.*—Some years ago nearly all the so-called oxide of zinc of commerce was nothing else than this salt, being prepared by a process similar to the first part of that given in the 'Pharmacopœia' for oxide of zinc, but the calcination which would have converted it into oxide of zinc being omitted. Carbonate of zinc is still sometimes sold for oxide of zinc, but not so frequently as formerly. 2. *Basic Sulphate of Zinc.*—This appears to constitute the greater part of the oxide of zinc of commerce at the present time. It is prepared by adding caustic ammonia to a solution of sulphate of zinc; but as oxide is soluble in free ammonia, it is necessary, in this process, to avoid adding excess of ammonia, and under these circumstances the oxide of zinc retains a considerable portion of sulphuric acid, besides water of hydration. 3. *Basic Chloride of Zinc.*—This is prepared in the same way as the basic sulphate, except that chloride of zinc is used in the process instead of sulphate. It contains chlorine and water in addition to oxide of zinc. Oxide of zinc, prepared by combustion, is not at the present time a commercial article among druggists.

V. *On the Preparation of Zinc Ointment.* By David Kemp.
(Pharm. Journal, Dec. 1854, p. 280.)

The softness and want of durability of zinc ointment, as prepared according to the London Pharmacopœia, lead Mr. Kemp to propose the following formula:—℞ Axungiæ opt., olei olivæ purif., ana ℥x; ceræ albæ, cetacei, oxidi zinci, ana ℥v. Melt in a water bath, strain into a warm mortar, and just before cooling add the zinc, and continue stirring very briskly till quite cold. If the temperature be too high when the zinc is added, the colour is impaired. He has never observed this ointment, which is of good consistence, to show any peculiar tendency to become rancid, or to require any unusual means to be employed for its preservation.

VI. *On the Substitution of Zinc for Lead in the Preparation of Diachylon Plaster.* By M. N. Guéneau de Mussy. (Bulletin Gén. de Thérap., 1854, p. 569.)

During a stay at the thermal springs of the Pyrénees, Dr. de Mussy was struck with the fact that where the baths were used by persons employing diachylon plaster, all those parts of the skin which had been in contact with it became covered with a thick layer of sulphuret of lead, which was very difficult of removal, and he was led to inquire how far it was prudent to maintain these saturnine compounds for a long time in contact with large absorbing ulcerated surfaces. He refers to an instance in which, on two different occasions, lead colic was induced by such an application. At the suggestion of this physician, M. Boileau endeavoured to form a plaster with a base of zinc instead of lead. A solution of white soap was brought in contact with a solution of sulphate of zinc, an abundant precipitate of oleo-margarate of zinc fell, which, being washed and dried, was combined with the various other substances which enter into the formation of diachylum; augmenting, however, the proportion of oil and wax in order to preserve a proper consistence to the plaster.

VII. *Observations on the Antimonial Powder of the last Dublin Pharmacopœia* (1850), *and on the Medical Effects of the Teroxide of Antimony.* By JONATHAN OSBORNE, M.D. (Dublin Quarterly Journal, Aug. 1854, p. 25.)

THE conclusions arrived at are the following:

1. That the antimonial powder of the present Dublin Pharmacopœia differs from that hitherto prepared, not only by containing the antimony exclusively in the state of teroxide, but by medicinal effects of which the older preparation is nearly, if not entirely, destitute.
2. That, as it has not been identified by a distinct name (which is to be regretted), the prescriber should, to avoid confusion, always distinguish it as the antimonial powder of the Dublin Pharmacopœia of 1850.
3. That the teroxide of antimony (Algaroth's powder), inasmuch as it contains all the active part of antimonial powder, may be safely substituted for it, the phosphate of lime not contributing to its virtues, and having been at first accidentally associated with it in consequence of the imperfect chemistry of the time when the original process was devised.
4. That the average maximum dose of the teroxide of antimony, as a diaphoretic for an adult, is three grains evening and night.
5. That the addition of acids renders it more emetic and more purgative.
6. That the occasionally violent effects ascribed to it by some of the older writers were most probably due to the presence of chloride of antimony, from want of care in the preparation, and that this may be most effectually excluded by precipitating it from tartar emetic by means of an alkaline solution.

VIII. *Modus Operandi of Arsenious Acid.* By BASIL SAVITSCH. (Schmidt's Jahrb., No. 10, 1854, p. 35.)

THE following are the chief conclusions of the author, as deducible from his inquiries:—1. Between the compounds of arsenic and phosphorus, as respects their operation, there exists no similarity. This conclusion is especially supported by the fact that the phosphoric and phosphorous acids, contrary to common opinion, possess no poisonous qualities whatever. 2. The degree of the poisonous operation of arsenious and arsenic acid depends upon the quantity of metallic arsenic which they contain. 3. The arsenious acid, and, as it would appear, the other active compounds of arsenic, are decomposed within our organism, and then probably is formed some one and the same compound capable of perilling life.

IX. *Note on some of the Medicinal Uses, as an Internal Remedy principally, of the Muriate of Ammonia.* By H. A. EBDEN, M.D. (Indian Annals of Med. Science, April, 1854, p. 552.)

THE author has written this paper under the conviction that the hydrochlorate of ammonia (sold in the native Indian bazaars under the name of "nausaada") is not sufficiently appreciated by English practitioners as an internal remedy, and states his conviction, after a full trial of its merits, that it is a very valuable and powerful remedy for the relief of neuralgic pain generally. The diseases in which he states that he has given it with success are facial neuralgia, tic-douloureux, nervous headache, toothache, clavus hystericus, sciatica, and neuralgic dysmenorrhœa. He usually prescribes it in doses of twenty-five to thirty-five grains in an ounce of mint water or camphor mixture every twenty minutes for three doses. The second dose is usually sufficient for the relief of the immediate pain; but he has observed that where it has been necessary to repeat and continue the doses, the patient has, in many instances, afterwards enjoyed a comparative immunity from the recurrence of pain; and he has, therefore, been led to continue the exhibition of the salt systematically, at six or eight hours' interval, for seven days. Externally applied, too, in strong solution and hot, under oiled silk, he states he has found it an excellent palliative to neuralgic pain. The other uses of the medicine he thus enumerates. In doses of five, ten, and fifteen grains, twice and thrice a day, in bitter infusions, it proves alterative, and stimulates the action of the liver in cases where the exhibition of mercurials is undesirable. Externally, in combination with nitre and sulphur in equal proportions, the powder being moistened with vinegar, he recommends it in ringworm and impetiginous skin affections; and it is thus, he believes, used by the natives of Lower Bengal. In very strong solution, applied hot, he recommends its use as a topical anodyne in various painful affections.

X. *On the Preparation of Atropine.* By Mr. Luxton.

(Pharmaceutical Journal, Jan. 1855, p. 299.)

A pound of the dry leaves of the belladonna are to be boiled in distilled water sufficient to cover them for two hours, and the decoction strained off through a coarse cloth into a precipitating jar. The leaves are again boiled in a second water, and the decoctions mixed, to which two drachms of strong sulphuric acid are now added; the vegetable albumen is precipitated, and the clear liquor is drawn off with a syphon into a filter. A clear sherry-coloured solution comes through, which is either decomposed by passing gaseous ammonia through it, or by suspending in it a lump of sesquicarbonate of ammonia. In either case the colour becomes changed to black, and crystals of atropine are slowly formed. At the expiration of a day or so, the supernatant liquor may be drawn off with a syphon, and the crystals thrown on a filter to dry. To discolorise them, about an ounce of spirit of ammonia may be poured upon the filter, which washes away most of the colouring matter, leaving the crystals moderately white. By this process about forty grains of the alkaloid have been generally obtained from a pound of leaves. The solution which Mr. Luxton recommends for external use is made by adding two grains of atropine to one minim of strong nitric acid (sp. gr. 1·5), and to this one drachm of distilled water.

XI. *On Veratrine.* By Dr. J. Leonides van Praag.

(Virchow's Archiv, Bd. vii. heft 2.)

A great part of this communication is occupied by an historical review of the experiments and clinical observations made with this alkaloid up to the present time. The veratrine employed by Dr. van Praag in his experiments had a bitter taste, and when a minute portion was laid upon the tongue occasioned a peculiar lasting scraping sensation in the throat. A similar quantity applied to the mucous membrane of the nose occasioned for a whole hour a continuous tickling in the nose, and sneezing. Even when applied to the integument it can give rise to a peculiar pricking sensation, with a feeling of cold in the part. On applying a dilute watery solution of acetate of veratrine, which produced no effect on the back of the hand, to the skin of his umbilical region, before going to bed, it occasioned almost immediately intolerable pain, as if he were being pierced with innumerable red-hot needles, which, after three or four minutes, became more tolerable, and finally disappeared, leaving behind it for some time a peculiar sensation of cold. No general symptoms were produced by the experiment. Dr. van Praag instituted a number of experiments upon dogs and rabbits, administering the alkaloid by the stomach, injecting it into the jugular vein, applying it to a wounded surface, and introducing it into the rectum. He also administered it to birds, frogs, and fishes, and, from the whole, he concludes that in many points its physiological operation coincides with that of delphinine.* He sums up thus:—

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* See No. xxvii. p. 237.

The respiration and circulation are lowered; the muscles lose their tone; the irritability of many of the nerves (especially of the peripheral cutaneous nerves) is considerably lowered. On the other hand, even by very small doses, vomiting and often even diarrhœa are induced; but more frequently diarrhœa only arises after larger doses. The secretion of urine is not remarkably affected. The salivary secretion is very considerably increased. The stage of irritation is exhibited in accelerated breathing and frequency of the pulse, tonic, and even clonic spasm of muscles, and exalted nervous irritability. The tetanic stiffening of the limbs, passing into a dancing movement of them, seems to be quite a peculiar symptom of veratrine-poisoning. Death appears to arise from paralysis of the spinal cord. The alkaloid acts most slowly when absorbed from a wound in the integument; more quickly when administered by the anus; more rapidly still when taken into the stomach; and by far most quickly when thrown immediately into the circulation. From a few observations on the human subject, Dr. van Praag concludes that its operation is similar to that on the lower animals, the pulse being early and remarkably reduced in frequency. To a female, aged thirty-four years, with rheumatic prosopalgia, $\frac{1}{20}$th grain was given four times in the day, and on the first day the pulse fell from 90 to 72. On the following day four doses of $\frac{1}{10}$th grain were given, and the pulse further sank to 64. The prosopalgia was not cured, but the pain ceased during the action of the veratrine. To a female aged twenty-nine years, with spasm of the stomach, veratrine was given twice a day, in doses increasing from $\frac{1}{20}$th to $\frac{1}{4}$th grain. The pulse, however, in this instance presented no considerable change in frequency. The pain was lessened at first, but after the third day the medicine had no further influence on it. To a female aged sixty-two years, with rheumatic lumbago, which nothing had relieved, a quarter of a grain was administered twice a day, and the pulse in one day fell about fifteen beats. The result upon the disease was only temporary. Dr. van Praag, arguing from its observed physiological operation, suggests that it might be serviceable in febrile diseases which are associated with augmented muscular tonicity, and, on account of its sedative effect on the pulse and respiration, in pneumonia, pleurisy, and inflammatory affections of the heart.

XII. *On Lemon Juice.* By Dr. Bence Jones. (Med. Times and Gaz., 21 Oct., 1854, p. 408.)

Experiments on fresh lemon juice, by Mr. Whytt, of the College of Chemistry, furnished the following results. It had specific gravity 1036·0, 1037·9, and 1038·4; and in two specimens each ounce of juice contained about 28·1 to 27·5 grains of pure anhydrous citric acid, and when burnt yielded only 1·64 grains of ash=3·60 grains of ash in 1000 grains of juice. The ash contained—

	In 1000 parts.		In 1 oz. of lemon-juice.
Potash	443·4	... = ...	·767
Soda	21·6	... = ...	·038
Lime	76·1	... = ...	·131
Magnesia	33·4	... = ...	·058
Sulphuric acid	124·7	... = ...	·215
Chlorine	12·3	... = ...	·022
Carbonic acid	196·6	... = ...	·339
Phosphoric acid	75·6	... = ...	·130
Phosphate of iron	10·6	... = ...	·018
Silica	5·7	... = ...	·010
	1000·0		1·728 grs.

An ounce of lemon juice, then, does not at the utmost contain two grains of citrate of potash. From experiments detailed, it was found that in a healthy person lemon juice in twelve ounce doses daily causes temporarily an excessive acidity of the urine, and gives a deposit of free uric acid in the urine. It acts then as a free acid on the urine, and does not at all resemble in its effects those produced by citrate of potash and other vegetable acid salts. When taken pure without water also, no strong or immediate diuretic action was observable, and the pulse presented nothing remarkable in force or frequency. Practically, then, it may be regarded as a solution of citric acid. In St. George's Hospital, Dr. Jones has found free uric acid in the urine twelve hours after it has been passed, in every case in which large doses of the lemon juice were given. Hence, in chronic rheumatism and gout the remedy should be given with care, lest red sand or uric acid calculus should be produced.

XIII. *A New Styptic Preparation.* (Bull. Gén de Thérap., 30 Oct., 1854, p. 386.)

The following formula is published by M. Hannon:—Benzoic acid, 1 part; sulphate of alumina and potash, 3 parts; ergotine of bonjean, 3 parts; water, 25 parts. Mix.

The whole is to be boiled for half an hour in a porcelain capsule, constantly stirring, and replacing the evaporated water by hot water. Evaporated with constant agitation to the consistence of an extract, it presents a chocolate-brown colour, strongly astringent taste, and an odour of ergotine. According to M. Hannon, it is the most energetic styptic at present known, whether applied externally to the seat of hæmorrhage, or administered internally. For internal use it is sufficient to prescribe the following pills:—Benzoic acid, 1 gramme; pulverised alum, 3 grammes; ergotine of bonjean, 3 grammes. Mix, and make 16 pills, one to be taken every two hours.

XIV. *On the Canchalagua.* (Bull. Gén. de Thérap., 30 Aug., 1854, p. 182.)

The plant which passes in America and Spain under this name is the *Chironia chilensis*, a gentianaceous plant belonging to a genus of which one species, the *C. centaureum*, has long been used and valued in this country. It is a native of Chili and Peru, where it is reputed to possess all the tonic and antiperiodic properties which amongst ourselves are ascribed to other plants of the same natural order. It does not appear that any properties are claimed for it which render it superior to those allied plants with whose use we are familiar.

XV. *On the Plaster of Conium.* (Bull. Gén. de Thérap., 15 June, 1854, p. 503.)

In the preparation of this plaster, and of others whose efficacy is believed to reside in the vegetable substance introduced, it is of the highest importance that the latter should exist in the plaster, in a form which shall favour the operation of its active principles. The formula proposed by M. Mouchon of Lyons, is the following:

Burgundy pitch	1000	grammes.
Resin	1000	„
Yellow wax	1000	„
Ammoniacum	750	„
Oil of hemlock	250	„
Hemlock in fine powder .	1000	„
Water	2000	„

Place the yellow wax and the oil of hemlock in a pan; form a pasty mass with the hemlock and the water, and add this last mixture to the former, previously liquefied; proceed to the evaporation of a great part of the water, and add the other constituents, previously purified by liquefaction, and straining through a cloth, a process which will remove about 125 grammes of impurities.

The plaster thus prepared is of a finer green than that which results from the process adopted by MM. Henry and Guibourt, the hemlock yielding its chlorophyll to the wax, oil, and resinous substances only with the intervention of a proper quantity of water. It is, moreover, evident that the presence of the water retained by the plaster favours the dissemination of the active principles, and improves the medicinal properties of the plaster. The object of M. Mouchon in employing the water is clear, since during the ebullition, the water dissolves the soluble principles of the hemlock, and leaves the extract interposed between the molecules of the plaster.

XVI. *Clinical Researches on the Employment of the Tincture of the Flowers of the Colchicum in Articular Rheumatism, Simple or Gouty, and in Neuralgia.* By Professor Forget. (Bull. Gén. de Thérap., 15 July, 1854, p. 1.)

Having been provided with some tincture of colchicum flowers, Prof. Forget administered it to eight patients suffering from the diseases mentioned, and puts forward his results in the following propositions, with that amount of reservation which the novelty of the subject and the small number of his observations demand.

1. The alcoholic tincture of colchicum flowers is a good remedy against acute articular rheumatism.
2. It is without any sensibly favourable operation in cases of chronic articular rheumatism, and in acute neuralgia.
3. Its physical, and probably its chemical, properties, its mode of administration, its physiological effects, and its therapeutical results, have considerable analogy with those of the tincture of the seeds of colchicum.
4. The efficacy of the tincture of colchicum flowers appears superior to that of the tincture of the seeds, in the treatment of acute articular rheumatism.
5. It should be administered in doses of ten to twenty drops and upwards three times a-day.
6. Although it may act without producing gastric derangement, I think that it is proper to increase the doses until several stools daily are produced; a point at which it is right to stop.

XVII. *On Two new Tænifuge Medicines imported from Abyssinia: the Saoria and the Tatzé.* (Bull. Gén. de Thérap., 15 July, 1854, p. 17, and 30 July, p. 71.)

The *Saoria* (*sauarja*) is the ripe and dried fruit of the *maesa* (*bacobotrys*) *picta* (Hachstetter). According to M. Schimper, it is found throughout Abyssinia, at the height of 7000 to 9000 feet, never below 6000 feet. The fruit is an ovoid drupe, covered over two-thirds of its extent by the calyx, and of a greenish-yellow colour. The seeds are turbinate, angular, flattened at the apex, and covered by a resinous substance in ellipsoid grains. The long diameter of the fruit is from three to four millimetres, the short diameter a little less; it is then about the size of pepper. The taste is at first somewhat aromatic, oily, and astringent, and leaves for some time afterwards a tolerably persistent acid sensation in the pharynx. M. Schimper states that the dried fruit is administered in powder, in doses of 32 to 44 grammes; that it purges, and kills and expels the worm entire, without affecting the health of the patient.

With a view to determine its action upon Europeans, M. Hepp collected observations upon its use from different medical men, and in the paper under analysis thirteen such observations are recorded. In two of these, however, no worm was suspected to be present; and in three, although suspected, no portion of it had been seen. In the remaining eight cases the parasite was expelled; but, as is commonly the case after other tænifuges, the head of the worm was not found. In Abyssinia, according to M. Schimper, the saoria passes for a tænicide, and one of the observations recorded appears corroborative of the fact. The other effects produced in the cases related were the following:—nausea, five times; vomiting, once; colic, five times, violent in one case; three to five alvine evacuations; and in three cases, general feeling of illness, with peculiar sensations explicable otherwise than by reference to the saoria were observed; and in several cases, with the exception of the purging, the symptoms referred to were wanting. The medicine exercises a special action upon the urine, imparting to it a violet colour.

The following is given as the mode of administration:—A moderate regimen the previous day; in the morning, fasting, thirty grammes of the powder of saoria suspended in a liquid. Should nausea occur, it may be allayed by some mild aromatic. Ordinarily in two or three hours liquid stools will occur, in which the tænia will be found dead. Should the bowels fail to act, castor oil may be administered in the course of the day. A mild regimen during the day; and on the morrow, if the stools have been scanty, some further evacuations may be obtained, with a view to drive out the remains of the worm not expelled the previous day. If the head of the worm is wanting, there is no objection to repeating the dose in four to eight days' time.

The following are the conclusions drawn:

1. The saoria is more sure than our indigenous tænifuges, though we cannot yet call its action constant. It would appear to be tænicide.
2. Its action is mild, seldom accompanied by disagreeable effects; and it is not difficult to swallow.
3. It may be fearlessly and readily administered to young children and to females, as well as to persons with a shattered constitution and weak digestive canal.
4. These different properties bespeak its superiority over our indigenous tænifuges.
5. It is preferable to kousso, on account of its milder and yet tænicide operation, and from the lower price at which it may probably be obtained, since it is much more extensively distributed than the kousso. Its more ready and longer preservation is equally an advantage over both this and the fern-root.
6. Time alone can pronounce whether its operation is radical or only palliative.

The *tatzé* is the fruit of the *Myrsina africana*, a native of Abyssinia, the Cape of Good Hope, the Azores, and Algeria. It is a more disagreeable remedy than the saoria; and in six cases in which it was administered, the patients did not complain of any colicky symptoms being induced, and its purgative operation is not constant. It imparts an inky tinge to the urine. It is said to be tænicide. It succeeded in expelling the tænia in each of the six cases in which it was given, and in one of these, several other active vermifuges had failed. The medium dose of the powder of tatzé is fifteen grammes, followed, if necessary, by a dose of castor oil.

XVIII. *On Emulsion of Gum-Resins, and of the Resins.* By M. CONSTANTIN. (Bull. Gén. de Thérap., 15 July, 1854, p. 34.)

DISSATISFIED with the imperfect suspension of the gum-resins and resins in draughts and lavements by the usual procedures, M. CONSTANTIN proposes the following method for adoption. He places the prescribed quantity of gum-resin in small fragments in a marble or porcelain mortar, and adds to it about four grammes of alcohol for every gramme of the gum-resin; he then sets fire to the alcohol, and triturates the whole with a porcelain pestle until the alcohol is completely consumed. The gum-resin then assumes the appearance of a soft extract, and by now adding to it gradually the potion or lavement into the composition of which it is to enter, a perfectly homogeneous emulsion is obtained, which does not separate when left at rest, and which exhibits the gum-resin in a state of extreme division, a result which it is difficult to obtain with yolk of egg, even at the end of a certain amount of trituration. When the quantity of gum-resin prescribed is very large, the addition of yolk of egg or gum arabic is, however, advisable. In manipulating with the resins, he adds to them what they want to constitute them gum-resins (powdered gum arabic answers this purpose), and then adding the alcohol, proceeds as with the gum-resins. The taste and odour of the medicines do not disappear after their preparation in this manner, showing that the loss of the volatile aromatic principles cannot be considerable.

XIX. *On the Constitution of Quinine.* By ADOLPHE STRECKER. (Ann. der Chemie und Pharm., Aug. 1854, p. 155.)

IN consequence of the doubt hanging over the precise formula of quinine, M. STRECKER undertook an analysis of the pure base, dried at 120°, by combustion with oxide of copper and oxygen gas. He adopts the constitution represented by the formula $C_{40}H_{24}N_2O_4$. The proportions per cent. were—

	Calculated.	Found.	
Carbon . . .	74·1 ...	74·0 ...	74·1.
Hydrogen . . .	7·4 ...	7·5 ...	7·5.
Nitrogen . . .	8·6.		
Oxygen . . .	9·9.		

Purified nitrate of quinine gave the formula, $C_{40}H_{24}N_2O_4$, HO,NO_5+2HO.
Sulphate of quinine (officinal), $C_{40}H_{24}N_2O_4$, HO,SO_3+2HO.
Acid sulphate of quinine, $C_{40}H_{24}N_2O_4$, $2(HO,SO_3)$.

XX. *Experiments with several pretended Substitutes for Cinchona in the Military Hospitals at Rome.* By DR. FELIX JACQUOT. (Archives Générales, June, 1854, p. 678.)

1. *Arsenic.*

THIS paper is a summary of a memoir addressed to the Conseil de Santé des Armées, on the employment of arsenic in the treatment of intermittent fevers in general, and of those of Rome in particular, and which was based upon 282 observations.

1. *Mode of Experimenting.*—In order to establish the efficacy of arsenic as a febrifuge, its administration should be limited to those cases which have resisted treatment without the use of quinine. The author of the paper before us has not strictly followed this course, since, giving the arsenic at the outset in the majority of the cases, he had no means of judging whether the fever was about to proceed steadily with its paroxysms, or whether, on the other hand, it had a tendency to spontaneous disappearance. But as the sulphate of quinine was administered in the same way, it was at least in a position to establish the comparative efficacy of the two medicines. His researches, too, permit him to consider separately the treatment with arsenic alone, and the complex treatment by this remedy, emetics, &c. Arsenic alone cut short the fever only in 8·33 per cent. of the cases, but the complex treatment in 16·66. But while the efficacy of the arsenic is doubled by the conjoined use of emetics, the febrifuge powers of the sulphate of quinine are so great, that those of emetics simultaneously employed are lost, or absorbed in them; thus, the percentage of fevers cut short by sulphate of quinine without emetics is 49·52, and by sulphate of quinine with emetics 50·47, as calculated on 210 fevers.

2. *Formulæ, Dose, Duration of Use of the Arsenic.*—The formulæ used was the following:—Arsenious acid, 1 gramme; distilled water, 1 kilogramme. The arsenic is boiled with more than this quantity of water till dissolved, and the latter reduced to the prescribed quantity, some soda being added should the solution be

imperfect. The dose of solution was administered in canella-wine. The author could derive nothing but confused ideas of the proper dose from writers on the subject, nor yet of the rapidity of its action.

3. *General Accidents, Tolerance.*—Most subjects bear without general accidents, three centigrammes at the outset, though the author has seen one centigramme produce serious local and general accidents; yet, on the other hand, the tolerance has persisted sometimes in spite of long-continued large doses. Out of 72 cases treated by arsenic, he has only noted general accidents six times, never fatal, and only once a source of anxiety. The local and general tolerances are quite independent of each other. The author considers the action of arsenic to be sedative, hyposthenic. In one of his subjects the pulse fell to fifty. General loss of strength, lassitude particularly affecting the legs and loins, have appeared to him the earliest phenomena of poisoning by moderate doses of arsenic; and while he thus differs from those who class it among the tonics, he asserts that it has no tonic operation, even upon subjects suffering under marsh cachexia.

4. *Local Accidents, Tolerance.*—Out of the 72 cases treated by arsenic, 24 or 25 presented gastro-intestinal accidents. The first dose of one centigramme may cause vomiting and epigastric pain; but, on the other hand, he has seen six centigrammes given by the mouth tolerated; and in others he has seen the arsenic continued for a month without the stomach revolting against it. Although the conditions favourable to the tolerance are not well known, yet he can mention the smallness of the dose, its ingestion in divided portions, and the quantity, and perhaps nature, of the vehicle. The local accidents are nausea, vomiting, diarrhœa, malaise, and sometimes pinchings at the epigastrium, and an insurmountable disgust at the medicine. Either general or local accidents followed in 31 out of his 72 cases, or in 43 per cent.

5. *Autopsies of Individuals treated with Arsenic.*—In three subjects examined, nothing was discovered which could be imputed to the employment of the arsenic, either in the heart, or in any other part of the body.

6. *Degree of Efficacy of the Arsenic, Comparison with Sulphate of Quinine, &c.*—The cases on which M. Jacquot founds his comparison, are those which had not received any previous treatment calculated to interfere with the accuracy of his experiments. He thus tabulates his results:

	Sulphate of quinine.	Arsenic.
Fevers cut short—i.e., which have not presented a single paroxysm from the commencement of the medication	50·00	13·88
Fevers which had presented one paroxysm in spite of the medication, but in which the second has been averted	25·71	22·22
Fevers which have presented two paroxysms, but in which the third has been averted	7·61	12·50
Fevers which have presented three or more paroxysms	5·23	34·72
Fevers which cannot be introduced into these categories, but which must be regarded as not cut short	11·42	16·66

The arsenic with or without the emetic has cut short the fever 13·88 times per cent.; the sulphate of quinine, with or without emetics, 50· times per cent.; that is to say, the arsenic has been efficacious as 1, the sulphate of quinine 3 times and a fraction. The arsenic with emetics cut short the fever 16·66 times per cent.; the sulphate of quinine without an emetic, 50·47 times per cent.; that is to say, the arsenic has been efficacious as 1, the sulphate of quinine as 3 and a fraction. The arsenic without emetic has cut short the fever 8·33 times per cent.; the sulphate of quinine without emetic, 49·52 times per cent.; the proportion being arsenic as 1, to sulphate of quinine as 5 and a fraction. Lastly, in comparing the cases the most favourable to the arsenic—viz., those in which it was administered in large doses, three to ten centigrammes, accompanied by emetics, and a diet whose only limit was the appetite of the patient, with the cases least favourable to the quinine, we arrive at the following results:

Fevers cut short by the arsenic, 9·68 per cent.
" " " quinine, 49·52 "

As respects the cases not cut short, it will be perceived, on referring to the first four figures of the two vertical columns of the table, that in the instance of the fever treated with quinine, the numbers are smaller and smaller according as we examine the categories of cases more and more refractory, whilst the contrary is noticed in the instance of the arsenical treatment. The contrast is perfect.

In about 35 cases it was possible to compare the effects of the quinine and arsenic, the two medicines having been administered in succession to the same patient, either for the same fever, or in two separate attacks. In a sixth of the cases, the arsenical and quinine treatment were of little efficacy; in another sixth, the two medications were followed by some success; in the four other sixths, the sulphate of quinine showed itself the more active, or the only active remedy of the two; and one observation furnished a very marked instance of fever resisting the sulphate of quinine, and cured by arsenic. In short, the author concludes that we see more fevers which resist arsenic yielding to quinine, than we do fevers refractory to quinine disappearing under arsenical treatment. He believes, also, that he has established the fact of the greater activity of the sulphate of quinine in the cases which have received no previous arsenical treatment (54 per cent. cut short), than in those first submitted to the action of arsenic (40 per cent. only cut short).

The general conclusion he draws is, that sulphate of quinine is not replaceable by arsenic; and especially is this true in respect to the fevers of hot climates, where it is necessary to apportion the dose to the intensity of the malady; under the latter circumstances we are immediately arrested in the arsenical treatment by the fear of poisoning. In those countries where, from one paroxysm to another, the pyrexia may become more severe, remittent, and pernicious, arsenic should not be employed during the endemo-epidemic season.

Confirmation of results by other observers.—After mentioning MM. Mayer, Cordier, Pasquier, Armand, and Gougé, as arriving at similar conclusions to his own, he states that in the Pontine marshes, Dr. Minzi, physician to the central hospital of that country, has experimented with arsenic in more than 400 cases, giving it to the extent of three centigrammes a day, and at last abandoning it from want of success. M. Salvagnoli Marchetti also, out of 16 cases, found 15 resisting arsenic.

Arsenic in Inveterate Fevers, and in Marsh Cachexia.—The observations of M. Jacquot do not encourage recourse to arsenic in inveterate fevers; and M. Cordier also concludes from his experience in Algeria, that it is the more recent and slighter cases which yield most readily to arsenic. In the palustral cachexia he thinks that arsenic may perhaps be used as an alterative, but that it is incapable of replacing iron and other tonics, which it is necessary to conjoin with it.

Relapses.—In preventing relapse, arsenic is inferior to sulphate of quinine. Out of the 72 cases treated with arsenic, the relapses were 22 or 30 per cent.—certainly a large proportion. They were less frequent in the cases treated with quinine. The relapses occurred even during the period of administration of the arsenic, which was continued after the cessation of the fever. This was not observed in the instances of the quinine treatment.

Arsenic in the Ingravescent and Remittent Fevers.—In 5 cases it was observed that, in spite of and during the employment of arsenic, the simple fever became aggravated, remittent, sub-continued, and pernicious—*à fortiori*, then, this medicine would have no action upon a fever already of this character.

Conclusions.—Arsenic is not for a moment to be regarded as a substitute for sulphate of quinine. It will probably find a limited place in the treatment of indigenous intermittent fevers, but it has absolutely no pretensions against the recent endemo-epidemic fevers of hot countries. We are scarcely authorized to employ it except in the fevers which resist all the preparations of bark. Uncertainty and contradiction reign over almost all points relative to arsenic. It is a medicine which we cannot yet handle with the double certainty of obtaining the effect desired, and of avoiding the dangers connected with its administration.

2. *Parsley Oil (Apiol), or Juice of Parsley Seed; Colophane treated by Nitric Acid.*

The author condemns the colophane almost absolutely. Of the efficacy of the parsley oil he expresses great doubt. The single case out of six trials in which the fever appeared cut short by it might have been an instance of spontaneous recovery, since 7 out of 19 cases submitted to expectation terminated in this way.

3. *Hydrochlorate of Ammonia.*

The doses employed were eight to twelve grammes in the day, and the experiments were made upon 21 subjects. The following table represents the results:

Fevers cut short	6, or 28 per cent.
Fevers which presented one paroxysm	1, or 4 per cent.
Fevers which presented two paroxysms	1, or 4 per cent.
Fevers which presented three or more paroxysms in spite of the medicine	11, or 52 per cent.
Fevers which cannot to be placed in the above categories, but which were not cut short	2, or 8 per cent.

The first number of 28 per cent. of fevers cut short would show a powerful febrifuge operation, were it not for the fact that more than a third of the cases submitted to expectation recover spontaneously. The remaining numbers, too, are little in favour of the efficacy of the medicine, since more than half were uninfluenced by it. The observations of M. Jacquot have not only established that the greater part of the fevers are completely refractory to this salt, but that the marsh cachexia becomes quickly developed, and assumes an accelerated course during its administration where the fever is not arrested, and also that the sulphate of quinine succeeds admirably in arresting fevers against which sal ammoniac is powerless. The conclusion of the author is, that this salt bears no therapeutical pretensions in the intermittents of hot countries, and that there is much doubt of its capability of rendering any service in those of our own climate.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

1. *Remarks on the Pathology and Treatment of Beri-beri.* By C. Morehead, M.D., Professor of Medicine, &c. (Transactions of Medical and Physical Society of Bombay, New Series, No. 2, p. 87.)

Dr. Morehead regards beri-beri as a general dropsy of a complicated character, for the most part appearing when the vessels are tolerably full of blood, and a blood abounding in watery constituent, and following exposure to external cold. He is of opinion that the term ought to be expunged from medical literature as likely to mislead by its indefiniteness; and he now seeks to remove some of the difficulties enveloping the subject by reducing it to known conditions.

He finds these conditions in the scorbutic diathesis. He believes that the disease, more "particularly in its acute form, will be found to present itself in individuals favourably circumstanced for the development of a scorbutic taint, and who, while in this diathesis, have been exposed to the sudden cooling of the surface of the body from sudden alternations of temperature or of wet." Regarding the disease as one of general dropsy, Dr. Morehead suggests that the principles to be followed in its treatment should be the same as those guiding the treatment of general dropsy. According to this view he explains the apparently conflicting statements made by various authors, of the advantage of the most opposite kind of treatment. Dr. Morehead admits the occurrence of cases justifying antiphlogistic treatment; but in the majority of instances diuretics, stimulants, and vapour baths are required. Four cases are brought forward, which confirm the views of Dr. Morehead both by their previous history, the course of the disease, and the post mortem appearances. The persons affected were sailors, who, during an unexpectedly extended voyage, had been restricted to a poor diet, and much exposed to inclemency of the weather; while all those who in the same ship had been supplied with antiscorbutic food, had escaped the disease. The symptoms were œdema of the lower extremities, causing a waddling gait, feeble pulse, scanty urine (not albuminous), uneasiness at epigastrium, moist tongue, gums discolored but not swollen. The post-mortem examination of two of the cases exhibited fluidity of the blood, and congestion of the liver; but no organic disease of any of the viscera. There was some serous effusion in the arachnoid, pleura, and peritoneum.

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2. *A Contribution to the Etiology of Elephantiasis-Arabum.* By Dr. RIGLER, Constantinople. (Zeits. der K. K. Gesell. der Aerzte zu Wien, Heft 1, 1855.)

DR. RIGLER's essay is intended to show that elephantiasis is not essentially a disease of tropical climates, but that it may occur sporadically all over the globe, provided certain circumstances affect the patient by which the functions of the vascular system, and especially of the lymphatics, are impaired. To an alteration of the functions of the latter he attributes the hypertrophy of the cellular tissue and of the skin which constitutes the chief feature of the disease. In four cases of elephantiasis of a lower extremity, the commencement of the affection, which he always observed on the left side only, was marked by induration and swelling of the inguinal glands, indicating an arrest of their normal function. The author compares the disease to phlegmasia alba dolens, and to sclerosis neonatorum, which Henle has attributed to an extensive insufficiency of the lymphatics. In two adult healthy Turks that came under Dr. Rigler's notice, the disease, affecting their left feet, followed the occurrence of plague buboes. A case of elephantiasis of the left arm was brought about by an axillary bubo of four months' standing; in another, it followed an attack of phlegmasia; and in the last instance which is quoted it was preceded by the plague. Phlebitis is the ordinary mode by which elephantiasis terminates. Dr. Rigler has found Luschka's observation confirmed, that the ichthyosis often accompanying elephantiasis only differs from the latter in presenting numerous coherent layers of epidermis agglutinated together by sebaceous secretions, in addition to the hypertrophy of the subjacent cutaneous tissues.

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3. *On Icterus Typhoides.* By Professor LEBERT. (Virchow's Archiv für Pathologische Anatomie, &c., Band vii. Heft 3 & 4.)

PROF. LEBERT analyses 72 cases of severe icterus, of which he has been able to obtain careful accounts in the writings of others, or in his own practice.

Of the 72 cases, 58 were fatal. The prominent points in the cadaveric examinations were:—1. The rapid putrescence of the body, and the generally diffused icteric hue pervading all the tissues. 2. The blood was of a reddish brown colour, ill-conditioned looking, liquid or semi-coagulated; in 28 cases, or one half, extravasations of blood were found in various parts of the body, especially in the sub-serous cellular tissue, but also in various mucous membranes. 3. The organ found to be most affected was the liver: in 29 cases it was diminished in size, an alteration upon which, from the uncertainty of the measurements, Lebert places little reliance; the organ generally presented icteric coloration, the texture was undefined, its consistency sometimes diminished, at others much augmented; the microscope generally exhibited icteric discoloration of the hepatic cells and oil molecules in them, or a still further disorganization of the cells. The liver was generally anæmic, and the gall-ducts empty. In one-third of the cases the gall-bladder was entirely or nearly void; in six there was a contraction of the cystic duct, which the author attributed to spasm. Several cases exhibited a slight catarrhal thickening of the larger gall-ducts; but in the majority of cases they were normal. 4. In one-third of the cases the spleen was manifestly diseased, in 14 cases it was very much enlarged, in 5 it was morbidly softened. 5. No definite conclusion was to be drawn from the appearances presented by the other abdominal and thoracic viscera. 6. On account of the frequency of marked cerebral symptoms in fatal icterus, special attention was paid to the state of the brain, but no uniform lesion has been traced. In many cases the consistency of the brain was diminished, and in 12 this softening was limited to the central parts, but this Professor Lebert attributes to the general tendency to early putrefaction

The main symptoms that obtrude themselves during the whole course of the disease are connected with the digestive organs—the taste foul and pasty; bitterness complained of only in one-quarter of the cases; the mouth dry after the appearance of cerebral and typhoid symptoms. The tongue generally remarkably dry, even when apparently clean, and almost always covered with a thick yellowish-white fur. The typhoid symptoms marked by a dry and brown tongue. Loss of appetite, thirst continuing more or less throughout, were general; nausea and vomiting occurred in little more than one-half of the cases. The vomited matter generally watery, rarely bilious. In 18 cases there was hæmatemesis; this occurred in one-third of the cases between the third and tenth day of the illness, in about one-half in the second half of the second week, and in three during the third week; in all cases coincidentally with the occurrence of cerebral symptoms. Pain and tenderness in the region of the stomach and liver occurred in two-thirds of the patients. Tympanitis occurred five times; severe hiccough in one quarter of the cases. The alvine discharges were examined carefully in 20 cases; in 7 they were coloured and contained biliary pigment; in 13 they were colourless, grey or white. In several of the worst cases there was severe diarrhœa towards the end, and no less than 15 patients passed their motions involuntarily; this is to be regarded as an immediate precursor of death. Disturbance of the nervous system was observed in 64 out of 72 cases; in 13 of these it took the form of typhoid exhaustion; in 51 of delirium, coma, and convulsions. Frontal headache was a common symptom at their commencement. A careful and interesting analysis of the period at which the nervous symptoms set in is given by Professor Lebert, into which we are unable to enter. With regard to the organs of circulation, the absence of fever during the entire illness was noted in only 5 cases. The general character of the fever was that of torpidity. Remarkable variations were noted in the rapidity of the pulse, especially during the severer stages, when it would vary in a short space of time from 60 to 120; on the return of health it was observed to become more uniform. No characteristic symptoms were remarked in the respiratory organs; there was occasional bronchitis with icteric sputa, twice slight hæmoptysis, and occasionally dyspnœa. In all the cases, but one, the urine contained biliary pigment; in 4, there was hæmaturia. Roseola and petechial eruptions were noted in 12 patients; in 7, violent epistaxis, menorrhagia in two. These hæmorrhages during life confirm the observations of the hæmorrhagic tendency made in the dead bodies. Seven cases were fatal within three days, 20 during the first week, 35 during the first fortnight; in the third week there were 22 deaths, and 8 between the twenty-second and thirty-first days. The great majority of the cases occurred between the ages of 15 and 25—viz., 32 out of 63 in which the age was noted. One-third of all the cases occurred in November and December. We omit the consideration of the causes, prognosis, and treatment, as offering nothing of special interest.

4. *On Epithelial and Villous Growths.* By Professor Bruch, of Bâle. (Vierordt's Archiv, Jahrgang xiv. Heft 1, 1855.)

The majority of pathologists are at present agreed that cancerous growths are primarily of local origin, and not the result of a cancerous dyscrasia; the proofs are also being multiplied that a transition from non-malignant to malignant growths may take place. Professor Bruch enters into an interesting disquisition on the nature of epithelial and villous growths, with a view to determining their pathological classification. He admits, with Lebert, Schuh, and others, the occurrence of new growths, consisting of cylindrical epithelium, which have hitherto been observed only in parts normally presenting that variety of epithelium, viz., stomach, rectum, collum uteri, male urethra, eye, and fourth ventricle of the brain. Professor Bruch regards epithelial growths as the result of a morbid development of the epithelial investment and epidermis, of the papillary body and of the cutis vera, which he considers to be non-malignant; and he establishes a subcutaneous variety of epithelial growth, which he regards as malignant. "An essential characteristic," he observes, "of epithelioma, papillary hypertrophy, and epithelial growths generally, is an intimate union and adhesion of the cells, which prevents their extrusion and causes their accumulation."

The villi of the chorion of mammalia are regarded by Professor Bruch as the prototype of all normal and abnormal villous or papillary formations; all being originally formed as simple elevations of the integuments of the surface, and especially of the basement membrane. After reviewing the various forms of villi occurring in the body, he concludes that the growth of papillary, villous, or fringed formations is an attribute of all the surfaces of the body, and that it is impossible to draw the exact line of demarcation between that which is normal and that which is abnormal Many warty excrescences, most condylomata, Clarke's cauliflower excrescence of the os uteri, the incipient condition of some villous cancers, are mere varieties of the same thing in different parts of the body. We cannot conclude this brief notice without adverting to an almost unique case observed by the author of the morbid formation of ciliary epithelium. It occurred in a woman aged forty, who died of phthisis, but had previously suffered from pain in the head, spasmodic action of the right arm, furred sensation of left hand, and burning of the soles of the feet. After death a rounded, well-defined tumour, of the size of a filbert, was found under the cerebellum, attached to the walls of the fourth ventricle. The microscope showed the tumour to be composed of villous prolongations, or pencils of vessels, closely aggregated in a dendritic or foliaceous arrangement. The surface was invested by a layer of cylindrical epithelium, many individual cells of which were recognised as ciliary epithelium. The form and size of the cells, the nature and seat of the nucleus, and the distinct, though naturally immovable, cilia, precluded all doubt as to their character.

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5. *On a Peculiar Black Discoloration of the Skin of the Face.* By J. Moore Neligan, M.D., M.R.I.A., &c. (Dublin Quarterly Journal of Med. Science, May, 1855.)

This case closely resembles the one related by Mr. Teevan in the Medico-Chirurgical Transactions, vol. xxviii.; like that one, it occurred in a young woman, whose catamenia were deranged, and affected especially the skin surrounding the eyes. The following is an abridgment of Dr. Neligan's account and the view he takes of it: Eliza D., aged twenty-one, a dressmaker, of leucophlegmatic habit, single, enjoyed good health until the disappearance of the catamenia two years ago. This was followed by an attack of erysipelas, recurring on the right side of the body at each monthly period. This continued for a year. About seven months ago, the erysipelatous affection was treated with tartar emetic in small doses, which induced vomiting at the time, and left a chronic irritability of the stomach, giving rise to daily emesis. In August, 1854, for the first time, some blood was observed in the vomited matter. In September she threw up about half a pint of reddish brown matter; the same quantity of a similar matter was vomited every morning for four or five days. In October, at the regular period, this black vomiting again returned, and now for the first time a dark bluish-black stain was manifested at the inner canthus of the left eye. When Dr. Neligan first saw the case in December dark stains covered nearly the entire upper eyelid of the right eye, and partly that of the left; the under eyelids of both eyes were completely stained, and on the right side the dark patch reached the skin of the cheek. The colour was precisely that of Indian ink; examined with a powerful lens, it was evident that the stain was unequal in depth, and was dotted over the surface of the skin; the dark dots corresponding to the orifices of the sebaceous glands; the skin was exquisitely tender. Dr. Neligan regards it as a case of stearrhœa nigricans, resulting not from a change of extravasation into the cellular tissue of the eyelids, but from an increased and morbid secretion of the sebaceous follicles of the region; a view which would seem to receive support from the case given by M. Bousquet at page 43 of the present number of the Review. The inference with regard to treatment, as Dr. Neligan observes, is that local remedies will be of no avail, and that a cure can only result from restoring the normal action of the sexual organs.

6. *On the Shape of the Stethoscope.* By Professor Fick. (Müller's Archiv für Anatomie, &c. Jahrgang, 1855, Heft 1, 2.)

Professor Fick, in order to avoid a fine humming noise which he finds that the application of the ordinary stethoscope to the ear invariably produces, recommends the employment of a tube without an ear-piece, which he introduces into the meatus. He has used a stethoscope of this construction for many years, and states that it presents the thoracic noises more distinctly, and possesses the advantage of allowing the head of the observer to be more extensively moved and more easily applied in various postures than the ordinary stethoscope. He explains the adventitious murmur by the compression the ordinary instrument exerts upon the tragus.

7. *On Chalk Metastases (Kalk Metastasen).* By Rudolph Virchow. (Virchow's Archiv für Pathologische Anatomie, &c. Band viii. Heft 1.)

Some years ago Professor Virchow performed the post mortem of a young lady whose case had thoroughly puzzled all the leading physicians of Berlin. She had complained of pains throughout the body, but these, which were attributed to rheumatism, had resisted all the means employed for its removal. After death numerous and large nodes of cancer were found in almost all the large bones, occupying cavities of a corresponding size in the osseous tissue, and nowhere rising above the level of the bone. A large quantity of white sandy deposit was found in the calyces and pelvis of the kidneys, and on dividing the lungs considerable deposits of a hard greyish white substance were found, and a similar deposit occupied the mucous membrane of the stomach. The salts which had been absorbed in consequence of the cancerous deposits in the bone appeared to have found a fresh nidus in the kidneys, lungs, and stomach. Professor Virchow has recently met with four similar cases; the following is a brief abstract of the characteristic features found on cadaveric inspection. It may be premised that in all four there was evidence of recent degenerative nephritis, in the second or third stage of the disease, in that period at which the disturbance of the secretion is greatest; a point upon which Professor Virchow lays great stress, as he attributes the chalky deposit to impeded excretion by the urine. In the first, a maid-servant, aged forty-three, the upper lobe of the left lung exhibited posteriorly a hard spot, the size of a nut, on section yellowish white, dry and friable, which proved to be chalky infiltration. Similar smaller spots were disseminated through the lung, The second case occurred in a girl aged fifteen, who presented a large mass of sarcoma, which had perforated the temporal bones from without inwards. The right parietal bone was occupied by a similar growth; the posterior part of the inferior lobe of the right lung presented several dense, dry, hard nodes of chalky infiltration. In the third case, a young man of nineteen years, who died of necrosis of the left femur, the mucous membrane of the stomach was found infiltrated with calcareous matter. The fourth case occurred in a man, aged seventy-three, who had laboured under a cancroid affection of the lip, of the clavicles, and several ribs, and died of gangrene of the lung. Here the lungs exhibited, in addition to spots of cancroid disease and gangrene, dense calcareous infiltration in a part that was very emphysematous, so that the septa of the tissue resembled hard spiculæ, and the inner surface of the pleura was invested with thick deposits. The last three cases resembled that of the young lady in Berlin in regard to the co-existence of extensive bone disease. In the first there was no apparent source from which the lime salts could be derived, but the bones were not especially examined. The chalky infiltration of the mucous membrane of the stomach, which is noted in two cases, deserves a little further attention. In both the altered parts were altered in appearance and to the touch. They appeared opaque, whitish, more or less spotted, feeling dry and resistant; in the first case observed the tissues crepitated on section. The microscope demonstrated a fine granular deposit (blackish by transmitted, white by reflected, light), external to the follicular structures, which would probably mean that a portion of the glandular tissue had been absorbed to make way for the deposit. Acids dissolved the granules, and caused an evolution of carbonic acid gas; after the application of sulphuric acid, sulphate of lime crystals formed. The co-existence of extensive disease of the bones with extensive ossification of the vessels of the medullary portion of the brain, in the case of a young man aged twenty-six, is also quoted in illustration of the view advocated by Professor Virchow.

Professor Virchow was of opinion that in these cases he had to deal with a direct cretification of the tissues, bearing, as he thinks, very materially upon the doctrine of metastases, on which account he has selected the title for his paper that we have placed at the head of this notice.

8. *Investigations into the nature of the Skerljevo Disease, and of some morbid conditions that have been compared with it.* By Professor C. Sigmund. (Zeitschrift der K. K. Gesell. der Aerzte zu Wien, 1, 2, 3 Heft. 1855.)

Affections known by the names, *Skerljevo* disease, *Male di Breno*, *Falcadina*, *Boala*, *Frenga*, prevail in the frontier countries of the Austrian empire, and are generally regarded as contagious, of syphilitic origin, and as being introduced from an adjoining country. Professor Sigmund has had opportunities of witnessing these disorders in their respective countries, and of treating them in the hospital of Vienna. In the papers before us he gives the results of his observations, and commences his account with a delineation of the Frenga.

1. The *Frenga* has existed in Servia since the Russian and Turkish troops occupied the country in 1810, and a government commission has elicited that it was unknown there previously. The following is a summary of one of the cases of Frenga that was under Professor Sigmund's care:

F. W., aged 20, well-built, a Servian tradesman, the only son of a man who died young of gout in the bones, was subject to ophthalmia from his 6th, and to tonsillitis from his 13th, year. When 17 years old, he perceived streaks of blood in his nasal mucus. There was intense headache. Subsequently the discharge grew purulent, the headache gradually became more severe, and in his 18th year, both nostrils became plugged up for days together, and by violent efforts brown coagula were forced out. The nose became swollen and the eyes œdematous; subsequently fragments of bone were discharged, and perforation of the bony septum took place. Dyspnœa, dysphagia, and difficult articulation followed, and the patient, not benefiting materially by the treatment pursued, came to Vienna. At that time the patient, who was well-fed, exhibited a perforation of the septum narium of the size of a sixpence. There was a muco-purulent fetid discharge from the nose; the posterior part of the hard palate was denuded, and there were sharp-cut ulcers on the soft palate, and one ulcer as large as a two-shilling piece at the back of the fauces. There was anosmia, breathing laborious, deglutition very painful, speech nasal, cervical glands enlarged, but no pain in the ulcerated parts. The actual cautery, followed by nitrate of silver, was applied locally to the exposed palate and the ulcers, and for four weeks iodide of potassium, exhibited in doses gradually increased to thirty grains, daily. The syrup of iodide of iron was next given for a fortnight. During the first three weeks of treatment, daily inunctions were made with half a drachm of mercurial ointment. Under this treatment a complete cure was established in five weeks, which has proved permanent three years after.

The government commission, to which we have adverted, states, that the symptoms which we have found in the above case, are those that mark Frenga—viz., the tedious course, the osteocopic pains with which the disease commences, the plugging of the nose, pain and ulceration in the fauces and palates. The commission declined expressing a categorical opinion with regard to its nature. They concluded that it was not identical with syphilis, scrofula, gout, or herpes; but that it must be regarded as a peculiar and idiopathic disease, closely resembling the Skerljevo malady. The commission satisfied themselves of the facility of effecting a cure by proper treatment, local and general, of the kind adopted above; mercurials being indispensable. Professor Sigmund enters into a disquisition as to the nature of the disease, and arrives at the conclusion that it is syphilitic, either acquired or hereditary; for if we are unable to show the original and primary syphilitic affection in the same individual, it does not follow that we may dispute the syphilitic character of his malady, which may be due to hereditary influences, or to the transference of secondary syphilitic diseases.

2. *Falcadina* is a disease occurring in the province of Belluno, and derived from the township of Falcade, where it is said to have first appeared in 1790, and to have been introduced from Fiume or Venice. The first cases exhibited manifest symptoms of primary syphilitis. Falcadina is characterized by severe osteocopic pains in various parts of the body, tumefaction of the affected bones, swelling of the superficial lymphatic glands; flat, pale-red or brownish-red eruptions, or vesicular, pustular eruptions; or warty, nodular, or granulating excrescences; ulceration of the fauces and nose, with destruction of the adjacent bones; difficult articulation and deglutition. All the cases of Falcadina are accompanied by scabies, which is not surprising, in a district where the majority of the population suffer from it. The treatment by mercury and iodides is as successful here—and in the Male di Breno, which presents the same symptoms as Falcadina, under a different name—as we have seen it to be in the Frenga.

3. The same applies to *Boala*, the analogue of the other diseases examined, but occurring in Bulgaria and Wallachia.

4. *Skerljevo* is a term applied to a variety of morbid appearances, mainly consisting in ulcers of the cutaneous surface, of the mucous membrane of the nares, mouth, and fauces, accompanied by affections of the bones and cartilages, and especially by periodical pains and inflammations of these parts. The term is derived from that of a village in the vicinity of Fiume. It was first brought to the cognisance of the Austrian government about 1800, by its rendering the recruit unfit for service, and was first called *male scabioso venereo*, the venereal itch. In that year, in the population of the small tract along the coast, there were as many as 2600 Skerljevo cases. An hospital was established in Portoré in 1818, solely for their treatment, and the gradual diminution of the number of cases is a striking proof of the advantages it confers. The number admitted in 1818 was 1855; the average of the last five years, 253. From an analysis of the cases admitted under the name of Skerljevo, it appears that the term is applied to various ulcerative diseases, differing in their origin, and found throughout the Austrian empire,

but occurring with special virulence in the sea-coast districts. Professor Sigmund is of opinion that the Skerljevo disease is identical with the Frenga and Boala just spoken of, with the Radesyge of Scandinavia, and Sibbens of Scotland, and that it consists essentially in syphilitic affections of the soft and hard tissues. The great majority of forms presented by the Skerljevo malady are those of tertiary syphilis. The reason of their producing such fearful results is sought in the extreme neglect of all cleanliness in the dwellings and persons of the coast districts; the climate, which is very changeable, is also regarded as influencing the character of the disease to a still greater extent; and the great poverty of the inhabitants is no less an important element in the perpetuation of the disease. The treatment is essentially mercurial, and perfectly successful.

Professor Sigmund sums up the whole subject thus:

1. The popular terms, Frenga, Falcadina, Male di Breno, Boala, Skerljevo, were originally used to designate morbid conditions manifestly and traditionally connected with syphilis. Their origin was always traced to the infection imported by strangers.
2. In the course of time the above terms came to be applied to a variety of ulcers, eruptions, and other diseased conditions in no way connected with syphilis, so that they lost their peculiar signification.
3. The majority and chief forms classed under the above names may still be found to belong to the family of syphilis; they put on more marked characters in the sea-coast districts, Serbia, Italy, &c.; but this is only a difference of degree, not of character.
4. The same causes that affect the spread of syphilis and favour the secondary symptoms in the countries alluded to, do so elsewhere.
5. The popular terms lead to a great deal of mischief, by causing erroneous impressions both among the non-medical and medical public, and ought to be abandoned.

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9. *Neuromas of the Perinæal Nerve.* By Dr. Passavant. (Virchow's Archiv für Pathol. Anat., &c., Band viii. Heft 1.)

There are about nineteen cases on record in which numerous neuromata have been observed on one or several nerves of the same individual. One of the most remarkable is that detailed by Professor Smith, in his monograph on the subject (Dublin, 1849). Dr. Passavant adds one to the list, which is curious on account of the site of the neuromatous growths. They were found in a man, a gardener, who died at fifty-eight of phthisis, and had never complained of pain or inconvenience resulting from the tumours which occupied the perinæum to the number of above one hundred. They were roundish, of the shape of a bean, varying in size from the size of a hazel-nut to an almost imperceptible swelling of the nerve. They occupied the left perinæal nerve, more or less closely aggregated, along the trunk and its ramifications into the scrotum. The perinæal nerve of the left side was perfectly normal. The tumours presented a fibroid character, and the flattened nerve generally ran along one side; in a few instances it was spread out over the tumour, so that the latter occupied a concentric position. Some of the very small tumours were filled with a gelatinous fluid, and the author consequently regards the exudation of a fibro-serous matter as the first step in their growth.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

A General View of the Results obtained by Subcutaneous Surgery. By M. Guerin. —In a paper submitted to the Académie des Sciences, M. Guerin declares that all his subsequent experience is only confirmatory of the statements made by him in his celebrated memoir in 1839. In a general review of the subject, he here re-states, in a summary manner, the principles upon which subcutaneous surgery is based, and the practical applications it is susceptible of.

1. *Tissues when divided under the skin undergo immediate organization.*—In the first memoir, he showed that if the absence of contact of the air and hermetical closure of the orifice of the incision were secured, immediate organization, with an absence of local and general reaction, resulted. He now adds that there are certain incidental conditions which, if not provided for, may interrupt this process, and cause failure even when the main condition of exclusion of the air has been observed. In this point of view the behaviour of the *fluids* of the economy is of importance. Some of these, as *arterial* blood, are *organizable*, and while a moderate quantity effused between the lips of the wound acts as an important element of their junction, even large quantities thrown out under the skin are absorbed with great rapidity. Other fluids, as *venous* blood, are *inorganizable*, and while portions of that effused may be resorbed, the remainder continues to offer a mechanical obstacle to immediate organization. Then, there are excreted fluids, such as bile, urine, and pus, which are *antipathetic* to immediate organization. Pus, confined under the skin, may either undergo chemical change, or retain its normal characters. In the former case, the smallest portion will immediately excite reaction, and in the other gives rise, by inoculation, to the secondary formation of small cold abscesses.

The cicatricial tissue resulting from the healing of ordinary wounds bears no resemblance to any of the normal tissues, and constitutes a functional interruption in the organs in which it is deposited. In the immediate organization which takes place after subcutaneous wounds, all divided tissues are susceptible of producing their like at the cut surfaces, muscle, muscle, nerve, nerve, and so on, each endowed with its proper power. When, however, from the effusion of too much blood by surrounding vessels, the divided surfaces are kept too far separated, and the proper blastema is not exuded, functional interruption ensues here also, the extremities of the cut tissues becoming atrophied and losing their specific organization. This is the case with muscle, tendon, nerve, &c., but in none so obviously as with arteries, which are sometimes converted into fibrous cords for the whole length of the limb, contrasting with the integrity of their calibre when the contiguity of the divided surfaces has been maintained. The lameness which has often resulted from section of the tendo-Achillis, and the loss of motion of the fingers almost always produced by a division of the flexor tendons, are dependent upon the production of heteromorphous tissue consequent upon the non-observance of some of the conditions necessary for immediate organization. So, too, in the operation for strabismus, the functions of the muscles, when divided in an exposed state, are more or less impaired.

2. *Surgical Applications.*—Although tendons had already been divided by operations somewhat analogous to those of M. Guérin, this was an empirical procedure only, having at most the object in view of limiting the amount of resulting inflammation. M. Guérin claims to have raised these subcutaneous operations into a method, based upon principles deduced from the teachings of experiment, and clinical and pathological observation. The *object* of this method is not to merely limit the amount of suppurative inflammation, but to absolutely prevent it, and to secure immediate organization by homogeneous tissue. The *means* of effecting this are comprised in four principal rules:—1. Make the wound in the skin at the greatest possible distance from that of the tissue to be divided, so that the track joining the two may be sinuous; 2. Circumscribe the section to the tissues to be divided, isolating them from surrounding parts by tension, contraction, &c.; 3. Prevent the effusion of inorganic or antipathetic fluids into the wound, and expel all such, as well as air, from its track after the operation; 4. Close the lips of the external wound securely by adhesive plaster. The *results*, when the conditions of the operation are observed, are so satisfactory, that M. Guérin declares that, in many more than 2000 cases, he has never met with a suppurating subcutaneous wound. In conclusion, he gives a summary of the various applications he has made of this method.

(A.) *Subcutaneous Sections:*—1. *Of the skin and cellular substance.*—In five instances, M. Guérin has detached bridles of cellular tissue, which by displacing the skin, or causing its adhesion, have produced grievous deformities of the face or neck. 2. *Tendons.*—These operations are now so common as to call for no remark, farther than indicating the superiority of the results attained compared with those of ordinary tenotomy. 3. *Aponeuroses.*—Besides the section of these as an orthopædic procedure, M. Guérin has resorted to it with success in *débridement* in inflammatory tumefaction. One of the most original applications of subcutaneous tenotomy was its employment in old and congenital dislocations, and the success that attended this practice has led to its adoption in aid of the reduction of recent fractures and dislocations. 4. *Muscles.*—The largest as well as the smallest muscles of the body have now been divided, some of the operations involving large muscular masses. Among the most important may be mentioned those applied to spinal curvature, congenital luxation of the femur, and strabismus. In one case, reported to the Académie, M. Guérin divided forty-two muscles and tendons, for general deformity of the articulations, at a single sitting, no suppuration or fever resulting. Myotomy has also been applied to the *radical cure of reducible hernia*, and for *débridement* in *strangulated hernia.* In the first of these, the entire thickness of the muscles and aponeuroses constituting the canal is traversed in various directions, the resulting exudation proving an effectual obturator, as witnessed in several of the author's eleven cases. 5. *Ligaments.*—The section of these in such deformities as resist the mere division of tendons and muscles has now been performed times out of number. 6. *Vessels*—In several instances, subcutaneous incision and scarification have converted vascular tumours into cicatricial tissue, and that without producing the suppuration consequent on ligatures; and various subcutaneous operations have been performed on the veins. 7. *Nerves* have been thus divided in neuralgia with advantage. 8. *Cartilages.*—M. Guérin states as a fact—of which we have met with, however, no confirmation elsewhere—that subcutaneous section of the symphysis pubis, for facilitating labour, is of common occurrence in France. 9. *Bones.*—Among these we have the subcutaneous ablation of painful exostoses, and the fracture of rickety bones in order to rectify their curvatures. M. Guérin has shown that in such curvatures the old bone is reduced to lamellæ, which are lost in bone of new formation. At the second stage of the disease, the latter is spongy and flexible, and if we make an incision half through the arch of an angular curvature, a deformity that threatened to become permanent may be at once removed.

(B.) *Punctures and Extractions.*—Under this head M. Guérin refers to three of the applications, as indicative of surgical progress: 1. The opening of large *congestive abscesses.*—So great is the danger attendant upon this, performed in the usual modes, that we have no example on record of success; the opening, in cases where recovery occurred, having taken place spontaneously, the pus gaining issue through a sinuous track at a distance from the seat of the collection. It is by imitating and generalizing this process of nature that the subcutaneous mode has been enabled to convert recovery into the rule and death into the exception. 2. *Thoracentesis.*—In a recent memoir, M. Guérin detailed 30 cases of this operation, deducing from them the conclusion, that the operation is devoid of all danger, and cures whenever serious complications do not exist. 3. The *extraction of foreign bodies* has been repeatedly performed with success.

(C.) Intermediate between the other two is another category of applications, comprising: 1. The *abortion of imminent phlegmon.*—M. Guérin believes that all phlegmon commences by a nucleus, a thorn as it were, in the cellular tissue, and has found, if this be divided by the subcutaneous incision, no suppuration will ensue, though the phlegmon may have reached a considerable size. 2. He has effected the *destruction of painful cervical glands* in four instances, by fixing the gland and dividing it in various directions, thus separating it from the surrounding vessels and nerves. It becomes converted into an insensible, amorphous, cicatricial tissue, which is afterwards resorbed. 3. The destruction of *painful tumours*, forming in the substance of muscles.—Under this head, too, may be noticed the section of fatty and other encysted tumours (*loupes*). Trifling as the operation is, when performed in the ordinary way, ablation has not infrequently led to fatal results.—*Gazette Médicale*, 1855, Nos. 4 & 13.

Treatment of Laryngitis by the Insufflation of Powdered Nitrate of Silver. By M. Ebert.—M. Ebert observes that every surgeon who has attempted the application of solutions of nitrate of silver to the larynx must be convinced of its difficulty, if not impossibility. Indeed, he doubts whether the fluid ever reaches that organ at all; and in two cases of croup, in which the attempt was made by a most expert surgeon, he found after death that not a trace had penetrated into the larynx itself. In repeated attempts in the dead body he has constantly failed to introduce a sponge mounted upon a whalebone, and only after manœuvring could he get a silver catheter to enter. How much more difficult must it be, then, during life, when the very attempt stimulates all the neighbouring muscles to resistance.

On the other hand, nitrate of silver, in powder, may be introduced by the simplest agency. We all know how easily pulverulent substances, drawn in unawares by a deep inspiration, enter the larynx, and give rise to irritating cough. Trousseau was the first to apply this fact to the treatment of laryngeal affections by nitrate of silver; and Professor Burow, of Konigsberg,* speaks very highly of this mode. He mixed three grains of arg. nitr. with one drachm of saccharum lactis, and caused the patient to insufflate daily as much as would lie in the barrel of a steel pen. Chronic laryngitis, which had gone on to the production of complete aphonia, was thus cured in a few weeks. Since he saw this account, M. Ebert has used the same mixture with surprising success in several cases of laryngitis. So minutely does the sugar of milk divide the nitrate, that if even a small portion of the powder only reach the larynx, it will still contain its proportion of the nitrate. He administers it in the following way. A steel-pen, charged with as much powder as it will hold, is attached to one end of the barrel of a quill, which is also open at the other end. This is introduced far enough into the mouth to bring the steel pen opposite the root of the tongue. The lips are now closed around the quill, and the nostrils compressed, while the patient is desired to draw in air rapidly and forcibly through the quill-barrel. Almost every one fails at first, but all succeed on the second or third attempt—the cough and irritation of the larynx announcing the penetration of the powder there. Even delicate females and children easily practice the insufflation, and will repeat it for days or weeks together. Young children may have it administered by an apparatus contrived by Professor Burow. M. Ebert has as yet only employed the remedy in laryngitis; and he briefly relates six cases of its successful application.—*Annalen des Charité Krankenhauses*, Band v. p. 89.

On Dr. Landolfi's Treatment of Cancer. By M. Lasegue.—Dr. Landolfi, surgeon in-chief of the Sicilian army, and lecturer on cancerous diseases at the Trinity Hospital, Naples, is now visiting different parts of the Continent, for the purpose of propagating his method of curing cancer. Having secured many adherents at Vienna, he has repaired to Paris, where a certain number of patients, selected from Salpêtrière, have been placed under his care, a medical commission watching the results. As this method, though exciting much attention in Italy and Germany, is scarcely known in France, and we may add in England, M. Lasègue, while awaiting the report of the commission, proceeds to give some account of it; and although naturally prejudiced against any specific method of treating the disease, the above-board course of procedure adopted by Landolfi, and the large amount of testimony of success he adduces, have evidently made a considerable impression upon him.

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* Deutsche Klinik, No. 21. 1853.

The specific employed by Landolfi is the chloride of bromine, applied externally as a caustic, and administered internally, the latter being of quite secondary importance. This caustic, mixed into a thick paste with liquorice powder, may be employed alone, or it may be combined with other caustics, as in the following formula: ℞ Chlor. brom. three parts, chlor. zinc. two parts, chlor. antimon., chlor. auri. ana one part. To be mixed in the air, on account of the fumes disengaged. In open cancer, Landolfi regards the chlor. zinc. as indispensable as a hæmostatic; and the chlor. auri. seems to exert a special action in encephaloid. Cutaneous cancer, epithelioma, lupus, and small cysto-sarcomas may be treated by an ointment formed of one part of chloride of bromine to eight of basilicon. The healthy parts around the tumour are to be protected by bands of linen one and a half to two inches broad, covered with ointment (four parts of chloroform to thirty of lard), and the patient is placed near a window, so that the fumes may escape. Small compresses, upon which the paste has been spread, are gently applied to the part, in an imbricated manner, so as to secure exact juxta-position, keeping two lines clear of the sound parts. The whole is then covered with charpie and diachylon. A sharp burning sensation is soon followed by severe pain, which may last for several hours, and is combated by repeated doses of anodynes. The paste is usually kept on for twenty-four hours, and on its removal a line of demarcation is generally perceived. The tumour is in part white, and in part reddish, or marbled with yellow and blue. Bread or lettuce-leave poultices, or basilicon ointment, are now applied every three hours. As the gangrene proceeds, the pain diminishes, and about the fourth or fifth day the eschar loosens, being removable by the forceps from the eighth to the fifteenth. A healthy granulating surface is then exposed, and if any vestige of the disease is observable, a little paste is reapplied to that spot. The wound is now to be treated as a simple ulcer, and if there is a deficiency of suppuration a lotion is to be applied, containing from twenty to thirty drops of the chloride of bromine in five hundred grammes of goulard water. The ulcer usually heals rapidly from the circumference, the cicatrix resembling that resulting from incised wounds.

In spite of the severe pain, there is rarely febrile reaction, and no change in the patient's regimen is required. Although Landolfi believes the paste acts by absorption, as well as locally, he regards internal treatment as only adjuvatory, and only so employs it, in the hopes of preventing relapse. The treatment, even in very bad cases, usually at least affords very notable relief, and in such as are quite hopeless or inaccessible to caustic, a lotion, containing ten to twenty drops in five hundred grammes of water, may exercise some useful modifying power.—*Archives Générales*, May, 1855, p. 609.

On Resection of the Head of the Humerus. By M. Baudens.—In his clinical lectures on "Gunshot Wounds," as far back as 1836, M. Baudens laid it down as a precept, of which additional experience has only confirmed the correctness, that in injuries of the upper extremity by balls, resection should be the rule, and amputation the exception. How far this applies to the head of the humerus may be seen from the fact that of fourteen cases in which resection was preferred to amputation at the shoulder-joint, thirteen recovered. Referring to the recorded experience of others in this operation, he observes as a usual result that there is great impairment of the power of moving the humerus, whereas he has always found, when the resection has been limited to the head of the bone, all the movements of the arm consistent with the ginglymoid form of the new articulation, were procurable. To obtain this, however, it is necessary to keep, during the whole period of treatment, the humerus in immediate contact with the articular cavity, or its margin. So, likewise, in the operation itself, the muscular substance must be respected as far as possible; and on this account he objects to the flap-operation, and to the incision as performed by White. M. Baudens makes it on the inner side of the arm, just externally to the coracoid process, for there the head is more superficially placed, and may be exposed through its whole extent by prolonging the incision between the acromion and the coracoid, and we are able to guide ourselves through the bicipital groove to the four muscles attached to the tuberosities, whose section is the key-stone to the operation. The moment they are divided the capsule becomes widely opened, and the head of the bone, which had been drawn up, descends, and is easily accessible through the breach in the capsule. The posterior parts of the capsule should be but little disturbed, and the section of the bone should be as sub-periosteal as possible, the periosteum so preserved being applied close upon the end of the humerus.

It must be laid down as a rule, that resection must be confined to the lesion, so as to leave as much of the osseous tissue as possible. Thus, in one case, M. Baudens only removed one half of the head; and when the lesion does not extend beyond the anatomical neck, he attaches the greatest importance to avoiding injury of the attachments of the capsule, this not requiring, indeed, to be opened after the section of the muscles. When the lesion compels us to go beyond this neck, it is very important, for the preservation of future motion, not to divide beyond the insertions of the deltoid, pectoralis and teres major, and the latissimus dorsi. As these insertions extend to a great distance, they may often at least be spared in part, as on this condition alone can any movements of the arm be preserved. In a case where the resection took place below the deltoid, although all movements of the humerus were lost, yet by fixing this against the side, those of the fore-arm and hand were made quite available.

In answer to the query determined affirmatively by Larrey and others, whether amputation is required when the fracture extends from the head of the bone, through the diaphysis into the medullary cavity, M. Baudens states that he has met with four cases in which he performed resection of the head, without such longitudinal fissures seeming to impede the cure. He regards the indication of resection, in all cases of injury of the head of the bone by a ball, as *absolute*. We have a penetrating wound of the joint, with admission of foreign bodies, and the severest accidents are otherwise to be dreaded. The patient may die of purulent infection, or have to undergo subsequent resection; or indeed he may survive with anchylosis, and become a victim to the endless sufferings consequent upon fistulous openings and the discharge of dead bone. *Immediate resection* is much to be preferred to consecutive. Of 26 patients in the ambulances of which M. Baudens had charge, the resection was performed immediately in 11, with 10 recoveries. Fifteen, from their injuries seeming less grave, were treated by expectation. Of these, 8 died of purulent infection, 3 underwent consecutive resection with success, and 4 suffered a long train of ill consequences from fistulous openings.

The new articulation formed after resection differs essentially from the old one. The division of the attachments of the muscles to the tuberosities abolishes all rotation of the humerus on its axis, and the movements of the bone are still more limited by the shortening it has undergone from loss of substance. Such shortening is, however, lessened when, through the careful application of the detached periosteum to the divided end of the bone, a partial reproduction of the osseous structure takes places. Thus far, M. Baudens has always found the new joint assume the *ginglymoid* form, this being more powerful in proportion to the completeness with which the insertions of the deltoid and other muscles have been preserved.—*Bull. de Thérap.*, 1855, No. 6.

Report on the Employment of Mathysen's Gypsum Bandage at the Charité Hospital, Berlin. By Drs. Grimm and Jungken.—Dr. Mathysen's bandage was employed in thirty-six cases of fractures of various kinds early in 1854. The bandage is prepared by stretching it upon a table, and well rubbing powdered gypsum into it on each side. It is then rolled up or otherwise arranged, according to the use to be made of it. Immediately before applying it, it is dipped in water or other fluid, the limb being protected by a flannel or other bandage prior to its application. Any portion of the bandage that is found not to have become wetted is moistened by a wet sponge. Flannel will take up twice as much gypsum during the rubbing than linen; but it is more clumsy, and not so easily applied. If a very firm, immovable bandage is required, some of the gypsum, in the form of a thin paste, should be applied during the last turns of the bandage. Its appearance is much improved by passing a damp sponge several times along it while still wet, and at a later period it may be smoothly polished by means of glass. To remove the bandage, it only requires to be again well wetted.

The Reporters pronounce this bandage as the best of all hitherto invented, including those that most resemble it, as the starch-bandage, upon the following grounds:—1. The rapidity with which it hardens. 2. Its simplicity and easy application. 3. Its small cost. 4. The ease with which it may be removed—the linen composing it being available, after twenty-four hours' soaking, for new bandages. 5. Its firmness and immovability render it suitable for the most oblique and difficult fractures. 6. From its rapid hardening and its firmness, it is well adapted for those cases which require extension and counter-extension to produce coaptation of the fractured parts. The position obtained remaining unchanged, apparatus of extension, so uncertain in operation, and so annoying to the patient, is not required. 7. The ease with which it is borne. 8. Its porosity. Cutaneous transpiration is not quite suppressed, and if the fracture be complicated by wounds, ulcers, &c., these are indicated by the discharges making their way through the bandage. 9. The gypsum bandage is a good conductor of heat, and a bladder of ice placed over some oil-skin, around the fractured part, takes effect in five minutes. 10. When the bandage is properly applied, the form of the limb is so well displayed, that any irregularity of the fractured part may be judged of externally. 11. Its handsome appearance and regularity distinguish it from all analogous bandages. 12. Fractures seem to unite sooner under its employment.—*Annal. des Charité-Krankenhauses*, Band v. p. 157.

On the Seat and Varieties of Cataract. By M. Malgaigne.—Not only is M. Malgaigne one of the ablest practical surgeons of the present time, but he has, by his patient research and great critical sagacity, been the means of overturning more than one time-honoured but erroneous doctrine. He thought he had succeeded in doing this as regards cataract, in his communication to the Académie de Médecine, in 1841; but finding some persons still holding erroneous ideas upon the subject, he, in this paper, re-states the case at some length.

The history of opinion upon the seat and nature of cataract may be divided into an ancient and a modern period. During the former, extending from the school of Alexandria to the beginning of the eighteenth century, the site of cataract was placed in front of the crystalline. The modern period is divisible into certain epochs—viz., 1. That of 1705, in which Brisseau demonstrated that the crystalline itself was the seat of cataract. 2. Between 1755 and 1763, Tenon and Hoin announced the existence of capsular cataract and cataract of the liquor Morgagni. 3. From 1790 to 1817, the German school multiplied the varieties of cataract beyond all measure. 4. M. Malgaigne published in 1841 the result of his necroscopical researches upon the subject. Prior to 1840, he believed in the existence of lenticular and capsular cataract, as other people did, when accident led him to investigate the matter in a succession of persons dying with cataract, at the Bicêtre, and to his astonishment he never could discover any traces of a capsular cataract. In 1841, he communicated accounts of twenty-five autopsies, and since then, these have reached to more than sixty. In none of these did he ever find the *capsule* opaque, or the opacity of the lens beginning at its centre—the cataract invariably commencing in the soft layers of the lens lying nearest the capsule, the opacity, in the great majority of cases, being complete at its anterior and posterior surfaces, while the nucleus continued transparent. In some rare cases the nucleus was also opaque. In other rarer cases, the capsule was found thickened from the deposition of coagulable lymph attaching it to the iris; but in no instance could he find an example of a simple *capsular cataract.* After a full consideration of the subject, and an examination of the criticisms that have been advanced, he comes to the final conclusions—1. That the existence of a cataract commencing in the centre of the lens is as yet purely hypothetical. 2. There is no example of a simple capsular cataract without opacity of the lens. 3. Complicated capsular cataract may form an exception to this rule; only two instances of this, however, having been demonstrated. As to the cataract of the liquor Morgagni, the author's researches lead him to deny the existence of any such fluid. As a final *résumé*, it it may therefore be stated that, to the present time, two varieties of cataract only are known, lenticular and capsulo-lenticular—the change in the crystalline always commencing in the layers adjoining the capsule, although this itself remains transparent.

In a note, M. Malgaigne makes an observation respecting the mode of judging of the transparency of the sound crystalline, when removed. The light can be very well seen through it, but objects cannot be distinguished. The surface has lost the polish bestowed on it by its capsule, and it resembles a piece of broken crystal, which, although transparent, refracts the rays too much to allow of distinct vision. If, however, we attach the object to the lens, we then discern it wonderfully—the smallest fibres, for example, of the tissue of a dissecting-room apron, upon which the lens has been laid, being distinctly visible. Applying this test to opacities, whether central or peripheric, slight or thick, yellow or brown, it becomes impossible to see the texture of the apron.—*Rev. Méd. Chir.*, tome xvii. pp. 18 & 85.

Case of Cure of an Artificial Anus by means of the Suture. By M. CHAISSAIGNAC.—The rarity with which the suture has succeeded in curing an artificial anus, unaided by any preliminary or autoplastic operation, justifies M. Chaissaignac in giving this interesting case in some detail. It occurred in the person of an *employé*, aged fifty-one, who had had a very small reducible umbilical hernia for twenty years. In February, 1853, this hernia protruded, with symptoms of strangulation; and after its reduction a small tumour, situated three centimetres above the umbilicus, and perceived now for the first time, inflamed, and presently fluctuated. Towards the end of March this was opened, and fæces discharged; and at the end of some days a fistulous opening was established, pus and fæcal matters continuing to issue. The odour of these indicated the transverse colon as the part involved. Nutrition continued good, and the passage of the stools was re-established. Whenever this was at all delayed, however, the entire fæces were discharged through the fistula. This condition having persisted for several months, iodine injections were employed every third day, together with pressure on the abdomen. After the seventh injection the cicatrization seemed complete, and the patient was supposed to be cured. About a month later fever and local inflammation were set up, the fistula re-opened, and discharged as before. A partial healing took place, and then a reproduction of the same accidents, so that it was determined to resort to an operation.

An elliptical portion of skin was removed for the purpose of an exact examination of the parts, and a well-defined, rounded aperture was found in the aponeurotic wall formed by the linea alba, through which fæcal matters passed. It was placed at some depth below the surface, and there were lateral diverticula extending between the aponeurosis and the skin. It was now evident that the patient had suffered from one of those small herniæ that obtrude between the fibres of the linea alba, and that the colon, pinched within the edges of these, had sphacelated to a small extent, and then contracted solid adhesions with the edges of the aponeurotic orifice—the subcutaneous detachments establishing a track of several centimetres between this and the cutaneous orifice. The iodine injections were unable to effect a closure of the inflexible edges of the aponeurotic orifice, although they had ameliorated the condition of the subcutaneous diverticula. An autoplastic operation, allowing this orifice to remain open and discharge matters under the skin, was obviously useless; and M. Chaissaignac succeeded, though with great difficulty, owing to its depth, in paring the edges of the aperture, and, traversing it with ligatures, raising it to the level of the cutaneous wound, and attaching it there at each extremity of the ellipsis, the external wound being closed by the quilled suture. The cure was complete, the cicatrization continuing sound when examined a year afterwards.

In commenting upon the case, M. Chaissaignac draws attention to the great difficulties that prevail in the diagnosis of these small *latent epigastric* herniæ, other forms of hernia being always co-existent with them, to which the symptoms of strangulation are naturally referred, and especially when the *embonpoint* of the subject prevents their detection. Without absolutely condemning *iodine injections* in intestinal fistulæ, his experience has not hitherto ran in their favour, for besides their inutility in the present instance, he refers to another case in which their employment was followed by erysipelas and fatal peritonitis.—*Archives Générales*, May, 1855, p. 820.

On the Healing of Abscesses by the First Intention. By M. Chaissaignac.—M. Chaissaignac has for some time past endeavoured to unite abscesses by the first intention, after their complete evacuation; and he reports that his success has been very considerable. His method of procedure may be judged of by the narration of a case which recently occurred at the Lariboisière Hospital, and was observed by all who attend there. A healthy man, aged nineteen, was admitted February 17th, presenting all the symptoms of an acute abscess of the axilla, which had been about a week in forming. On the 19th, chloroform having been given, a considerable quantity of well-conditioned pus was discharged by the bistoury, pressure being exercised in all directions for the purpose of securing complete evacuation. The cavity of the abscess was next thoroughly washed out with water introduced through the tube of an irrigator, in order to bring away any remaining pus, the injection being continued until the water returned completely limpid. Pressure was again employed to force out every drop of the water, and the orifice was strapped up. A large pad of charpie was introduced into the axilla in order to make pressure over that region, and the arm was confined in one of Mayor's bandages, as if for fracture of the clavicle. On the 21st cicatrization was complete, no discharge of pus whatever being visible. The bandage was continued as a matter of precaution for two or three days, and then the arm was allowed to hang down, no pain being reproduced. In the site of the abscess a little indurated spot could be felt. On the 27th he was discharged quite well.*—*Gazette des Hôpitaux*, 1855, No. 38.

On the Removal of Articular Bodies by the Subcutaneous Section. By M. Chaissaignac.—The author prefers this designation to that of loose cartilages, as prejudging in nowise the nature of the bodies. In the present paper he relates two cases in which the operation devised by M. Goyraud of Aix was performed with success. This consists in opening the joint by the subcutaneous section, and forcing the articular body through the track of the incision into the cellular tissue, and leaving it there for future removal. The following are the conclusions the observations of these and other cases have induced M. Chaissaignac to arrive at. 1. The pain, which is induced by the pinching exerted by the surfaces between which they are compressed, is not felt when the bodies are voluminous. 2. The character of this sudden pain is not pathognomonic, pain quite similar to it accompanying certain invasions of rheumatism or gout, and the dislocation of the semilunar cartilages. 3. Our diagnosis may be at fault, from our mistaking the slipping of the fingers over the walls of the articular sac for the displacement of a mobile body—a mistake that may far more easily occur than would be supposed. We have also to distinguish these bodies from partial indurations of the capsule, and from inequalities of the edges of the osseous articular extremities. 4. Among the concomitant affections that may be produced by the presence of these bodies are hydrarthrosis and anchylosis. 5. Although when hydrarthrosis produces great tension it is an obstacle to our diagnostical examination, a moderate repletion of the capsule favours the exploration. 6. The place of election for the operation on the knee-joint is the lower *cul-de-sac* of the synovial membrane on the inner side. 7. Before commencing any operation, small articular bodies must be previously fixed by acupuncture, as there is always great danger of their escaping at the moment of operating. 8. When these bodies are multiple, we should collect them all at one point, so as to expel them by a single operation. Still, where one or two escape us, the operation does not always fail. 9. Articular bodies left under the skin for a considerable period, undergo a great diminution, so that a secondary operation for their removal is not always required. 10. So important is it to avoid suppurative inflammation after the operation, that leeches should be freely applied at the root of the limb, both as a preventive and curative measure. 11. Angioleucitis is the species of inflammation most to be dreaded after operation upon the knee. 12. In expelling the foreign body from the joint, we should endeavour that its course should be made as long as possible, experience showing that in such case a portion of the track may suppurate with impunity, or at all events without the pus invading the cavity of the joint.—*Revue Médico-Chirurgicale,* tome xvii. p. 148.

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* See Medical Times, March 31, p. 316.

On the Dressing of Amputated Limbs. By M. Coste.—In this paper, M. Coste, who is senior surgeon to the Hôtel Dieu at Marseilles, describes the mode of treating amputated limbs by what he terms *pansements rares,* or infrequent dressing. He alludes to M. Chaissaignac's mode of dressing by occlusion, which might seem to bear some resemblance to his own, and which consists in carefully closing up compound fractures and other severe lesions by imbricated strapping, which is retained without changing for a week or fortnight. This is, however, especially applicable to severe injuries of bone, where absolute quietude and the exclusion of air are important; and the loading a stump with so thick a covering of strapping would be a very questionable procedure. M. Sédillot's mode of dressing stumps, again, differs also from the author's. In fact, it consists in suppressing all dressing, a large anterior flap covering the surface of the wound by its own weight. A piece of rag, about two fingers' breadth, and spread with ointment, is folded up and applied around the end of the bone, so as to act as a channel for the discharge of the fluids. The angles of the flap are united by two points of suture, and the stump remains uncovered upon the napkin it is to lie upon, and which receives the pus as it escapes. The folded compress applied to the end of the bone is removed on the third or fourth day.

This mode is not applicable to the case of circular amputation, while that advocated by M. Coste may be employed in all modes. After the bleeding is arrested, the lips of the wound are brought gently together by two or three strips of plaster, taking care to leave the angles somewhat open for the passage of the ligatures and the escape of the fluids. Over this are applied a pledget, some layers of charpie, and a bandage. The first dressing only takes place from the sixth to the twelfth day after the operation, and the others from every eighth to twelfth day; the occurrence of severe pain, hæmorrhage, sense of constriction or too abundant suppuration (all of which are extremely rare) indicating a condition of phlegmasia and the necessity of more frequent dressing. The two chief perils after amputation are inflammatory strangulation of the stump and retention of the pus, and M. Coste attributes the non-occurrence of the former in his stumps to the fact that he never employs sutures. He appeals to those who have witnessed his hospital practice as to whether rapid union and the production of a good stump are not the consequences of this mode of dressing.—*L'Union Médicale,* 1855, No. 35.

On Methodical Cauterization of Abnormal Divisions of certain Organs. By M. Jules Cloquet.—In two memoirs recently read at the Académie des Sciences, M. Cloquet furnishes some account of his mode of cauterizing fissures and fistulous openings, founded upon the observation of the powerful contractile effects exerted by cicatricial tissue acting at the angles of wounds caused by burns. He adopted it first in the case of *fissure of the velum palati.* The object was not to cauterize the entire extent of the edges of the division, and bring the granulating surfaces into contact by means of sutures and apparatus—a plan long tried, sometimes succeeding, but oftener failing. The caustic is applied over a very limited surface, at the exact angle of the fissure; and, after the cicatricial tissue which results has had time to produce its retractile effect, the caustic is again applied to the angle of the remainder of the division. The application is repeated again and again, at intervals, so that the fissured parts are thus brought towards each other bit by bit, and united by a series of cauterizations that may be regarded as so many points of successive suture. The operations for this infirmity, devised by Gräfe and Roux, are difficult of execution, and not infrequently fail in success. M. Cloquet first put his plan into execution in a case in which the whole left side of the palate was fissured as a result of syphilitic ulceration. From eighteen to twenty cauterizations with the acid nitrate of mercury sufficed to effect a complete reunion. In another case of congenital fissure, the patient had already been operated upon by Roux' method, but violent coughing had caused the sutures to tear through. Successive cauterizations firmly united one-half of the fissure, with which the patient was satisfied, and refused to persevere. In a third case, M. Nelaton healed a traumatic division by a similar employment of the electric cautery. In 1851, a congenital fissure, existing in a child eleven years old, was completely closed after twenty applications. In all these cases the pain was slight, no change had to be imposed in the regimen or mode of life, and no accidents resulted. Even unpractised surgeons may perform so simple an operation without the aid of an assistant. It is slow in the production of its results, but this is one of the conditions of its success, and is of little consequence, as it does not interfere with the business of life. M. Cloquet thinks the actual cautery is the best means to employ, but for patients who dread this the electric cautery may be substituted.

Another affection usually intractable is *recto-vaginal fistula*, and encouraged by the above success, M. Cloquet applied successive cauterization to its management. The index finger, guarded against the heated body, is introduced into the rectum, and, the external parts being held open, the apex of the fissure is touched by a small cautery, the patient being able to at once get up and pursue her occupations as before. The pain and inconvenience of the application are so slight, that the women readily submit to it. A more considerable amount of union is produced by the early cauterizations than by those made later, so that the complete cure becomes tedious—a less inconvenience from the fact that the most serious consequences of the infirmity disappear after the early cauterizations.—*Gazette Médicale*, 1855, Nos. 9 & 17.

[M. Cloquet refers to but six cases of cure of this fistula, and to six of fissured palate: and we cannot but feel surprised that a procedure—known by him since 1826—so easy of execution and so fertile in success as he states it to be, does not exhibit more abundant results.]

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)

Physician to the Metropolitan Free Hospital, late Physician-Accoucheur to the Western General Dispensary.

I. Diseases of Unimpregnated Women.

1. *Synopsis of Thirty Cases of Ovariotomy.* Performed by Washington L. Atlee, M.D., Philadelphia. (Amer. Jour. of Med. Soc. April, 1855.)
2. *A Case of Removal of the entire Body of the Uterus by the large Abdominal Section.* By E. R. Peaslee, M.D., Maine. (Amer. Jour. of Med. Soc. April, 1855.)

1. Dr. Washington Atlee gives a summary of all the cases, thirty in number, in which he has performed the operation of ovariotomy. Such an assemblage of

facts constitutes a valuable contribution to the history of this subject. We can only give a condensed view of the leading facts.

Case.	Died.	Recovered.	Tumour found.	Operation completed.
1.	Sixth day—peritonitis	—	Double ovarian	Yes.
2.	— — — —	Yes	Fibrous extra-uterine	Yes.
3.	— — — —	Yes	Ovarian, fibrous	Yes.
4.	Six months—erysipelas	—	Uterine	No.
5.	— — — —	Yes	Cystiform	Yes.
6.	In 3½ years, from progress of disease	Yes	Uterine	No.
7.	Thirty-nine days after, of cholera, caused by eating duck	Yes	Extra-uterine, fibrous	Yes.
8.	Sixth day—peritonitis	—	Cystiform	Yes.
9.	Third day—exhaustion	—	Cystiform	Partly.
10.	Third day—exhaustion	—	Cystiform	Yes.
11.	— — — —	Yes	Uterine	No.
12.	— — — —	Yes	Cystiform	Yes.
13.	— — — —	Yes	Cystiform	Yes.
14.	Pregnant two months at time of operation; died in 30 days of exhaustion	—	Double cystiform	Yes.
15.	Third day—peritonitis	—	Cystiform	Yes.
16.	Third day—hæmorrhage	—	Extra-uterine, fibrous	Yes.
17.	— — — —	Yes	Extra-uterine, fibrous	No.
18.	— — — —	Yes	Cystiform	Yes.
19.	Thirteen hours—exhaustion	—	Cystiform	Yes.
20.	Nine hours—exhaustion	—	Ovarian	Yes.
21.	Third day—peritonitis	—	Three fibrous extra-uterine	Yes.
22.	— — — —	—	Cystiform	Yes.
23.	Twenty-two days—gangrene of jejunum	—	Cystiform	Yes.
24.	— — — —	Yes	Cystiform	Yes.
25.	Fifth day—exhaustion	—	Cystiform	Yes.
26.	— — — —	—	Cystiform	Yes.
27.	Fifth day—hæmorrhage	—	Fibrous and cystiform, both ovaries	Yes.
28.	— — — —	Yes	Cystiform	Yes.
29.	Sixth day—hæmorrhage	—	Cystiform	Yes.
30.	— — — —	Yes	Cystiform	Yes.

From the tabular statement we have thus constructed, it appears, that out of 30 cases in which the operation *for ovariotomy* was undertaken, ovarian tumours were found in 22 cases, and fibrous tumours of the uterus in 8 cases; that in 4 cases the operation was abandoned, in as far as related to the object of the operation—the removal of the tumour; that 13 cases only recovered with life, in 2 of these the tumour being left behind; that in 17 cases the patient died, at intervals of from nine hours to thirty days after the operation, with the exception of one that survived six months. The causes of death were—in 3 cases, hæmorrhage; in 6, exhaustion; in 4, peritonitis; in 1, gangrene of jejunum; in 1, cholera from indigestion; in 1, erysipelas. It is right to state, that in some cases the author attributes the death to causes independent of the operation. In 7 cases the operation was undertaken under desperate circumstances, and with the view of arresting impending death; 5 of these died; 2 survived.

2. Dr. **Peaslee**'s case of removal of the uterus by the large abdominal section, although fatal, as might have been anticipated, we record on the same principle that led the author to publish it—namely, because "unfavourable cases are just as valuable as the favourable ones." He who points out a rock to be shunned, renders a service to mankind.

The patient, a widow, aged 35, the mother of four children of good conformation, applied to Dr. Skinner in April, 1853. There was a tumour in the right iliac region, considered to be ovarian. Her last labour, seventeen months previously, had been tedious, and followed by inflammation of pelvic organs; it was several weeks before she could walk. The tumour was first noticed soon after confinement, and increased rapidly. When the bladder is distended, the tumour will rise above the umbilicus; and on evacuating the former, it will fall as low as the pubes. Examined by Dr. Peaslee, in August, 1853, patient was confined to her bed. The tumour then was about four inches across, and reached to within one inch of the umbilicus; firm, round, smooth; perfectly movable; not sensitive under pressure. During this examination the bladder was full; this emptied, the tumour fell so as hardly to appear above the pelvis. The uterine sound passed with great difficulty, but on reaching the uterine cavity, passed at once to a distance of three inches and a half. The diagnosis was—a tumour commencing in the right ovary, and not yet adherent. The patient insisted upon its removal, and some improvement having taken place in her health, Dr. Peaslee performed the operation in September following. The tumour exposed, was felt to be fluctuating, and a trocar was thrust in, but only a few drops of blood escaped. The tumour, drawn out through the incision, *was now found to be the uterus.* The abandonment of the operation was contemplated, but the uterus was bleeding freely by the puncture from the trocar. It was therefore determined to remove it. This was done, along with the left ovary, which was diseased. Ligatures were applied so as to prevent hæmorrhage, when the necessary incisions were made through the broad ligaments.

The patient survived till the fifth day.

Autopsy.—The right rectus muscle was found in a gangrenous condition. The original incision had completely united, except in two places, where the intestine had formed herniæ. The intestine was not strangulated, but livid. Four ounces of bloody serum in pelvis; peritoneum there of a livid hue.

The tumour.—weight, nineteen ounces; length, five inches and a half; width, four inches; thickness, three inches. In size and shape it resembles a uterus enlarged by impregnation. The lower part being slender, all the part above can be easily *flexed upon that in every possible direction.* The two halves not symmetrical. On section, there was found a fibrous growth, already softening and degenerating in its central portions.

This case, illustrating forcibly the uncertainty of diagnosis of ovarian tumours, may be added to the thirty preceding cases of Dr. Atlee.

II. GESTATION: FŒTAL PHYSIOLOGY AND PATHOLOGY.

1. *On the Rhythm of the Heart of the Fœtus.* By Dr. FLEETWOOD CHURCHILL. (Dublin Quarterly Journal. May, 1855.)
2. *Blighted Fœtus at the Fifth Month; Retained and Expelled with a Living Child at Full Term.* By W. W. BELT, M.D. (American Journal of Medical Science. April, 1855.)
3. *Small-pox occurring to the Fœtus in Utero.* By R. AULSEBROOK, Esq. (Lancet, vol ii. 1854.)
4. *Small-pox in Utero.* By FRANCIS J. OSBORNE, Esq. (Lancet. May, 1855.)
5. *The so-called Fatty Degeneration of the Placenta.* By Dr. HELFFT, of Berlin. (Monatsschr. für Geburtsh. Marz, 1855.)

1. Dr. CHURCHILL has contributed some valuable experimental observations to the physiology of the fœtus. The observations referred to were all made at the termination of pregnancy, and were all in the first stage of labour. He believes that the rhythm of the fœtal heart is different from that of extra-uterine life. He submits the following conclusions:

(1.) That the pulsations of the fœtal heart range from 110 to 160 per minute, the average being somewhere about 136, and the audible sounds double, therefore ranging from 220 to 320.

(2.) That, of the two sounds, the first is the weaker and less distinct; the first audible only within a short distance of the fœtal heart; the second, over a considerable extent of the uterine tumour.

(3.) That the rhythm may be expressed by dividing the entire period of a pulsation into four parts, and placing a dot under the figures, according to the succession of the two sounds, as $\underset{.}{1}$, $\overset{.}{\underset{.}{2}}$, 3, 4, and an accent over the louder sound.

(4.) That immediately after birth, the first and second sounds of the heart become equally loud and distinct from an increase in the first sound.

(5.) That the rhythm changes, and may be expressed thus—$\underset{.}{1}$, 2, $\underset{.}{3}$, 4.

(6.) That this peculiarity of the rhythm continues for about a year and a half, and then gradually changes to that of the adult, expressed thus—$\underset{.}{1}$, $\underset{.}{2}$, 3, 4, with the first sound stronger and louder than the second.

2. The case of Dr. **Belt** is of great physiological interest. He was called, on the 23rd of January, 1852, to see a girl supposed to be threatened with abortion. She states that the waters had escaped; and there was slight hæmorrhage and strong pains. Abortion did not occur. She soon recovered her usual health. On the 21st of May following, Dr. Belt delivered her of a healthy child. On tracking for the placenta, he came in contact with a dead fœtus of five months.

3. (The case of Mr. Aulsebrook is of great interest, as tending to show that protection by vaccination of the mother does not insure immunity from small-pox to the fœtus in utero; and that the organism of the fœtus, however closely dependent for nutrition upon that of the mother, is so far distinct as to have a pathology of its own.)

Variola existed in Bierbon, near Aylesbury, in November, 1833, contiguous to the residence of Jacob W——. His son, who had never been vaccinated, caught the disease, the eruption appearing on the 30th of November. Mrs. W—— and all her other children were vaccinated on the 1st of December. Mrs. W—— had but one vesicle; but there is now (June, 1854) a tolerable scar visible. Fourteen days before her confinement, Mrs. W—— states that she remembers most distinctly on that day that a most nauseating and depressing effect was produced in her by the odour of one of the stools of her variolous son, whom she was nursing; and on the 4th of January, 1854, five weeks minus one day after her vaccination, she was delivered. The infant, examined immediately after birth, showed numerous spots on different parts of the body. On the following day the eruption was more out: each pustule had a distinct red inflamed base, was depressed in the centre, and had unequivocally the true variolous character. The child was feeble, did not suck, and died on the third day. The mother had herself no illness.

4. The case of Mr. **Osborne** deserves to be placed in contrast with the preceding. In the beginning of September, 1854, Mr. Osborne attended Mrs. R—— for distinct small-pox. She went regularly through the disease. She was then in the seventh month of her first pregnancy. In the following November she was delivered of a strong, healthy male child. In due time it was vaccinated, and went through the regular stages of the disease. The mother had been vaccinated when young.

(These cases show that: 1st. The fœtus may take the small-pox through the maternal blood, the mother remaining unaffected. 2nd. The fœtus may remain unaffected, notwithstanding the progress of small-pox in the mother.)

5. The memoir of Dr. **Helfft** will be considered by the Reporter in the continuation of his articles on "Diseases of the Placenta."

III. Labour.

1. *On Artificial Premature Labour by means of Injections into the Uterus.* By Dr. Riedel, of Berlin. (Monatsschr. für Geburtsk. Jan. 1855.)
2. *Two Cases of Artificial Premature Labour brought about by Injection into the Cavity of the Uterus.* By Dr. Cohen. (Monatsschr. für Geburtsk. Jan. 1855.)
3. *Case of Labour with Occlusion of the Os Uteri.* By Dr. Schweitzer, of Militsch. (Monatsschr. für Geburtsk. Feb. 1855.)
4. *Cæsarian Section on a Woman dead of Apoplexy: Extraction of a Living Child.* By Dr. H. Walther. (Monatsschr. für Geburtsk. May, 1855.)
5. *Death from Inhalation of Chloroform during Labour.* Anonymous. (Med. Times and Gazette. April 14, 1855.)

1. (No methods of inducing premature labour have been brought forward under more powerful recommendations than those by intra-uterine and by vaginal injections. It is desirable to accumulate all the experience possible, in order to determine with precision the advantages and disadvantages of these proceedings. Dr. Riedel specially reviews the history of the intra-uterine injection. He adds two cases of his own; and two more from Dr. Cohen will be found in the next paragraph.) Dr. **Riedel** assigns the first idea of the operation to Schweighaüser, who proposed it in 1825, but does not appear to have practised it. But Schweighaüser's proposal remained unnoticed until Dr. Cohen of Hamburg, in 1846, brought forward in the 'Neue Zeitschrift für Geburtskunde' "A New Method of bringing about Artificial Premature Labour," by means of injections of tar-water into the uterine cavity, having probably no recollection of Schweighaüser's proposal. Dr. Cohen illustrated his proposal by the relation of a successful case. In 1829, Dr. Ortwin Naegele* related a case in which labour had quickly and successfully followed intra-uterine injection. Scanzoni, in his 'Lehrb. der Geburtskunde,' 1852, announced himself as a decided opponent of this method, and advocated, as more safe, the method of Kiwisch, which consists in irrigating the vaginal portion of the uterus without introducing a tube through the cervix into the uterine cavity, as Cohen recommends. Harting, in 1853, proposed an alternative resort to these two methods. In October, 1853, Dr. Sack related a case illustrating strongly the efficacy of intra-uterine injection. In November, 1853, there was published in the 'Monatsschrift für Geburtskunde,' a paper by Dr. Cohen, entitled "Some Improvements of his Method of Exciting Premature Labour." He had removed some defects in the apparatus. He now recommended a clyso-pump with flexible tube, with a smaller pewter or elastic tube of four inches in length screwed on to the first; this last being for two inches and a half from the point only of one and a half—two lines in diameter, and gradually from this part expanding to nine lines in diameter; it has an opening at the extremity, or, better, four side openings like a catheter. For the more convenient introduction of this tube (cervical) he provided a stem of six inches long to screw on to it, and which might after introduction be unscrewed. The apparatus *in situ*, twenty-four ounces of injection are to be thrown up slowly and uninterruptedly, or until a feeling of stretching in the abdomen is felt, or until the fluid returns. The proceeding is to be repeated in fifteen minutes or two hours. Dr. Cohen adduced 17 cases in which his method had been adopted: 10 by his colleague, Dr. Steetz; 1 by Pontomier; and 6 by himself. The result of all these, he said, was certainty and quickness of operation, safety for mother and child, and exemption from pain. In Dr. Steetz's account of his 10 cases is seen that in 3 cases three injections were made; in 1 case, four injections; in 2, seven injections; and of the remaining 4 cases, nine, ten, twelve, and sixteen injections were required. Only warm water was used. From the commencement of the injections until completion of labour, intervals occurred of thirty-six hours in 1 case; of fifty-six hours in 1; of two

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* Med. Vereins-Zeit., No. 36.

days and a half in 2; of four days in 1; of five days in 1; of six days in 3; of eight days in 1. Dr. Riedel details two cases from his own practice at great length. Labour was brought to successful termination after two injections and five hours in the first case. In the second, after sixty-three hour and a half, and six injections.

2. In the first of the two cases related by Dr. COHEN, two injections of tar-water were used, and labour was completed successfully for the mother in eighteen hours and a half. The child was born apoplectic. In the second case one injection sufficed, and delivery was completed in twenty hours. The mother and child did well.

Dr. Cohen makes an observation on the *modus operandi* of his method. He believes that the injection does not act by detaching the chorion from the decidua, but through the imbibition of the injected fluid by the surface of the body and fundus of the uterus. In the first case related, he says that he felt—having one hand applied over the uterus externally—the injected fluid spread itself throughout the uterus up to the fundus. No injection is effectual if the fluid does not penetrate into the cavity of the uterus, and is not retained there.

Dr. Cohen maintains, that the great merit of his method consists in the certainty of result, and, by repeating the injections at short intervals, when desired, the power of hastening the delivery at will.

3. A woman was delivered of a first child by perforation; from that time, fæces partially passed through the vagina. Twelve years afterwards she was in labour for the second time. Pains having lasted many hours, examination made: a recto-vaginal fistula was felt; but an os uteri could nowhere be found. But after repeated examinations, and during pains, there was at length discovered, by finger and ocular inspection, a *hard, thick, radiating scar*, of about an inch long, in the inferior segment of the uterus; out of this there spurted at every pain a fine stream of water, by an orifice the size of a pin-hole. The pelvis was also contracted. Strong pains continued four days without opening the os. A crucial incision was therefore made by inserting a Pott's fistula-knife, guarded up to near the point, within the scar. A seven-month child was extracted, the feet presenting. Placenta followed immediately. But trifling loss of blood ensued, and the patient recovered favourably. There is no doubt that the obliteration of the os was the result of injury during the first labour.

4. A woman, aged 39, of sanguineous temperament, often suffering from hysterical symptoms, had borne several living children, and had had many abortions. In her last pregnancy, an inconsiderable metrorrhagia came on after an hysterical attack in the third month. Later, she suffered from headache, which passed off, leaving her in good health down to the sixth month. Then violent cough seized her, with hæmoptysis; this lasted five days. She went on well until the thirty-eighth week, when, after violent mental commotion, headache set in, then vomiting, and convulsions, which lasted seven hours and a half. The movements of the child were felt immediately before her death; so the Cæsarian section was performed directly after the last breath. A living child was extracted, asphyxiated. It was restored by hot and cold baths, &c., but died eight hours after birth.

5. Some authors who advocate the use of chloroform in natural labour, appear to argue that parturient women are more especially secure from fatal accidents than patients undergoing surgical operations; it has often been urged that no woman in labour had been known to perish from taking chloroform. A case is now, however, reported, on the authority of the editor of the 'Medical Times and Gazette,' in which death so caused has undoubtedly occurred. The patient had borne children previously. The labour was quite natural. The chloroform was administered by the nurse at the desire of the patient, the proceeding being concealed

from the medical attendant, who, when sent for into the room, found the patient dead. It is proper to admit that the chloroform was, in all probability, not administered with proper care and knowledge. Five drachms were probably consumed. The mode of application was by handkerchief. Anæsthesia was no doubt carried far beyond the degree recommended and practised by medical practitioners. But the case is not the less an instance to show that parturition confers no immunity against the poisonous action of chloroform.

IV. DISEASES OF THE PUERPERAL STATE.

1. *The Recent Epidemic of Puerperal Fever in the Dublin Lying-in Hospital.* By Dr. M'CLINTOCK. (Dublin Quart. Journ. May, 1855.)
2. *Account of an Epidemic of Puerperal Fever of a Septic Character in Brakel, District of Minden.* By Dr. DISSE. (Monatsschr. für Geburtsk. Feb. 1855.)
3. *Contagiousness of Puerperal Fever.* By M. DEPAUL. (L'Union Médicale. March 3, 1855.)
4. *Scarlatina Puerperalis.* By Dr. A. CLEMENS, of Frankfort. (Monatsschr. für Geburtsk. Feb. 1855.)
5. *On the Use of Ipecacuanha in Metritis and Uterine Hæmorrhage following Labour.* (Gazette des Hôpitaux. March 10, 1855.)

1. The very interesting report of Dr. M'CLINTOCK on "Puerperal Fever," that has recently appeared in the Dublin Lying-in Hospital, is too replete with facts to admit of condensation; but the opportunity of presenting the leading features of epidemics of the same disease in other parts of the world renders us desirous of giving such a summary of Dr. M'Clintock's observations as may serve for comparison.

This epidemic broke out in the first week of last December, and subsided in the middle of February. It was not sudden, inasmuch as twelve or fourteen cases of puerperal peritonitis and phlebitis, together with a few isolated cases of typhus and scarlatina, had occurred in the house during the preceding nine months. During the period covered by the epidemic, 182 women were confined in the hospital; of these, 38—that is, 1 in every 5—were unequivocally affected with symptoms of the disease: of these 38, 17 recovered, 21 died. In three cases the puerperal disease was complicated with scarlatina. Two of them died, one made an excellent recovery, though the metritic attack was a marked one, and the scarlatina very severe, showing itself so early as the second day after delivery. On two occasions this woman seemed to owe her preservation to the liberal exhibition of wine and brandy, and this, too, at the very time when we had every reason to fear the existence of uterine inflammation.

Symptoms and Course of Disease.—In many cases, including some of the most malignant, there was *no initiatory rigor.* In some cases, from the onset to the termination, intense abdominal pain was a prominent symptom; in others, equally fatal, *no* complaint of the belly. Vomiting in some cases appeared early and was constant; in others not at all, or only at the close. Tenderness of the uterus on pressure, with perceptible augmentation of its bulk, was almost invariably found from an early period of each case. The first approaches, when not ushered in by rigor, were sometimes slow and insidious. The only deviations from normal convalescence being a trivial acceleration of pulse, and a slightly furred state of tongue, with, perhaps, diminished secretion of milk. The patient's own representation of her state could not always be relied on, owing to her unconsciousness of the presence or absence of the malady; and this although apparently in full possession of her mental faculties.

Guided by the experience of this epidemic, Dr. M'Clintock is disposed to regard the state of the tongue as a more reliable prognostic than any other *single* symptom. With only one or two partial exceptions, he never saw a patient recover whose tongue had become dry, or brown, or glazed: this symptom was not absent in any fatal cases.

Diarrhœa was present in most cases. Fulness of the belly, with tympanitis, to a greater or less extent, was almost universal.

Cerebral disturbance was rare, but four cases were exceptions. Two women, some hours after the first appearance of the disease, became quite lethargic, and remained closely bordering on coma till their death. Another patient was not less excited, and with difficulty restrained from getting out of bed; she had noisy delirium. Thus these women exhibited in a marked degree the same morbid appearance, putrescence of the interior of the uterus and sloughing of the vagina. The fourth became manifest three or four days after development of puerperal fever; she, however, regained her reason, and recovered. There seemed to exist a strong tendency to putrescence or sloughing of the uterus and vagina, and this quite irrespective of the length or character of the labour. In six cases there was direct proof of this gangrenous condition; two of these were patients that recovered, and had sloughing of the vagina.

In every instance the pulse was very rapid. At the commencement it was rarely below 112; as the case went on it rose to 130, 140, and even 150. It was invariably soft and yielding, "liquid or undulating."

Not a case of trismus or convulsions occurred amongst the children during the epidemic.

Several cases of the disease had occurred in the town before its appearance in the hospital.

The virulence of the disease was greatest at the outbreak: the first 7 patients perished.

Two were attacked in three hours from completion of labour; 1 in four hours; 1 in twelve; 1 in fourteen; 1 in seventeen; 1 in twenty-two. All these terminated fatally. One was seized in twenty-two hours after delivery, and 1 in twenty-three; the former recovered, the latter died. Twelve were affected on the second day, 6 of whom died; 10 on the third day, 3 dying. In other cases the date of accession could not be fixed.

Exactly one-half the cases were primiparæ.

With reference to the question of *contagion*, nothing definite was observed.

Treatment.—Bleeding appeared to be inadmissible. Tried in 2 cases, death followed, the disease seeming to be wholly unaffected, if not aggravated. Local bleeding Dr. M'Clintock speaks of more favourably: most of the cases that recovered were leeched over the hypogastrium at the very beginning of the attack, as it appeared, with decided benefit. Epithems of spirit of turpentine, hot-water fomentations, linseed-meal poultices, and hot salt, were used externally with advantage. Mercury was tried in a large proportion, in various doses, but Dr. M'Clintock cannot say that any decided improvement was traceable to its specific action. Rectified oil of turpentine was largely employed, but in only 2, or at most 3, cases did it seem to be productive of benefit, and in all these wine was given at the same time. In 1 case opium was given to narcotization, with amendment. Wine was allowed in all cases, and in some from a very early period. On no occasion did Dr. M'Clintock regret its free use. He sums up his conclusions as to treatment thus: "Leech promptly; purge actively; stimulate freely:" always presuming that an epidemic similar in character is to be dealt with.

Morbid Appearances.—Some cases presented intense peritonitis, others phlebitis, and a few putrescence of the uterus, and these either separately or conjointly. In one case the interior of the uterus and vagina presented one continuous slough. This case was very rapid, and from the first was of a marked adynamic character, attended with delirium.

2. Dr. Disse's account of an epidemic of puerperal fever is a contribution of unusual value. The disease occurred in a small community, and under circumstances that permitted the narrator to take a comprehensive view of the sanitary condition of the whole population, and the puerperal history of every woman delivered during the period of interest. He was thus enabled to give an unbroken history of a particular epidemic.

Brakel is a town containing 3000 inhabitants; it is one of the healthiest in Westphalia; and, hitherto, epidemics have been rare. It is surrounded on two sides by the streams Nethe and Brucht, which unite below the town to pour their waters into the Weser. The soil is clayey.

The epidemic appeared on the 15th September, 1852, and lasted throughout October, November, December, and ended about the 11th January, 1853. At the time of the outbreak, bilious diarrhœa prevailed in Brakel and the neighbourhood. The lying-in women were free from this. From the 15th September to the 11th January, 28 women were delivered in Brakel, of which 13 were attacked with the fever: 12 died; the remaining 15 remained healthy. All the women attacked had, with one exception, boys, who, two only excepted, died soon after birth, with convulsions.

The epidemic ran the following course:—The first case occurred on the 15th of September in a healthy multipara, aged 36; she was taken on the third day after delivery, and died on the fifth. 2. 20th of September, a healthy multipara: seized the fourth day, died the seventh. 3. 7th of October, a multipara: seized on the third day, died on the fourth. 4. 8th of October, a strong, healthy multipara: seized on fourth day, died on the eighth. 5. 10th of October, multipara: seized second day, died fourth. 6. 13th of October, multipara: seized second day, died fourth. 7. 15th of October, multipara: seized third day, died on the eighth. 8. 18th of October, primipara: seized third day, died seventh. 9. 27th of October, primipara: seized fourth day. This was the only woman who recovered. Herself and child did well. 10. 30th of October, multipara: seized second day, died seventh. 11. 2nd of November, multipara: seized second day, died sixth. 12. 9th December, multipara: seized second day, died seventh. 13, and last, 11th of January, a primipara: seized second day, died third.

The 15 other women delivered within the same period, and who escaped the epidemic, were delivered on the 19th and 23rd of September; 16th, 17th, 25th, 27th, and 28th of October; 11th, 16th, 18th, 21st, and 25th of November; 8th, 17th, 30th of December. Of these, 9 bore girls and 6 boys.

All the 28 labours were natural.

Symptoms.—On the second or third day a strong shivering-fit appeared, followed by great heat and unquenchable thirst. Soon after this, burning and cutting pains came on in the region of the uterus, increased by pressure, spreading towards stomach; great tympanitis. Tongue in the middle moist, sides red and dry. In most cases, nausea and even vomiting; in many cases, constipation; in some, diarrhœa of mucous and offensively-smelling masses. Pulse from 120 to 130: feeble, irregular, compressible. Respiration oppressed. Heart-beat tumultuous. Urine in some retained; when passed, red, turning turbid. Skin dry (calor mordax). Perspiration, milk, and lochia arrested. All evinced anxiety of countenance, a yellowish-green complexion, features distorted. All, at beginning, had unclouded consciousness, but with feeling of a heavy sickness. Towards the end consciousness was disturbed, meteorism reached its greatest point; and the pulse attained its greatest frequency.

Autopsy of case 11, *thirty hours after death.*—Abdomen gave vent to an immense volume of stinking gas, and a thin brown offensive pus flowed from the peritoneal sac. Stomach distended with gas; on the mucous membrane a grey, slimy, stinking mass; the duodenum and small and large intestines showed strong vascular injection, of a dark-brown or blackish colour, and distended with air; between the convolutions of the small intestine a great quantity of a bloody serous exudation, but without any fibrinous coagulation. The mesentery was deeply injected, and of blue-black colour. Peritoneum in similar condition. Liver, large, deep-black, full of blood; parenchyma, soft, lacerable. Kidneys, same character. Uterus, large; peritoneum, very vascular, dark-black; broad ligaments, same; ovaries, same; also Fallopian tubes. Inner surface of uterus covered with a chocolate-coloured, slimy, stinking exudation; os internum deep-black; nowhere coagulation; muscular substance soft, lacerable. Its vessels contained no pus, as in metro-phlebitis. Vagina in same condition as uterus. In the iliac veins was found slimy, stinking, chocolate-coloured exudation.

Etiology.—Dr. Disse thinks it connected with the epidemic of bilious diarrhœa which caused a similar blood-poisoning in the puerperal women. It could not be caused by any dissection poison, as observed by Semmelweis in Vienna. The first patient was examined by no one who could from any source be so infected.

Treatment.—The septic character of the disease precluded antiphlogistics. Turpentine, camphor, and ipecacuanha were used. The patient that recovered was treated with turpentine, internally and externally.

3. M. **Depaul** believes that puerperal fever, especially when epidemic, is contagious. He relates the following examples. During an epidemic of puerperal fever at the Maternité, a midwife was entrusted with the case of a woman recently delivered, affected with a most severe metro-peritonitis. One morning this midwife, in giving the attention to the patient which her situation required, was powerfully impressed, and as if suffocated, by the emanations which escaped on raising the bed-clothes. The same evening a strong shivering fit occurred, her abdomen became very painful, pulse small and frequent, greenish vomiting, diarrhœa; at last all the symptoms most characteristic of puerperal fever. She died in forty-eight hours. At the autopsy the changes usually observed in cases of this nature were found, the tissues of the uterus being unaltered. M. Depaul was enabled, moreover, to establish that this young woman was not only not in any form of puerperal state, but that she presented all the signs of virginity.

A physician was engaged in making the post-mortem examination of a woman who had died of puerperal fever, when he was summoned to attend a labour. Precautions of every kind, change of clothes, washing, could not rid him of the smell that autopsies of this kind commonly leave on the hands. The labour took place in the usual way, but in the evening the patient was seized with a most severe puerperal fever, and died the next day. M. Depaul relates also another similar case in which the woman died in a few hours.

4. Dr. A. **Clemens** relates two cases of scarlatina in puerperal women. Both occurred during an epidemic of benignant scarlet fever. Case 1. A woman in her ninth month, just prior to her delivery, nursed her child, aged seven months, who had taken the fever. She was delivered of twins after severe labour. On the tenth day an intense eruption of scarlatina broke out, attended with fever. The most marked symptoms were cerebral, and shivering fits, followed by exacerbations about the ninth day of the fever. The abdomen was soft, and free from pain on pressure. Bleeding, calomel, antimony, were freely used. The patient recovered. Case 2. A delicate primipara was delivered by forceps on the 24th of March. On the 26th, after shivering and heat, scarlatina appeared. The fever was intense; the head, as in the first case, severely affected; the throat moderately. She recovered. Dr. Clemens observes that in both cases the fever broke out between the second and third days after delivery; the milk and lochia persisted; the head was most severely affected; the period of greatest danger was between the eighth and ninth days; great prostration remained for some months; weakness of head was felt for a long time; they lost their hair almost completely, but this returned again luxuriantly. Referring to a memoir by Helm on 'Puerperal Scarlatina,' he observes, that in both cases, it happened as Helm had described it, that the eruption broke out suddenly and with great violence.

(In a case observed by the Reporter last autumn, the scarlatina did not appear to be much more severe than usual, or to be materially modified by the puerperal condition.)

5. Ipecacuanha has long been usefully employed in various morbid puerperal conditions. M. Legroux is said to resort to it with almost uniform success at the Hôtel Dieu, in sub-inflammatory and congested states of the uterus following

delivery. A case in the services of M. Valleix, at the Pitié, is instructive. A young woman, aged 22, suffered under sub-acute metritis with hæmorrhage, following upon premature delivery: the uterine neck was bulky, elongated, and directed backwards; of normal consistency, slightly patulous, and emitting an albuminous discharge; anterior lip red, covered with granulations. The body of the uterus of full size, movable, and anteverted. According to M. Valleix the anteversion was the result of inflammation: he rested this conclusion on the flooding and almost constant sign of metritis. Fever and a saburral condition supervened, which led M. Valleix to order ipecacuanha in repeated doses as an emetic. But another unexpected event resulted. The flooding entirely ceased, and the leucorrhœa had sensibly diminished. A few weeks of rest and regimen completed her recovery. The inflammation was resolved, and the anteversion was removed.

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.

By W. B. Kesteven, Esq., F.R.C.S.

I. Medico-Legal Psychology.

Medico-Legal Evidence in Cases of Insanity.—We were prepared to have laid before our readers an analysis of Dr. Winslow's lucid exposition of this very difficult subject, as contained in his Lettsomian Lectures, published in the 'Psychological Journal.' The task has, however, to a great extent, been taken out of our hands, and has been far more ably performed by Dr. Laycock, in the preceding number of this Journal.* We would, however, take the opportunity of commending Dr. Winslow's lecture to the attention of our readers. They will therein find the question, "What is unsoundness of mind?" answered upon strictly medical grounds. "It is our duty," observes Dr. Winslow, "to recognise no form of mental unsoundness which is not *positively the product of disease.*" The means of ascertaining this fact, and of stating it in a court of law, are here laid down. Dr. Winslow gives the best advice as to the manner in which the evidence of the medical witness should be given, and illustrates his remarks by apt examples. Dr. Winslow's observations upon the employment and signification of the term "delusion," will be found of service to those unaccustomed to give evidence upon questions of this kind. "No notion of the mind," he states, "however ridiculous, illogical, fallacious, and absurd, should be admitted to be a delusion, or evidence of unsound mind, unless it be obviously and unmistakeably the product of a diseased intellect." The author further shows, by an example, the danger of confounding an illogical and unphilosophical deduction with the conceptions or delusions of a diseased mind.

Dr. Winslow enforces the necessity of caution in signing, and in declining to sign, a certificate for the confinement of an alleged lunatic. The *test* of insanity in all cases should be "the comparison of the mind of the alleged lunatic, at the period of suspected insanity, with its prior, natural, and healthy manifestations;" the consideration "of the *intellect in relation to itself*, and to no artificial *à priori* test." This rule cannot be too closely adhered to. We have very recently had experience of an instance in which its observance saved us from all the consequences and penalties of an incorrect opinion. We were called by a husband to certify to the insanity of his wife, who, at the time of our first visit, certainly seemed to be raving incoherently. On further inquiry and a subsequent interview, we learnt from her ordinary medical attendant that her raving was a very frequent and usual exhibition of uncontrolled temper, and that what we had regarded as incoherence was derived from the trifling nature of the cause of offence. There was nothing whatever unusual in her conduct at the time, however strange it might have appeared when compared with the average standard of ladylike manners.

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* See British and Foreign Medico-Chirurgical Review, Jan. 1855, p. 25.

The 'Lancet' (Feb. 24th) contains an elaborate report by Sir A. Morrison and Dr. Winslow upon the state of mind of a Mr. Greenwood, whose confinement in a lunatic asylum has been made the subject of legal inquiry.

Post-mortem Examinations of the Bodies of the Insane.—We would direct the attention of our readers to the very valuable series of observations, by Dr. Webster, now publishing in the 'Psychological Journal.'

II. OBSTETRIC.

Pregnancy sine Immissione Membri.—Dr. BÖRLEBEN, of Hildesheim, has placed on record an instance of impregnation without admission of the male organ. A young woman, about twenty-seven years of age, had been in labour of her first child forty-eight hours, when he was called to see her by the midwife in attendance. Dr. Börleben found the passage of the child's head obstructed by a strong thick hymen, which on full stretch only presented an orifice of an inch and a half at its greatest diameter. An incision having been made, delivery was speedily accomplished.

Dr. Börleben was informed by the husband that he had never been able to effect penetration. His prepuce covering his glans penis, he suffered acute pain, consequent on retraction of the prepuce, on every attempt to consummate sexual union. No defect as to formation or size of the penis existed.—*Casper's Vierteljahrsschrift*, January, 1855.

III. WOUNDS, &c.

Suicide by Cut-Throat—Threatened Suffocation by a portion of the Tongue falling into the Larynx.—A middle-aged man was taken into Guy's Hospital, having attempted suicide by cutting his throat. He appeared in instant danger of death from suffocation. At the post-mortem examination, the cause of this was found to have been a triangular flap divided from the root of the tongue, and which had fallen over the glottis. The injury had been inflicted with a knife, the point of which the patient had first thrust into his throat, and with which he had made several cuts before he inflicted as much injury as he wished.—*Medical Times and Gazette*, Dec. 16th.

Blood-stains.—At a trial for murder by a youth, eighteen years of age, at Carlisle, on the 23rd of February, Dr. ALFRED TAYLOR's opinion upon the source and date of certain blood-stains upon the prisoner's clothes, &c., furnished a most important link in the chain of evidence. Dr. Taylor was able to distinguish old from recent blood-stains by their aspect, and the chemical detection of rust of iron mixed with the remains of blood on the blade of a knife. Microscopic examination showed the real nature of certain stains on the clothes, alleged to have been caused by the blood of a cow, but which, from the size of the globules, were doubtless human blood.—See Report in *Times*, Feb. 27th.

Tattooing, a Means of Identity.—CASPER, of Berlin,* and HUTIN,† of Paris, have investigated this subject; and M. TARDIEU has now added his researches. The question was, whether tattoo marks ever entirely disappear; it was raised in connexion with the disputed identity of the body of a person who had been murdered. Casper examined the old soldiers at the Hôtel des Invalides, at Berlin, and found thirty-seven individuals among them who presented tattoo marks. Of these, in four instances, no traces of tattooing remained performed thirty-six years previously. In five or six others the marks had partially disappeared. In one instance a tattooing, dating only ten weeks, had been entirely obliterated by inflammation of the skin, consequent upon the operation.

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* Vierteljahrsschrift, Band i. s. 274.

† Recherches sur la Tatouage. Paris, 1853.

M. Hutin's researches had been made upon the inmates of the Hotel des Invalides, Paris. Of three thousand invalides, five hundred and six were found to present tattoo marks. These were grouped as follows by M. Hutin.

1. As to the age at which the operation was performed:—Before 20 years, in 141; from 20 to 30, in 322; from 30 to 50, in 40; from 50 to 75, in 3.

2. As to the seat of the tattooing:—On the arm, in 496; on the hands, in 4; chest, 48; thighs and buttocks, 4; anus, 1.

3. As to colour and persistence:

Of 182 tattooed with a single colour—

	Vermilion.	Gunpowder.	Indian ink.	Writing ink.	Washing blue.	Charcoal.	Black.	Totals.
Distinct . . .	16	32	39	2	1	1	1	92
Faintly . . .	19	10	4	2	—	—	—	35
Partly effaced .	32	10	2	—	—	—	—	44
Entirely effaced	11	—	—	—	—	—	—	11
	78	52	45	4	1	1	1	182

Of 324 tattooed with two colours—

	Two colours mixed.	Red and Black in different parts.	
Distinct	144	7	130
Faintly	28	39	4
Partly effaced	15	58	—
Entirely effaced	—	33	3

In two other cases, tattooed with vermilion, after thirty years the marks had partially, in another after forty years completely, disappeared.

4. Duration:

Distinct (after from 4 to 65 years), in 342.
Partially effaced (in from 10 to 64 years), in 117.
Completely effaced (in from 28 to 60 years), in 47.

M. Tardieu's observations were made upon three hundred and five individuals in the Hôpital la Riboisière, forty-eight of whom were found to have been tattooed. To these are added other instances, making a total of fifty-one individuals. The dates of the marks M. Tardieu found to vary from five to forty-seven years. The age at which most frequently the operation had been performed was from eighteen to twenty-five years. As to the seat of the marks, this was sixty-two times on the fore-arm out of seventy-six tattooings. As to the material and colour:—Of thirty-one distinct single coloured tattooings, twenty-three had been performed with Indian ink, two with blue ink, three with vermilion, &c. Of twenty-six presenting two colours—e.g., Indian ink and vermilion—twenty-five were distinct, twelve partially effaced, one had disappeared.

It is evident, from the preceding statistical results, that, although tattoo marks do not differ essentially from other cicatrices, or other natural or accidental exterior marks, yet that, on account of their varied and bizarre forms, they have obviously a peculiar value in connexion with questions of identity. The existence and character of tattoo marks may serve to indicate the social condition of a deceased person. The images depicted are generally more or less closely related to the calling of the person on whom they are found.

M. Raffer's dissections have shown that the material forming permanent tattoo marks is fixed in the *cutis vera*, the epidermis which covers them not differing from that structure in other parts of the body. In some instances, the colouring matters have been found to have penetrated the subjacent areolar tissue, and to have resisted sphacelus of the integuments. It would appear that such marks are indelible; nevertheless, they have been known to have become obliterated, as shown by Casper and Hutin. This has occurred in the proportion of 1 in 6, according to Casper's statistics; 1 in 11 (Hutin); and 1 in 25 (Tardieu). The disappearance of tattoo marks cannot, therefore, be a mere matter of chance. The disappearance is a natural process, probably not entirely inexplicable. Thus, where two colours have been employed, red and black, the vermilion part has become obliterated, while the black remains. Figures entirely red have also become faint. It may, therefore, be stated as a general rule, that the durability of tattoo depends upon the depth of the puncture and the nature of the colouring matter.

It is important in a practical point of view to bear in mind that tattoo marks may be obliterated by the application of escharotics; but however skilfully these may be applied, they leave traces of their action detectable by an experienced eye. —*Annales d'Hygiène*, Janvier, 1855.

IV. MISCELLANEOUS.

Inquests in the Western Division of the County of Middlesex.—It is a matter of satisfaction that Mr. Wakley has determined upon giving the profession the benefit of his large experience as coroner of so populous a district as is assigned to his charge. It has been ever matter of regret that the vast amount of medico-legal facts constantly accumulating in the records of Coroners' Courts, should have been permitted to run to waste—however, *Sera nunquam est ad bonos mores via.* The greater attention, however, that has been of late years given to the science of Medical Jurisprudence, in this and other journals, has, we suppose, induced the editor of the 'Lancet' to devote a portion of that journal to the especial purpose of preserving from complete oblivion the information which his tribunals can afford to the medical and legal professions. Great would be the advantages to society if other coroners would follow the example thus set by Mr. Wakley.

The subjoined table opens the series of articles promised in the 'Lancet,' under the head of Medical Jurisprudence:

A Table of the Deaths from Suicide in the Western Division of Middlesex in Two Years, from 1852 *to* 1854.

—	Jan.		Feb.		Mar.		Apr.		May.		June		July		Aug.		Sept.		Oct.		Nov.		Dec.		Total.	
	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F		
Drowning	...	1	...	1	...	1	1	1	2	2	1	1	6	1	4	2	1	...	...	1	2	...	...	...	28	
Hanging, &c.	4	1	2	...	3	...	7	1	...	1	5	...	8	2	4	1	1	2	9	...	7	4	7	2	71	
Cut-throat, &c.	1	1	3	...	3	...	2	1	4	...	1	2	4	...	2	2	5	1	4	...	3	...	2	1	42	
Falls	...	...	1	...	...	1	1	1	...	2	...	...	...	...	...	...	2	...	...	...	...	...	...	...	8	
Shot	1	...	...	...	1	...	...	...	...	...	1	...	1	...	1	...	1	...	...	...	1	...	...	...	7	
Burning	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	
Poisoning:																										
Oil of bitter almonds	2	...	...	...	...	1	...	...	...	...	3	...	...	...	2	...	...	...	...	...	...	...	...	1	9	
Prussic acid	1	...	...	1	...	...	...	...	2	...	1	...	1	...	...	1	...	...	...	...	...	...	...	...	7	
Arsenic	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	
Laudanum	...	...	...	...	2	1	...	1	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	5	
Nitric acid	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	32
Corrosive sublimate	...	...	...	...	...	...	1	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	3	
Oxalic acid	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	1	...	2	...	...	...	...	4	
Nux vomica	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	1	
Unknown	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	1	
Males and females	10	4	6	2	10	4	12	5	9	6	13	4	21	3	13	6	10	4	14	3	13	4	9	4		
Total	14		8		14		17		15		17		24		19		14		17		17		13		189	

Apoplexy in Infants.—Among the inquests held by Mr. Wakley, there have, during the last two years, occurred several in which it has been found that infants had died from apoplexy. This is considered a rare cause of death, and the statistics of Rilliet and Barthez, Barrier and West, are cited in support of the opinion. The main features presented by these comparatively infrequent cases are thus stated:—"They resemble each other greatly in their details. The seizures in four out of the above five cases are proved to have taken place in the morning; and in all, except one of the instances, they occurred in bed. It will be remarked, that whereas sanguineous apoplexy in adults is evinced by a loss of motion, in the infantile state it would seem uniformly to be characterized by convulsions, or a morbid exercise of the motor functions. In most of the cases narrated above, the stomach was found filled, or nearly so. Whether the overladen stomach, by pressing on the great vessels, the liver, or the cavity of the chest, and by thus impeding the proper performance of the function of circulation, tends to produce the apoplectic attack; or whether, supposing an overladen stomach to have any influence in the production of the seizure, it operates by occasioning irritation of the nervous centres, as do worms in the intestines, &c., or by withdrawing a due amount of organic nervous energy from the vessels of the brain, and thus conducing to the attack, are questions yet undetermined. In the last case detailed, the food with which the stomach was found filled was a heavy pap, quite inappropriate for an infant of the tender age of two months, and the circumstance of the stomach being completely filled, as it was, with such a material, cannot be regarded as without its influence."—*Lancet*, Jan. 20th.

V. TOXICOLOGY.

Suicide with Oxalic Acid.—Mr. John Southgate, of Finchley, on Sunday, the 17th of December, about ten o'clock at night, was found in a comatose state, and died shortly afterwards. He had gone to bed about eight o'clock, and it was supposed had taken some Epsom salts. An inquest was held, when it was found that the deceased had taken oxalic acid. He had made no complaint, although his daughter had been in his room, and had spoken to him, after he had gone to bed. In his bedroom were found two sealed packages, one that had been opened, and an empty paper of the same description as the sealed packages. These packages bore the label, "Epsom Salts," with the address, "Dr. Neill, 175, Aldersgate-street."

The question to be solved was, where did the oxalic acid come from?

Suspicion in the first instance attached to the assistant of Dr. Neill, who had served the salts to the coachman of the Barnet Mail, who deposed to having purchased them for Mr. Southgate. The evidence of Dr. Neill and his assistant was to the effect, that nothing had been done to alter the condition of the drawer from which the Epsom salts were sold, and that the drawer had been filled from a twenty-eight pound parcel. No ounce packages of salts being ready at the time, four parcels were made up at the moment from this bulk. The same Epsom salts had been sold to many other persons before and subsequently. A small parcel of oxalic acid had been taken into stock, and was kept in a different part of the shop, and the sale of the whole could be traced. It was, moreover, proved by Mr. Lucas, chemist, of Cheapside, that the deceased had purchased half a pound of oxalic acid of him, on the day before his death; but, on inspection, the crystals of this acid did not correspond with those found in the packages in Mr. Southgate's bedroom. We may here state that, before the conclusion of the inquest (which was adjourned six times), the legal adviser of the family of the deceased publicly acquitted Dr. Neill and his assistant from all blame. The point next to be determined was, whether the deceased had committed suicide. The facts sworn in evidence, which might throw a light upon the motives to such an act, were of a most complicated character, embracing sundry money transactions, together with a correspondence with a young unmarried woman who had formerly lived as servant in Mr. Southgate's family.

The verdict of the jury was, that deceased died from the effects of oxalic acid, administered by his own hand while in an unsound state of mind.

We have not space for more than the above outline of this case. Our readers will find all the circumstances detailed in the columns of the 'Morning Advertiser.' We have not succeeded, however, in finding proof of the unsoundness of mind expressed in the verdict. Without losing sight of the disgraceful result of a verdict of *felo de se*, but looking at the inquiry in a medical point of view, we cannot shut our eyes to the weight of moral evidence of deliberate design and well-executed intention involved in the verdict of suicide in this instance.

It is hardly to be believed that a person in the habit of taking Epsom salts, as was deceased, should unintentionally have swallowed above an ounce of oxalic acid without finding out his mistake, and have taken immediate steps to remedy the accident.

No Epsom salts could be discovered mixed with the oxalic acid, either in the stomach or in the paper packages, although chemically analysed and microscopically examined by Professor Taylor and Mr. Scanlan. The Epsom salts must have been entirely abstracted, oxalic acid must have been substituted, the packages must have been unsealed and resealed,—all this must have been effected with design, care, and precaution, which baffled the scrutiny of scientific investigation. Dr. Taylor's report states that one end of a packet submitted to examination appeared to have been closed at one end, as if in haste and carelessly. No trace, however, could be detected of any laceration of the paper, or of substitution of wax of a different character having been used in resealing. The operation, doubtless, could have been performed by the cautious application of heat to the original sealed ends.

The substitution must have been made by measure, and not by weight. Dr. Taylor points out that five drachms of oxalic acid occupy the space of six drachms of Epsom salts. This relation was observable in the unopened package.

Some intent to conceal the deed may possibly be inferred in the fact of his having employed the agency of the coachman to purchase the Epsom salts.

The opinion of Dr. Taylor was, that death was owing to accidental poisoning, for the following reason:

The contents of the stomach and the remaining packages were submitted for analysis to Professor Taylor. The liquid removed from the stomach was of the dark brownish colour usually seen in cases of poisoning from oxalic acid. It was found to contain a small quantity of oxalic acid, and no trace of Epsom salts. It would appear that the poison in this case must have passed off by purging, as there was no evidence that vomiting had taken place. The fatal influence of the poison seems to have been exerted upon the heart and organic nervous system;—deceased was speedily prostrated and rendered unconscious.

We are indebted to the kindness of Dr. Alfred Taylor for the use of his notes of the analyses of the packages produced at the inquest, as well as of numerous samples of oxalic acid obtained from other sources, in order to determine whether sulphate of magnesia exists as an impurity in commercial oxalic acid.

The following results are given by Dr. Taylor and Mr. Scanlan:

		Sulphate of magnesia in 100 parts.
1. Packet opened at inquest by Mr. Wakley	1st expt. on 52 grs.	0·96
	2nd expt. on 100 grs.	0·60
2. Another packet found in Mr. Southgate's room		0·90
3. Sample from the laboratory at Guy's		None
4. Sample from Mr. Rouse	1st expt. 52 grs.	0·76
	2nd expt. 100 grs.	0·60
5. Mr. Brande's sample, Royal Mint		0·40
6. Sample from Davy, M'Murdo, and Co.		1·20 and soda also
7. Sample from Wyman, Dr. Neill's druggist		0·70

A close examination of the crystals found in Mr. Southgate's possession, made by Dr. Taylor and Mr. Scanlan, failed to detect any *crystals* of sulphate of magnesia, although this salt was discovered by chemical analysis. It was thus ascertained that sulphate of magnesia is present as an impurity in commercial oxalic acid, although it is not equally diffused, as will be seen by the results of the analyses, 1 and 4.

With reference to the source of the oxalic acid taken by Mr. Southgate, we subjoin the following extract from Dr. Taylor's report. "It is in my judgment impossible, without some traces, to suppose that any substitution can have been made in the contents of these three packets, without some trace of crystallized Epsom salts being found in it. . . . . . The portion of paper packet which I analysed was one still partly folded and imbedded in the crystals. On analysis, it yielded no trace of the presence of Epsom salts, although this was as favourable a case for detecting it, if present, as any which had occurred."

Dr. Taylor having thus failed to discover any crystals of sulphate of magnesia, or to detect the presence of that salt in greater than the ordinary proportion of impurity to be found in the commercial oxalic acid, concludes that oxalic acid alone was originally contained in the paper packages under examination.

In the face of a scientific opinion entitled to so much weight as was Dr. Taylor's report, the counterbalancing moral and circumstantial evidence must have been heavy indeed to have removed all blame from the parties upon whom it would otherwise have fixed the suspicion of carelessness.

Still more forcibly, however, does it tell for the soundness of mind in the deceased, that he could so skilfully and successfully have practised such an artful imposition, with the object, doubtless, of removing all evidence of *felo de se*. Consideration for the feelings of survivors justified the verdict of *suicide*.

Poisoning with Colchicum.—Dr. Casper relates that four men drank some *Vinum Colchici*, taking it to be bitter *Schnaps*. Profuse purging, pain in the abdomen, vomiting, prostration, were produced, and all died from the effects of the poison. The blood in all was dark-coloured and thick. The kidneys were congested, the bladders filled with urine. The brain also exhibited unusual fulness of vessels. In one case the vessels of the stomach were extremely congested, and its inner surface presented scarlet patches of inflammation. In another, the mucous surface was pale, while spots of ecchymosis were found on its outer aspect. In the other two, slight congestion was found, with traces of inflammation of the bowels. The gall-bladder was variously full, corresponding with the difference observed during life as to vomiting. Chemical analysis separated *colchicin* from the contents of the stomach and intestines, in about the quantity of from two-fifths to one-half a grain; a quantity supposed to have been sufficient, in Herr Schacht's opinion, to have produced the fatal result.—*Casper's Vierteljahrsschrift*, January, 1855.

Death from Chloroform.—Hannah B., aged fifty-six, was admitted into Guy's Hospital on the 15th of November, 1854, under the care of Mr. Birkett. She described herself as in good health, but presented a most cachectic appearance, edentulous, and appeared perhaps ten years older than the age assigned. She complained of chronic ulceration of her left leg for the last two years, the ulceration appearing to surround the leg at about the junction of the middle and lower thirds. Two months before admission into Guy's, the ulcer assumed a sprouting, fungating aspect, became very painful, and during the last month it bled frequently and profusely. In the left groin there was an enlarged gland. Various means having been ineffectually employed to destroy the cancer, amputation below the knee was proposed, and agreed to by the patient. The woman had a little wine and water, according to a recent suggestion, before taking the chloroform.

At one o'clock P.M. she was brought into the operating theatre, and chloroform administered in the usual method at Guy's, on folds of lint. During the inhalation of the vapour, the tourniquet was adjusted to the femoral artery;

it was screwed tightly for a few seconds, to ascertain if it commanded the artery, but, we observed, was immediately after slackened. The inhalation had been continued a minute or two, when considerable muscular movements, so frequent in this stage, took place; these were followed by rigidity. Mr. Birkett was about to operate, when Mr. Callaway, in the capacity of assistant at the operation, called attention to the only remarkable circumstances noted, the pulse stopped at the wrist, swelling of the veins of the neck, expirations performed also with considerable sufflation of the cheeks, and sudden opening of the lips. The poor woman was less than five minutes under the action of the vapour; there had been no vomiting, and particular care was taken there should be no solid food in the stomach. We give these particulars, as we think the working out the problem of the cause of death after chloroform inhalation one of the most interesting perhaps which can engage the profession at present.

The post-mortem, conducted with great care and skill by Dr. Wilkes, revealed a very large amount of organic disease. The external appearance was that of a woman at least ten years older than her real age; hair quite grey; *arcus senilis* of the eyes; body spare, the skin presenting the smooth, yellow appearance too often indicative of senile fatty degeneration. A chronic ulcer, of very bad kind, existed on the right leg, and on the left a large fungating mass. There was the usual amount of blood in the brain; the membranes healthy; the subarachnoid fluid greatly increased in quantity, due perhaps to wasting of the brain; the convolutions were shrunken, but healthy; the ventricles contained an increased amount of serum; one choroid plexus vesicular; the cerebral arteries contained atheromatous matter and bony patches; the medulla oblongata did not exhibit any morbid appearance. The larynx, trachea, and pleuræ were all healthy; the lungs were very much congested with blood, bleeding very freely when cut. The heart was of usual size; *all its cavities empty;* the endocardial membrane of the left ventricle had various patches and streaks of fibroid degeneration; the heart-fibres presented zig-zag lines of fatty degeneration; they were slightly granular, and contained fat, as seen under the microscope; this was more in the right ventricle than in the left; the fleshy columns of the mitral valve were also diseased in the same way. The liver was also diseased. On examining the ulcer on the leg, it was found to be full of cancer-cells, large and nucleated.

This fatal case, like others that have preceded it, forcibly teaches caution in the mode of administering chloroform. We cannot think that the inhaler, if such it deserve to be named, which was here employed is sufficiently precise to ensure safety. Dr. Snow, who has probably administered chloroform under all circumstances, and for all sorts and conditions of patients, has met with only one fatal case. This we do not hesitate to attribute to the extreme care taken by that gentleman to regulate the due admixture of air with chloroform vapour. It is with great satisfaction we see it announced that Dr. Snow is about to submit to the profession his experience on this matter. In the mean time, we would refer our readers to Dr. Snow's remarks published in the 'Lancet,' Jan. 23rd.

Poisonous Properties of Carbonic Oxide.—MM. Lassaigne and Tardieu, in a report upon an instance of death from this cause, quote and state their concurrence in the conclusions of M. Leblanc, which are also illustrated by the case they relate—viz., that an atmosphere charged with three or four per cent. of carbonic acid, disengaged from the combustion of charcoal, may be instantly fatal to a large dog, whereas from thirty to forty per cent. of pure carbonic acid would be required to produce a similar consequence. One kilogramme (=2·204 pounds avoirdupois) of " breeze," or small coal, and still more certainly the same quantity of charcoal in free combustion, will render asphyxiable the air of a chamber of the capacity of twenty-five cubic metres (=27·44075 yards English). Thus, not only is it found that an atmosphere may be rendered unfit for respiration by an apparently small quantity of combustible matter, but the important truth is ascertained, that this noxious property is attributable rather to the presence of carbonic oxide, which, diffused through the air in the proportion of only one per cent., may render it poisonous to warm-blooded animals.—*Annales d'Hygiène*, October, 1854.

Death after Treatment of Cancer with Arsenic.—The following particulars show with what impunity quackery is countenanced in England; how little store is set upon human life, whilst those who hazard it unjustifiably meet with protection rather than punishment:

J. L. Ward was tried before Baron Alderson, at the Winter Assizes, York, Dec. 1854, charged with the manslaughter of Mrs. Lambert, at Bradford.

It appeared that the prisoner was a quack doctor, carrying on business in Wellington-street, Leeds, and professing to cure cancer and similar disorders without the use of the knife. The deceased, having an incipient cancer, applied for advice to the prisoner, who attended her for twenty-eight weeks, when she died; upon which an inquiry took place before the coroner, and a verdict of manslaughter against the prisoner was returned.

William Lambert said—The deceased was my wife. In December, 1853, she complained of a slight pain in the breast. Dr. Field attended her, and my wife declined having it cut. About the second week after Christmas we went to Leeds, to the prisoner's house, where on the door was a plate, "James Lawrence Ward, Curer of Cancer without the use of a Knife." She showed the prisoner her breast, and he said, "It is a cancer." I asked if he could cure it, and he replied that he could. I said I would publish it in the papers if he did. He brought out a small bottle, with a white powder, which he put on a marble slab; then he put some black liquid from another bottle, and mixed them together, and applied the liquid to my wife's breast with a feather. It appeared to give her pain, as if it singed her, and as if it burnt her breast. He put a plaster on, and I paid him five shillings. We then left, and the week following she went to the prisoner again, and returned with a box of salve and pills. The third week we went again, and the prisoner put some white powder on the breast with some lint. This also gave her great pain. The prisoner said that she was going on very well. About this time her appetite failed, and she wasted away. We went to the prisoner for twenty-eight consecutive weeks, and paid five shillings each visit. The prisoner put on the liquid till it ate away the nipple and part of the breast, and the lump she complained of came away also. We then thought all was well, but the sore got worse. The prisoner then said he thought there was another cancer under the old sore. We went for seven weeks after the part of the breast had come away, and then a large lump arose which also came away, and then the prisoner said that he thought all would be right, but she got worse. I went to Dr. Field's for advice about a month before she died, which took place on the 4th of August last.

Medical evidence was taken, to the effect that the treatment described was improper and dangerous to life.

His lordship said that, in his opinion, the treatment of the deceased must be so extravagantly wrong as to amount to *mala fides;* but it was a question for the jury to decide upon.

Mr. Overend then addressed the jury for the prisoner, and contended that the prisoner could not be convicted, unless it could be shown that he was either grossly or wilfully ignorant. Physicians and surgeons had given opinions upon speculations which had been formed upon the treatment which the deceased had received from the prisoner, for they were totally in the dark as to what had been applied. The medical men themselves differed completely upon the mode of treating cancer, and the result was that if Mr. Hey was right in his opinion, Mr. Field ought to be convicted of manslaughter. There was nothing to show that the prisoner had been guilty of gross ignorance, and he had used his best skill for the poor woman, who had refused to have the knife used upon her. Before they could convict the prisoner, they must be satisfied—first, that the prisoner had employed improper remedies; and secondly, that he was guilty of gross negligence or gross ignorance, which had not been established.

. . .

His Lordship, in summing up, said that in order to substantiate the charge against the prisoner, it must be shown that he was guilty of such gross negligence, want of care, or ignorance, that he could not be said to be acting in good faith. The criminal law was not the mode of deciding doubtful points of medical law, and there was a remedy by action at law for want of skill; but when you endeavour to punish a medical man criminally, you must have as your criterion whether he acted *bonâ fide*. If juries acted otherwise, they would have no scientific experiments made, which would be a great injury to the community; and consequently the rule should be very wide for the benefit of the medical profession themselves!!

The jury immediately returned a verdict of Not Guilty.

His Lordship then told the prisoner that after the medical testimony he had heard, he had better take care how he pursued his practice.—*Morning Herald*, December 8th, 1854.

Tartar Emetic as a Means of Preventing Poisoning by Lucifer Matches.—M. CAUSSE proposes that tartar emetic be added to the phosphorus paste employed in making matches. Vomiting would be caused thereby, and the poison consequently be rejected. It does not prevent deflagration, and would not materially augment the price of the matches. It will remain long in the animal organ, where it may be detected after four months. It is not likely to be administered as a remedy in combination with phosphorus. If, therefore, the salt be detected by chemical analysis, at the same time that the quantity of phosphorus discovered be much greater than is ordinarily found in the organism, it would afford strong presumption that death had been caused by chemical matches containing tartar emetic.—*Annales d'Hygiène*, Janvier.

Amorphous Phosphorus as a Substitute for Ordinary Phosphorus in the Manufacture of Chemical Matches. By MM. CAUSSE and CHEVALLIER, fils.—Amorphous, or red, phosphorus is considered by M. Schrötter to be an oxide of phosphorus. It is obtained by subjecting phosphorus in a closed tube to a temperature of 240° to 250° (=528° to 546° Fahr.). It is pulverulent, non-luminous in the dark. When exposed to the air it does not give off the strong odour that is usual from phosphorus. It does not burn at ordinary temperatures, but requires a high degree of heat, when it burns without producing any odour. This amorphous phosphorus is susceptible of ignition by friction, as in ordinary matches. That it does not possess poisonous properties has been shown by the experiments of Lassaigne and Reynal. As, moreover, the necrosis of match-makers is caused by the vapours of phosphorus, the use of the amorphous phosphorus gives exemption from that disease.—*Annales d'Hygiène*, &c., Janvier.

Case of Poisoning with Belladonna. By SAMUEL SOLLY, Esq.—The following case appears to me of sufficient interest to be placed on record:—

W. S., aged sixty, was under my care for chronic rheumatism in the knee-joint. The following liniment, in addition to constitutional treatment, was prescribed:—Soap liniment, five ounces and a half; extract of belladonna and tincture of opium, of each two drachms; infusion of roses, four ounces. To take a fourth part every two hours. This was made up at the Apothecaries' Hall, and labelled for "external use."

On the 13th of January, at half-part seven A.M., his servant poured out two tablespoonfuls of this liniment, which would contain twelve grains of the *extract* of *belladonna*, and gave it to him instead of his usual medicine. He drank it off before he perceived the mistake, and even then attached so little importance to it, that he had his breakfast about half-an-hour afterwards. This meal consisted of kidneys, tea, and bread and butter. He declined seeing any medical man in the country, though urged by his wife to do so. He drank freely of water, but did nothing else. When he came to London, he called on me, about two hours from the time of his taking it.

At this time there were no symptoms of poisoning. I ordered a scruple of ipecacuanha immediately, which he took at my neighbour, Mr. Beale's, following it up with some warm water. This acted as an emetic very freely; the matter vomited smelt of belladonna. After the vomiting, I gave him chloride of mercury and powdered jalap, of each five grains, immediately; sulphate of magnesia, one ounce, and distilled vinegar, one ounce; and sent him home in a cab, Mr. Beale kindly accompanying him. He was there left in charge of Mr. Dukes, of Kingsland, who tells me that all the symptoms of poisoning by belladonna soon appeared.

Great dryness of the throat, difficulty of deglutition, all sorts of delusions, rambling incoherent conversation, dilated pupils, insensible to light, eyes rather prominent, and a vacant stare; muscular power generally feeble; very drowsy; pulse feeble and irregular. He was altogether so low and depressed, that Mr. Dukes thought at first the case was almost hopeless. He gave him some coffee, and kept him walking about.

I saw him again between two and three o'clock. Found him very drowsy, asking earnestly to be allowed to sleep, complaining that he should sink into the earth, and the next minute rambling incoherently. No eruption on the skin of any kind could be seen, though looked for carefully. The stomach having been completely emptied by the emetic, and the bowels not having been opened, I ordered a turpentine enema. This soon acted very freely, and from this time he rapidly recovered. The next day he was quite well, excepting a little dryness of the throat. He told me that his ocular delusions the day before were most extraordinary and fanciful, but that, by shifting the position of his eyes, he could remove them.—*Lancet*, Feb. 3rd.

MEDICAL INTELLIGENCE.

The late Editor of the 'British and Foreign Medico-Chirurgical Review.'

Our readers have been informed by the weekly medical periodicals that the late editor of the 'British and Foreign Medico-Chirurgical Review' has accepted the important and honourable post from Government, to establish and direct a hospital in the neighbourhood of Constantinople capable of receiving 1500 patients from the seat of war. After considerable difficulty, Dr. Parkes has succeeded in finding a site possessing the advantages of a good water-supply and close proximity to the sea, in a beautiful situation seven miles south of the Dardanelles, near a village called Ranqui. In repeating the announcement, we cannot refrain from expressing what we believe to be the opinion of the profession, that the selection of Dr. Parkes for the post will redound to the benefit of our country, and that his labours will add to the achievements which, in the perils of battle and the greater and more harassing toils of the hospital, have rendered so many members of the medical profession worthy of imperishable honours. We feel assured that the great loss to the profession by the removal of Dr. Parkes from the editorship of this Journal will, if it please God to spare his valuable life, be counterbalanced by the results which cannot fail to crown the profound knowledge, the practical tact, the thorough honesty and burning love of his profession, which he carries to the prosecution of his new task.

For ourselves, we approach the responsibility of conducting the 'British and Foreign Medico-Chirurgical Review' with sincere diffidence, but with a full consciousness of the obligations imposed upon us. We shall not be required to make a profession of faith, but of one thing we would desire to assure our readers, that it will not be want of earnest zeal, if, in the position of editor of this Journal, we fail in our duty in aiding to maintain the honour and dignity of the medical profession, and thus in securing for the Quarterly Review the rank which it has held under our predecessors.

The Royal Medical Benevolent College.

The formal opening of the Royal Medical Benevolent College will have taken place before these lines are submitted to our readers; the inauguration will be shortly followed by the election of the pensioners and foundation scholars, who are to occupy the buildings prepared for them in the vicinity of Epsom, at Michaelmas. After much and careful deliberation the choice of head master of the school has fallen upon the Rev. Robinson Thornton, tutor of St. John's College, Oxford—a gentleman whose antecedents in every way justify the high confidence placed in him by the council of the college.

The strongest proof that can be offered of the hold the institution already has upon the profession is, that not only the number of candidates for the foundation scholarships is much larger than the number of vacancies, but that the applications for the exhibitionerships, for which an annual sum of 30*l.* will be paid, considerably exceeds the number (75) calculated upon.

As the Royal Medical Benevolent College is the first institution of the kind which has been reared by the joint efforts of the medical profession throughout England and Wales, no small credit attaches to Mr. Propert, the founder, and those gentlemen who, with him, have brought the undertaking to so successful an issue.

The Crimean Medal.

The following medical officers of the army have received the Crimean Medal at the hands of her Majesty for distinguished services at the battles of Alma, Balaklava, and Inkermann:—Dr. Skelton, Coldstreams; Mr. C. R. Nicoll, Grenadiers; Mr. Macculloch, 5th Dragoon Guards; Mr. Gilborne, Royal Artillery; Mr. Hearn, Royal Regt.; Mr. Crighton, 4th Light Dragoons; Mr. Fyffe, 30th Regt.; Messrs. King and Abbott, 41st Regt.; Mr. Noot, 50th Regt.; Mr. Scott, 57th Regt.; Mr. Flower, 63rd Regt.; Mr. Johnston, 68th Regt.; Mr. Sinclair, 93rd Regt.; Drs. Dumbreck and Forrest, deputy inspectors-general; Messrs. Ewing and Saunders, staff surgeons; Messrs. Darcy, Mulock, Mitchell, and Stewart, staff assistant-surgeons.

Increased Pensions to the Widows of Medical Officers of the Army.

A warrant just issued (June 21st, 1855) from the War Department, fixes the pensions of the widows of the medical officers of the army, and the allowance to their orphans, at the same rate as those of the corresponding military ranks.

BOOKS RECEIVED FOR REVIEW.

On Lateral Curvature of the Spine; its Pathology and Treatment. By Bernard E. Brodhurst, Assistant-Surgeon to the Orthopædic Hospital. London, 1855.

On the Treatment of Cancer by Congelation and an Improved Mode of Pressure. By James Arnott, M.D. Second Edition. London, 1855.

Report of Dr. Burrell on the Plague of Malta in 1813, in the Appendix to the Second Report on Quarantine. (General Board of Health.) London, 1854.

The Micrographic Dictionary. By J. W. Griffith, M.D., and Arthur Henfrey, F.R.S. Parts VIII., IX., X., XI.

The Pathology and Treatment of Leucorrhœa. By W. Tyler Smith, M.D., Physician-Accoucheur to St. Mary's Hospital. London, 1855.

A History of the British Marine Testaceous Mollusca. By William Clark. London, 1855.

An Inquiry into the Statistics and Pathology of Abscess of the Liver. By Edward John Waring, Residency Surgeon at Travancore.

On Electro-Lithotrity; or, the Application of the Mechanical Force of the Electrical Discharge to the Disintegration of Stone in the Bladder. By George Robinson, M.D. London, 1855.

An Expository Lexicon. By R. G. Mayne, M.D. Part IV.

On the Use of Creasote in Scorbutic Camp Dysentery. By J. B. Wilmot, M.D. London, 1855.

Osservazioni Microscopiche e Deduzioni Patologiche sul Cholera Asiatico. Memoria del Dott. F. Pacini. Firenze, 1854.

Elements of Chemistry. By W. A. Miller, M.D., Professor of Chemistry in King's College, London. Part I. Chemical Physics. London, 1855.

Remarks on the Pathology and Treatment of Beriberi. By C. Morehead, M.D., Professor of Medicine. Grant, Medical College. (Reprint from the Bombay Transactions.)

On Stricture of the Urethra and Fistula in Perineo. By James Syme, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh. Second Edition. Edinburgh, 1855.

Pathological and Clinical Observations respecting Morbid Conditions of the Stomach. By C. Handfield Jones, M.B., B.A., Cantab. F.R.C.P., F.R.S., Assistant-Physician to St. Mary's Hospital.

Letter of the President of the General Board of Health to the Right Honourable the Viscount Palmerston, accompanying a Report of Dr. Sutherland on Cholera in the Metropolis in 1854. London, 1855.

Report on the Results of the Different Modes of Treatment pursued in Epidemic Cholera, addressed to the President of the General Board of Health, by the Treatment Committee of the Medical Council. London, 1855.

Illustrations of Medical Evidence and Trial by Jury in Scotland. Edinburgh, 1855.

Die Wirkung des Nordseebades. Von F. W. Beneke, M.D. Hannover, 1855.

Lectures in Reply to the Croonian Lectures for 1854, by Charles West, of London, on the Pathological Importance of Ulceration of the Os Uteri. By Henry Smith, M.D. Louisville, 1855. (Reprint)

The Ethnological Exhibitions of London. By John Conolly, M.D., D.C.L. London, 1855.

Gout and Rheumatism, and the Curative Effects of Galvanism. By R. M. Lawrance, M.A., M.D. London, 1855.

Die Heilgymnastik in Schweden und Norwegen dargestellt. Von Edmund Friedrich, M.D. Dresden, 1855.

Die Schutzmittel gegen die Cholera, &c. Von Dr. C. J. Heidler. Prague, 1854.

Report by the President's Council of the Royal College of Surgeons of Edinburgh upon a document entitled, Draft Bill for Regulating and Improving the Medical Profession, &c. 1855.

Notes on Nurses. Practical Suggestions addressed to English Ladies. London, 1855.

On Epidemic Diarrhœa and Cholera, with a record of Cases. By George C. Johnson, M.D., F.R.C.P. London, 1855.

A Guide to the Practical Study of Diseases of the Eye. By James Dixon, Surgeon to the Royal London Ophthalmic Hospital. London, 1855.

The Obstetric Memoirs and Contributions of John Y. Simpson, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh. Edited by W. O. Priestley, M.D., and H. B. Storer, M.D. Vol. I. Edinburgh, 1855.

On the Diseases of Miners. By Herbert Mackworth, M. Inst. C.E. London, 1855.

The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias. By Henry Beasley. Sixth Edition. London, 1855.

On the Influence of Education and Training in Preventing Diseases of the Nervous System. By R. B. Carter, M.R.C.S. London, 1855.

The Principles and Practice of Ophthalmic Surgery. By T. Wharton Jones, F.R.S. London, 1855.

A System of Instruction in Qualitative Chemical Analysis. By Dr. R. Remigius Fresenius. Fourth Edition. Edited by J. L. Bullock, F.C.S. London, 1855.

The Re-organization of the Medical Department of the Army. By Philo-Medicus. London, 1855.

Untersuchungen über die Höhlenbildung in Tuberculösen Lungen. Von Dr. Hugo Rühle. 1853.

Observations on the Life, Disease, and Death of John Hunter. By Joseph Ridge, M.D., F.R.C.P. London, 1855.

Die Epidemische Cholera. Von Dr. C. J. Heidler. Leipzig, 1848.

Versuch einer neuen Begründung der Cholera-Wissenschaft. Von Dr. C. J. Heidler. Prague, 1854.

An Essay on the Action of Medicines in the System. By F. W. Headland, M.B. Second Edition. London, 1855.

The Diagnosis of Diseases of the Brain, Spinal Cord, Nerves, and their Appendages. By J. Russell Reynolds, M.D., London University Medical Scholar. London, 1855.

Twelfth Annual Report of the Managers of the State Lunatic Asylum. Albany, U.S., 1855.

Universal Formulary, containing the Methods of Preparing and Administering Officinal and other Medicines, the whole adapted to Physicians and Pharmaceutists. By R. Eglesfield Griffith, M.D. Edited by R. P. Thomas, M.D. Philadelphia, 1854.

A Manual of Clinical Medicine and Physical Diagnosis. By T. H. Tanner, M.D. London, 1855.

An Experimental Enquiry into the Existence of a Sixth Sense, here called the Sense of Force. By Richard F. Battye. Edinburgh, 1855. (Reprint.)

Fourth Annual Report of the Wilts County Asylum, for the year 1854. Devizes.

Medical Notes on the Climate of Burmah. By Charles Murchison, M.D., Physician to the Western General Dispensary. 1855. (Reprint.)

A Paper on Protracted Valvular Disease of the Heart. By J. W. Corson, M.D., Physician to the New York Dispensary. 1855. (Reprint.)

On the so-called Fatty Degeneration of the Placenta. By James M. Cowan, M.D. Edinburgh, 1855. (Reprint.)

Erratum.—In the April number of this Journal, p. 425, *for* Dr. Schulkof *read* Dr. Schulhof.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1855.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Report from the Select Committee on the Army before Sebastopol, with the Minutes of Evidence and Appendix.* Ordered by the House of Commons to be printed. 1855.—4 vols. folio.
2. *Report upon the State of the Hospitals of the British Army in the Crimea and Scutari, together with an Appendix.* Presented to Parliament by Command of her Majesty. 1855.—pp. 357, folio.

The history of the origin and progress of the war now waging in the East—the dark tale of incompetency and departmental mismanagement—the resulting misery and suffering among our troops—their brilliant deeds of daring in the battle-field—their indomitable perseverance in the siege operations, under privations of no ordinary kind—their discipline, their fortitude, and their unmurmuring patience under suffering, are events of too recent occurrence to require to be here recorded. The heroic exploits of the gallant army will be chronicled, as they well deserve to be, by abler pens than ours; and the fatal mistakes which have been among the chief causes of its sufferings, will, we trust, lead to a satisfactory reform in the military system of this country, which, by putting the right men in the right places, may prevent a recurrence of such deplorable events.

These are subjects which it would be out of place to discuss in the pages of a medical journal, but for the circumstance that much blame has been thrown upon the medical department of the army. Its organization has been loudly condemned by the newspaper press; its system of management has been called in question; an outcry has been raised against the Director-General, and he has been held up to public odium, as having, through his neglect, sacrificed thousands of our gallant soldiers. We propose to examine, by the evidence contained in the

Reports laid before Parliament, how far these charges can in justice be sustained; to whom blame can fairly be attached; to what extent the mismanagement is attributable to the system or to individuals; and what appear the most likely means to render the department more effective in future. Before entering upon these questions, however, it may be advisable to give a very brief and general sketch of the movements of the army, and the state of health at the different places where it was stationed.

In February, 1854, the government having decided to send an army of observation to the Mediterranean, to be ready in case of a declaration of war with Russia, 10,000 men were assembled at Malta. It was shortly thereafter resolved to increase the force to 25,000, and to send it to some place nearer the probable scene of its future operations. Accordingly, towards the end of April, the additional troops from England, as well as those already assembled at Malta, were landed at Gallipoli, on the shores of the Dardanelles, and subsequently moved to Scutari and Koolalee, near Constantinople. In June, the army embarked at Scutari and proceeded to Varna, in the neighbourhood of which it encamped. After a short time the force was spread over the country; the heavy cavalry and the third division remained in the immediate neighbourhood of the town; the first division encamped at Aladyn, a distance of nine miles from it; the second division about eighteen miles up the country; the light division at Devno, four or five miles further on, while the horse artillery and light cavalry were pushed forward as far as Jenibazar, a distance of forty miles from Varna. At Malta, Gallipoli, and Scutari the troops enjoyed good health; but about a month after landing at Varna, cholera broke out among the Sappers and Miners, and afterwards prevailed to a considerable extent in all the divisions of the army. Fever and diarrhœa were also very prevalent. It may be necessary here to observe, that in the selection of the ground on which to encamp the various divisions, the medical officers do not appear to have been consulted—the spot having been fixed upon by the general commanding the division. Indeed, General Bentinck states, that it is not usual to take the opinion of any medical officer on this point, "unless something occurs to render it necessary." When the cholera broke out in the first division at Aladyn, the troops were encamped not far from a lake, from which very dense mists rose in the morning and evening,* and which was stated by the inhabitants to be a very unhealthy locality.† Shortly after the disease appeared, the division was moved by the Duke of Cambridge to Gevreckler, "about six miles distant, on the top of the hills, a long way from the lake." General Bentinck states, that after the change the health of the men improved slightly, but the cholera and typhus did not quite cease; but the Duke of Cambridge does not admit that the removal had any effect upon the sickness. There can be no doubt, however, that the first site was badly selected, and that had the troops been from the commencement at Gevreckler, they would have had a better chance of escaping disease. This is but one of many instances illustrative of the necessity for a council of health with every army in the field, to advise the General Commanding on all sanitary questions, and which should have a powerful voice in the selection of the ground for encamping, whenever strategic considerations do not interfere. The brigade of Guards lost from cholera and fever 170 men out of a strength of about 3000; the second division lost only about 60 out of 6200; while the light cavalry and horse artillery enjoyed a still greater exemption.

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* Lieut.-Col. Wilson's evidence.

† Major-General Bentinck.

During the time the army was in Bulgaria, it was indifferently well-provisioned. Occasionally the supply was not very regular, owing to the want of sufficient transport, and the forage for the horses was often very irregular. Even at this early period of the campaign, the deficiencies of the medical department began to show themselves. When the various divisions were, as already stated, moved up the country, a general hospital was established at Varna, with a view to relieve the field hospitals of such cases as, from the nature of the diseases, were likely to remain long under treatment; but from want of transport for the sick, this arrangement was of little service. A few cases were sent down in native carts, but there was so much difficulty in procuring even these, and they were so unsuited for the transport of sick men, that it was carried out to a very limited extent. After some time, an ambulance corps, which had been organized in England, was sent to Varna, with proper waggons and the necessary equipments; but as, up to the date of the army embarking for the Crimea, the waggons were not supplied with horses,* it might have been left at home, so far, at least, as the removal of the sick was concerned. The supply of medicines and medical comforts also was lamentably deficient. When the light division was ordered to Aladyn, "it was with the greatest difficulty that the smallest allowance of transport was granted by the military authorities. No medicine-chests, reserve supplies of medicines, or comforts, were allowed to be carried, not even a small supply of medicine for the staff, &c."† The ticking of a paillasse was issued for the use of the sick in the regimental hospital marquees; but when an application was made for straw to fill them, it was returned with the remark, that "the supply of straw for the sick was deemed unnecessary."‡

The difficulty of finding transport also prevented the necessary supplies of medicines and medical comforts being sent up to the divisions; and after the outbreak of cholera the want of these was severely felt. When at length the troops were again assembled at Varna, with the regimental panniers exhausted, and with the prospect of immediate active service, it was with the utmost difficulty that a very moderate supply of the most necessary medicines was obtained.§

In the end of August the army embarked at Varna for the Crimea, and, after various delays, effected a landing unopposed at the Old Fort, on the 14th of September. The night after the landing was very wet, and the men being without tents suffered a great deal in consequence. The following day the tents were landed, but, owing to the want of transport, they were again put on board ship before the army advanced on the morning of the 19th. On the 20th the battle of Alma was fought, and the army remained on the field of victory during the two following days, to remove the wounded to the ships, and to bury the dead. The ambulances having been left behind at Varna, there were no means of conveying the wounded to the beach, but a very few arabas, and the stretchers carried by the bandsmen of the regiments. The navy, however, kindly lent their aid, and landed 1000 men with 500 stretchers, who rendered most efficient assistance, and the French ambulance corps gave their valuable services; so that by the 22nd all the wounded were embarked. On the morning of the 23rd the army advanced to the Katcha, the following day to the Belbeck, on the next to the Tchernaya, on the 26th arrived at the valley of Balaklava and occupied the town; on the following day the allied armies took up their position on the heights above Sebastopol.

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* Capt. Wrottesley's evidence.

† Staff-Surgeon Alexander's evidence.

‡ Col. Haly, 47th regt.

§ Staff-Surgeon Alexander; Lord Cardigan.

The men had been landed in very light order, carrying only their great coats and a few things rolled up in their blankets, and leaving their knapsacks on board ship. As already stated, the tents had been re-shipped, owing to the want of transport, and each regiment was allowed carriage for only one bell tent, to be used as an hospital tent. The men were consequently exposed at night to the cold and dews, with no other covering than their great coat and blanket. Diarrhœa and cholera, which had scarcely subsided, again broke out among them, and it is to be feared that many who were compelled to fall out on the line of march perished, whose lives might have been saved had an efficient ambulance corps accompanied the army. The tents were taken by sea to Balaklava, and were issued to the troops in the beginning of October, but until then the men were constantly exposed. About the same time the ambulance corps was brought from Varna, and was employed in carrying the sick and wounded from the front down to the general hospital established in Balaklava.

Two days after the arrival of the army before Sebastopol the siege operations were begun, and the trenches were opened. The work was very severe, the men being on picket or in the trenches usually three nights out of four;* and their health naturally suffered from it, diarrhœa and cholera becoming very prevalent. On the 25th of October the celebrated cavalry action at Balaklava was fought; and on the 5th of November the Russians attempted to carry the position of the allies, and were defeated in the bloody action of Inkermann, where their numbers put *hors de combat* exceeded the whole force of French and English engaged on that day. The wounded in these two actions were first taken to the field hospitals, and thence removed by the ambulances to Balaklava, where a large number were put on board ships, and sent down to the general hospital at Scutari. About this time the weather began to break, and a considerable increase took place in the amount of sickness, from the exposure of the men to wet in the trenches, the insufficiency of the tents as a protection against the weather, and the want of a change of clothing, their knapsacks not having been restored to them since they landed at Old Fort. But upon the 14th of November a terrific storm occurred, which blew down all the tents, converted the whole camp into a vast swamp, destroyed completely the road from Balaklava, and wrecked thirty-two transports laden with warm clothing, with provisions, medicines, medical comforts, and forage—in short, with almost everything on which the comfort, the health, nay, the very existence, of the army depended. The consequences of this storm were most disastrous; the land transport, which had before been inadequate, completely broke down, the horses dying from over-work and want of food. The men were put upon salt rations, and these, though not often very deficient, were issued irregularly; the necessary fuel was wanting for cooking; the men, after long exposure to wet in the trenches, returned to their tents, where, overpowered by fatigue and sleep, they lay down in their wet clothes upon the ground, which had been trodden into mud; while the miserable bell tents kept out neither wet nor cold. As a natural result, dysentery in its most intractable form—scorbutic dysentery—became extremely prevalent. The ambulance horses having been made use of for general purposes by the quarter-master-general, succumbed, like the others, to the exposure and over-work, and there existed no means of removing the sick to Balaklava; they were consequently treated under canvas; and as many of the regiments were not provided with hospital marquees, the only shelter the unfortunate patients had, was the common bell tent.

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* Lieut.-Col. Wilson's evidence.

The artillery lent their waggons when they could be spared, and the few remaining cavalry horses were employed in bringing down such of the sick as could ride—a most unsuitable conveyance for dysenteric patients. It was to our allies, the French, we were chiefly indebted for the removal of the sick, as they lent us their ambulance mules, with cacolets and litters. Even after warm clothing had reached Balaklava, it could not be brought up to the front, from the want of transport, and the men remained exposed to the wet and cold, most inadequately clothed, when there was an abundant supply of everything necessary within six or seven miles of them. The medicines essential to the treatment of the cases were not to be procured from the divisional medicine chests, which had been long exhausted; and when a medical officer sent, or, as sometimes happened, went himself to Balaklava, he frequently found that they were not to be had even there. Under such circumstances the surprise is, not that there was so much sickness, but that the whole army was not annihilated. Independent of the sick treated in camp and at Balaklava, there were admitted into the hospitals at Scutari, Abydos, and Smyrna, from the 7th of September, 1854, to the 18th of February, 1855, no less than 17,537 soldiers, being one-third of the whole of the army which left England, all reinforcements included, and of these upwards of 3000 died.

That this enormous amount of sickness was chiefly the result of over-work, exposure, inadequate clothing, and bad feeding, was shown by the fact that when means of transport were at length procured by the formation of the Balaklava and Kadikoi railway, when abundance of warm clothing was provided, when fresh meat was again issued, when by a re-distribution of the siege works with our allies the men were a little relieved from excessive over-work, their health immediately began to improve, disease became less prevalent, and of a less formidable type; and the amelioration continued progressive until the army, before the end of April, was reported in a highly satisfactory sanitary condition. This view is further corroborated by the much smaller amount of sickness and mortality among the horse artillery than the line. From not being employed in the trenches, the duty came round to the former only every fifth night, they were much better supplied with warm clothing and provisions, and had waterproof sheets to lie upon. The influence of these circumstances may, perhaps, be better estimated from the fact, that while at the end of January the effective strength of the army under Lord Raglan had been reduced by deaths and by sickness to less than half its original numbers, of 165 men of the horse artillery who were landed in the Crimea, there were at that date 140 fit for service.*

We shall now proceed to examine briefly what measures of preparation were adopted to meet the probable wants of the army in relation to the medical service. On the 10th of February, 1854, Dr. Smith, the Director-General, was instructed to provide all the necessaries for the medical department of an army of 10,000 men, about to proceed to Malta. So promptly was this order obeyed, that the whole of the stores were ready at the Tower by the 22nd. They were then put on board a sailing vessel, which was detained so long that the army had left Malta before it arrived there. The stores were then trans-shipped, and ultimately reached Constantinople on the 15th May, nearly three months after they had been reported ready for embarkation.

The steps taken by Dr. Smith to ascertain the nature of the climate, the diseases, and the salubrity of the country to which the army was about to proceed, were so judicious, that we shall give them in his own words.

"On the 13th of February, four days after I was informed that there was an army to go to Malta, with a great chance of proceeding to Turkey, I made a representation to the commander-in-chief, stating, that as we were ignorant of the nature of the country of Turkey, the nature of its climate, the nature of its diseases, and the nature of its resources, it was essential to the interests of that army that long before it should reach that country we should be in full possession of information on all those points. I proposed that three of the senior medical officers who were destined to serve in that army should at once be despatched; and I also proposed that they should be accompanied each by an engineer officer to make a military sketch of every place where it was possible that an army might have to halt. The application on behalf of sending medical officers was sanctioned, but not that for engineer officers to sketch the country. I immediately drew up instructions for the guidance of those medical officers, and they left this country about the 28th of February, about a fortnight after I first knew that the troops were going. . . . . Dr. Dumbreck went to Vienna: he communicated with Dr. Sigmund, and remained with him, and saw everything at Vienna in the way of military arrangements; in about four or five days he proceeded down the Danube, accompanied by an interpreter found for him by the government, communicated with every post of the Turks, inquired into the character of the country, the diseases to which the people were subject, the supply of water, and the nature of the locality, whether healthy or unhealthy, and how far it would be advantageous or disadvantageous to an army requiring to encamp there. The two medical officers who went to Constantinople were Dr. Linton and Dr. Mitchell. I instructed Dr. Linton on his arrival there to proceed along the coast of the Black Sea, that being one of the military roads, to examine the whole of the country, and the diseases of that part of the country; then to proceed to the westward along the Balkan mountains, and down the principal road *viâ* Adrianople, making the same inquiries at every locality where it might be possible, from water and other circumstances, that an army might halt. Then Dr. Mitchell, the third medical officer, I directed to proceed to Adrianople at once from Constantinople, and make his examination to the south-west as far as Gallipoli, and further than Gallipoli, in fact."(*Q.* 8075-6.)

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* Capt. Shakespear's evidence.

In about two months voluminous reports were received from these officers, giving full and detailed accounts in reference to every subject to which their attention had been drawn. A copy was sent to the army for the information of Lord Raglan and the principal medical officer, and extracts furnished to the authorities at home, where the suggestions were of such a nature as to require their assistance in carrying them out.

When the expeditionary force was increased to 25,000 men, there was some difficulty necessarily experienced in providing the requisite number of medical officers, but this was successfully met, partly by appointments of assistant-surgeons from the list of candidates, and partly by the employment on the staff in the East of the assistant-surgeons of the cavalry regiments and the depôts at home.

But in addition to these preparations, which were strictly within his jurisdiction, and were carried out under his own orders and superintendence, Dr. Smith brought under the consideration of the military authorities certain other measures likely to have an important influence on the health of the troops, but which the medical department had no power to carry out.

The first of these was the establishment of an ambulance corps,* to consist of 400† men, raised by volunteers from the depôts of regiments not going out to the East, and furnished with waggons for the carriage of the sick and wounded, and of the hospital tents and stores. The waggons were provided, and the corps raised; but instead of "able-bodied men, of good character, from Chatham, and efficient soldiers, it was decided that pensioners should be sent out," notwithstanding an energetic protest by Dr. Smith, founded on his experience of what pensioners are. Distrusting the utility of the men thus raised, Dr. Smith then proposed to organize an hospital corps of at least 800 Armenians, placed under regular military discipline, who should be furnished with stretchers or bearers to carry the wounded off the field, or where, from the badness or total want of roads, the ambulance waggons would not be available; and who might at other times be employed in the hospitals as orderlies.‡ After considerable delay and frequent pressing, the Duke of Newcastle at length consented to send out Dr. Brett, a retired surgeon of the E. I. Company's service, to form the corps; "but his mission was unsuccessful, he having been told on the spot the men would run away the moment they heard the firing; the corps was never raised."

The next matter brought under consideration was the clothing of the soldier. In a letter to the Military Secretary, dated 13th April, Dr. Smith recommended the adoption of a loose garment of lighter material in lieu of the red coat; the substitution of a light head-dress for the schako; the abolition of the stiff leathern stock; and the issue of flannel shirts and flannel drawers. In a subsequent letter, dated 28th April,

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* Letter to Military Secretary, Feb. 18th, 1854.

† This was intended for an army of 10,000 men.

‡ Letter to Military Secretary, April 4th, 1854.

Dr. Smith brought to the notice of the Military Secretary the following recommendation in the report by Dr. Dumbreck, already alluded to:

> "Of the severity of the climate during winter we have ample proofs, and on that account I beg most earnestly to recommend that timely measures be resorted to, with a view to protect the troops against the inclement weather to which they will be exposed should they have to pass the coming winter either in Turkey or in the Principalities." (*Q.* 8771.)

These letters appear to have received due consideration from Lord Hardinge;* and although it was not deemed expedient to adopt all the recommendations, certain orders were given on the subject, and steps were immediately taken to provide the requisite *winter* clothing. A large quantity was delivered at the Tower by the contractors in the beginning of August, and application made by the Ordnance to the Admiralty for freight on the 7th of that month, and repeated on the 15th of September; but partly through the carelessness of the Ordnance in not marking their application "urgent," and partly through culpable delay at the Admiralty, it did not leave this country till the 18th of October. It was sent out in the screw-steamer *Prince*, which took the 46th regiment from England to Balaklava, where she arrived on the 8th of November, and immediately landed the troops. Although the army had then begun to suffer from want of warm clothing, the supplies were not landed, but the vessel was ordered out of the harbour, and lay at anchor outside till the 14th, when she was lost in the hurricane, and in her were lost 53,000 woollen frocks, 17,000 pairs of woollen drawers, 35,700 pairs of woollen socks, 16,100 pairs of blankets, 3700 rugs, and 2500 watch-coats. Immediately on this being known at home, steps were taken to send out abundance of warm clothing of every description, but from the difficulty of obtaining steam transport, and the complete break-down which had taken place in the land transport, it was the middle of January before it began to reach the army, and considerably later before all the men were supplied—indeed, part of it arrived as late as the 14th of March! It is impossible to estimate the amount of suffering the soldiers must have undergone during the inclemency of the winter, or the extent of mortality which arose from this melancholy disaster. Well may Captain Milne say, "Had this ship not been lost, no inconvenience would have been felt in regard to warm clothing." The blame, however, clearly rests with the Ordnance and Admiralty for having so long delayed its transmission, and not with the military or medical authorities.

Another measure of great importance recommended by Dr. Smith, in a letter dated the 11th of May, 1854, was to provide and properly fit up a number of ships for the sick and wounded of the army. These were to be used for three different purposes: some were to be employed to convey direct to England men never likely to become available for further service, or who were not likely to do so within a reasonable time; others were to be set apart for the transport of sick and wounded to hospitals which might be established at a distance from the army—such as those at Scutari, Smyrna, and Abydos; and a third set were to be convalescent establishments, for cases which were making no progress to recovery on shore. The last were suggested in consequence of the benefits which had been derived in such cases at Hong Kong, and other places where malarious diseases were a source of great inefficiency. These recommendations appear to have been treated with neglect; the Secretary for War has no recollection of their being sent to him, and the Admiralty repudiates all knowledge of them,—in fact, beyond the Military Secretary, the Sebastopol Committee does not appear to have been able to trace them, and the only inference which can be drawn is that the letter was deposited in one of the pigeonholes in his office, and never again thought of. Had Dr. Smith's recommendations been adopted as to the first two classes of ships, all the opprobrium which has been thrown upon the medical department, in consequence of the sufferings of the unfortunate soldiers in what has not inaptly been termed "the middle passage," would have been avoided. The blame clearly rests with the authorities at the Horse-Guards, and not with the Director-General.

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* See Lord Hardinge's evidence.

At a subsequent date, after reports had reached home of the disgraceful state of the transports in which the wounded had been sent to Scutari, Sir James Graham wrote a private letter to Admiral Dundas, "inviting" his consideration to the question of fitting one or two steam transports specially for this purpose. The invitation, however, appears to have produced no effect; an official letter was therefore sent, on the 28th of December, calling the admiral's attention to the subject, and two vessels were subsequently fitted up. It is but fair, however, to Admiral Dundas to state that he denies having ever received any orders from home to prepare transports for the sick and wounded—a matter which may well be left to be settled between him and Sir James Graham.

We have detailed these matters thus fully, because, in our opinion, they go far to exonerate the Director-General from the accusations of neglect and incompetence which were so freely brought against him; while, at the same time, they show a greater amount of foresight and consideration on his part than characterized most of the other departments.

The Duke of Newcastle and Lord Hardinge both find fault with Dr. Smith, as it appears to us unfairly, because he did not either reiterate his recommendations until they were attended to, or appeal to the Minister for War against the military authorities for not acceding to his wishes. But surely, when the head of the medical department had made his official recommendations, and transmitted his requisitions to the proper authorities, he transferred to them the responsibility of any subsequent neglect. His duties in superintending his own department were sufficiently onerous, without having superadded to them the task of overseer to the Commander-in-chief, the Board of Ordnance, and the Admiralty Board.

With an army in the field, the necessary arrangements for the treatment of the sick and wounded in field hospitals, and for the establishment of general hospitals in the rear, devolve upon the principal medical officer with the force. This appointment was, in the first instance, held by Deputy Inspector-General Burrell, who was succeeded at the end of May by Deputy Inspector-General Dumbreck, and he was relieved by Inspector-General Hall, who joined at Varna in the end of June. With the latter, therefore, rested the responsibility of all the arrangements at the date of invading the Crimea.

When the army was at Scutari an hospital was established there, and on the embarkation for Varna taking place, all the sick of the regiments were transferred to it. After disembarking at Varna, a general hospital was opened for the reception of the sick in case of the army taking the field, and to form a depôt for medicines and purveyor's stores on which the medical officers in charge of divisions might draw for the necessary supplies. When the army embarked for the Crimea the hospital at Varna was broken up; part of the stores were put on board a ship to accompany the expedition, and the remainder were ordered to be sent without delay to Scutari, which was to be the general hospital for the sick and wounded of the army. Unfortunately, this order was not complied with—at first, from want of sea transport, and afterwards, apparently, from neglect on the part of the persons in charge of the transport department, and want of energy in the medical officer on the spot. Dr. Smith says:

> "I may have written, and I have written, not condemning (the medical officers), but regretting that they did not show more positive determination with the authorities who had the power of doing what was necessary, than perhaps they did show; and communicating to those authorities, that if such a state of things was permitted to continue, it must be understood that all responsibility was removed from the medical department." (*Q.* 8474.)

Much of the subsequent misery at Scutari arose from the hospital stores being still at Varna, where they were not required.

Before leaving Varna, Lord Raglan called upon Dr. Hall to furnish him with a statement of the amount of transport which would be required for the conveyance of the wounded, on the army taking the field. This he accordingly did, in a letter dated 3rd of August, in which he detailed at considerable length the whole of the arrangements he deemed requisite, and the various duties to be performed by the officers of the department. Dr. Hall recommended that with each division of the army there should be 2 large store waggons for head quarters, 2 smaller ones for brigades; 6 spring waggons, one for each regiment; and 96 canvas stretchers, with two bearers to each. These proportions would give 30 waggons for infantry, 6 for cavalry, and 6 for artillery; making a total of 42 waggons for the whole army; 288 stretchers, carried by 576 men, for the infantry; 24 by 48 men for the cavalry; and 24 by 48 men for the artillery—making a total of 336, with 672 bearers.

It will hardly be credited that after all "no waggons whatever were embarked, except three, and these had no horses, harness, or drivers." It does not appear whether this was done by the quartermaster-general, under whom the ambulance corps was placed, or by Lord Raglan's orders, but he was of course responsible. Neither is there any evidence to show what steps Dr. Hall took upon the occasion, but IF he did not energetically remonstrate with Lord Raglan, and protest against being thus deprived of all his *matériel*, he was guilty of a gross dereliction of duty. It would be quite as sensible to land artillery in the face of an enemy without their ammunition tumbrils, as to land the medical officers without their store waggons, or the means of conveying wounded men to the hospitals in the rear, or, as at Alma, to the beach for embarkation. The consequences of this unaccountable proceeding have been already detailed in these pages.

Dr. Hall likewise suggested that two steamers should be fitted up and equipped as hospital ships, to receive the wounded if the landing of the army were opposed. The *Andes* and *Cambria* were told off for that purpose, but had merely the fittings of ordinary troop ships. Dr. Hall states they were not so large nor so well calculated for the service as he could have wished, and were wholly insufficient for the sick and wounded after the Alma. He was not consulted as to the vessels, or as to the number to be put on board of each.

After the army had reached the south side of Sebastopol and commenced the siege operations, a house in Balaklava was given over for a general hospital, and fitted up with the stores which had been brought from Varna. This hospital appears, by the evidence of Captain Dacres and Dr. Dumbreck, to have been got into a very creditable state; but on the 25th of October, when Balaklava was threatened by the Russians, orders were given, by an officer of the quartermaster-general's department, to dismantle it and embark all the stores, which was accordingly done. The hospital does not appear to have been reorganized efficiently, at least for a considerable time; it was chiefly used as an "entrepôt for men arriving late at night from the front, and who were embarked on the following day." In consequence of this state of the hospital, and of the total break-down of the land-transport, the field hospitals became in reality permanent hospitals. As already stated, only some of the regiments had marquees, the others being provided with nothing but bell tents, in every way unsuitable for the treatment of sick, whose sufferings were still further increased by the want of fuel. Under these circumstances it was deemed expedient, as often as transport could be procured, to send down the sick from the front to Balaklava, and embark them at once for the general hospital at Scutari. This leads naturally to the inquiry, What steps were taken to provide for their transmission? and as the sufferings of the unfortunate sick were very much increased, from bad arrangements, it may not be out of place to go at some length into this part of the subject.

From the very commencement this important branch of the service seems to have been conducted in a very discreditable manner. After the army had landed at Old Fort, it was determined to send the sick down to Scutari, and for this duty the *Kangaroo* was told off. Captain Christie, the naval officer in charge of the transports, had a signal made to send all the sick on board, and neglected to make any limitation as to numbers. The consequence was every ship sent some, and the *Kangaroo* was overcrowded to such an extent that there was not moving space on the decks. A second steamer and a sailing vessel were then got ready, to which a large number of them were sent; but these were of course not fitted up for the reception of sick. Cholera prevailed on board, and 51 deaths are reported to have occurred, during the four days occupied in the transit to Scutari, out of about 1300 embarked.

After the battle of the Alma the wounded were brought down to the beach, as already stated, chiefly by the sailors and the French ambulance corps. There was only one vessel there which had been equipped as an hospital ship, the other having been previously despatched to Scutari. The *Andes* took about 450 officers and men, but as the number of sick and wounded brought down for embarkation amounted to upwards of 2800, the remainder, with the exception of 453 embarked in H.M.S. *Vulcan*, were put on board transports in no way fitted for their reception. The medical officers of the navy were most active in superintending the embarkation of these men, and afforded much valuable assistance when they had been put on board. A serious miscalculation of the amount of transport likely to be required, appears to have been made by the authorities. Dr. Hall applied for two vessels only to be fitted up, and even if they had been of the largest class they could not have accommodated above 800 men. Admiral Dundas, also, in his evidence, says, "Who ever thought that there would be 1500 or 2000 men wounded? You could stow 300 or 400 men, with beds, on board those transports, but no one suspected that there were 1500 men wounded, and 1500 sick patients." This mistake, however, is scarcely excusable, for in a paper by Mr. Guthrie, giving a sketch of medical arrangements for a force of 12,000 men, 1500 is the number he computes as likely to be wounded in the first battle; and he adds his belief "that 24,000 men will probably suffer little more in one action, than half their numbers if opposed to a superior force." As a copy of this sketch was sent to the military and medical authorities, more ample provision ought clearly to have been made for anticipated casualties.

But if the unexpected amount of wounds and sickness be deemed sufficient to excuse the very defective arrangements after the battle of the Alma, the experience of that occasion ought most assuredly to have demonstrated the necessity for some more extensive and permanent provision, in case of another battle being fought, or sickness continuing to prevail so extensively in the force. But nothing of the kind seems to have been effected. After the army had reached Balaklava, and fatigue, exposure, and bad feeding had done their work in inducing disease, the sick were still embarked in vessels wholly unprovided with any of the essentials of hospital transports—without bedding, without proper means of cooking, without any conveniences beyond a few ship's pails, with a deficient supply of medical comforts, and with too few orderlies, and these often unfit for their duty. The consequence of this was a great amount of suffering among the unfortunate patients, and, it is to be feared, a considerable increase in the mortality, which by judicious arrangements might have been avoided.

It is difficult to say with whom the blame of this mismanagement rests. Sir James Graham's "invitation" to Admiral Dundas, to consider the propriety of fitting one or two steam transports specially for the use of the sick and wounded, does not appear to have received any attention. But Admiral Dundas repudiates all responsibility connected with the transports. "They were entirely under the management of Lord Raglan, Admiral Boxer, and Captain Christie." (20,495). This Admiral Boxer, who may therefore be considered responsible for the fitting of the ships, is thus described by Deputy Commissary-General Smith:

> "Whatever I ask is regarded as a bore, or granted as a favour; and all the arrangements of one day, which have occupied great time, and caused me immense trouble, may be altered the next, as is frequently the case. Admiral Boxer, in short, is a confused man, and has but little control over the ship-masters, who entertain neither respect for, nor fear of him." (15,888.)

It is not to be wondered at that under such a superintendent the service was badly conducted.

But even when the transports arrived at Scutari, the miseries of the unfortunate soldiers did not end. They were, in many cases, detained on board several days, either till room could be made for them in hospital, or from want of boats in which to land them; on one occasion the sick were thus prevented landing for four days, in consequence of Admiral Boxer having applied the boats to another purpose.

The responsibility of an inadequate number of orderlies to attend upon the sick rests, not with the medical authorities, but with Lord Raglan. The Queen's Regulations fix the proportion of these at one for every ten sick—certainly not too high a proportion for such cases as were being sent from the Crimea to Scutari. But Lord Raglan, in a memorandum dated October 18th, with a view, doubtless, to diminish the drain of men from the already over-worked army, ordered the number to be sent on board transports to be in the proportion of one to twenty-five, "a number wholly inadequate for the service on which they were employed."

It does not appear from the evidence that Dr. Hall took any steps to make Lord Raglan acquainted with the disgraceful state of the transports, or to have them improved by the naval authorities. The medical officers in charge at Balaklava seem to have exerted themselves to the utmost of their means. Dr. Dumbreck states that there were two thousand five hundred empty paillasses which he attempted to get filled with hay for the use of the sick in the transports. "I applied for hay again and again; at last with difficulty I got 1000lbs., and I stuffed a hundred of them, and there the hay ceased." (11,508.)

The evidence of Captain Dacres, R.N., also shows that the neglect of the sick at Balaklava, which had been a subject of complaint in some of the newspapers, was not chargeable upon the medical officers, for he says:

> "I never saw more attention paid to people (than to the sick and wounded coming down for embarkation). Their own people came down with them to Balaklava; their own soldiers brought them down, and there were surgeons on the wharf seeing every man put in, and there were people giving them drink and everything else, very different from what it had been after the Alma." (16,202.)

It has been already stated that when the army left Constantinople for Varna a general hospital was established at Scutari, to which the sick then under treatment were transferred. When the expedition to the Crimea was in contemplation, instructions were sent to prepare more extensive accommodation, and for this purpose a large barrack near the hospital was obtained. It was a building badly adapted for an hospital, but was accepted as being the only available one capable of affording sufficient accommodation. Some progress was made in cleaning and whitewashing it, but when the sick and wounded began to arrive it was still in a filthy condition, and the hospital stores, as before stated, not having been forwarded from Varna, it was deficient in the furniture and fittings requisite for the proper treatment of the patients. The general hospital, which was capable of accommodating from eight to nine hundred, was tolerably well furnished, but the barrack hospital was without beds or proper bedding, unprovided with the necessary utensils, having no operating-room, no dead-house, no wash-house, and a kitchen insufficient for the ordinary cooking of the establishment, and, of course, without the slightest chance of preparing the extra diets and comforts so requisite for the sick. Add to this that the purveyor was incompetent, the apothecary inefficient, and the commandant afraid to incur responsibility, and it will be admitted that it would have required administrative talents of a high order to bring the place into a satisfactory condition. On September 21st, when the sick first began to come down from the Crimea, there were only four surgeons and twelve assistant-surgeons at Scutari under the superintendence of Staff-Surgeon Menzies. On that date 1262 sick arrived, and were landed the following day; on the 24th there were 435 wounded brought down, and on the day following the same number; on the 26th there arrived 1104, on the 28th 317, and on the 29th 274; the last two batches being chiefly cases of cholera, severe dysentery, and diarrhœa. There were thus thrown upon these hospitals, ill-adapted for the purpose, badly provided with the necessary fittings, and having a very small staff of officers, no less than 3817 cases, chiefly of severe wounds or of formidable diseases, in the short space of nine days. Probably no establishment, however well organized, could have satisfactorily provided for the wants of such an influx of patients, and it is impossible not to sympathise with Dr. Menzies in the difficult position in which he was placed, and which he thus describes:

> " . . . . My difficulties on the arrival of the wounded from the Crimea were wholly caused by Mr. Ward's [the purveyor] inefficiency, and not having made timely arrangements for supplying the required bedding, &c., for the sick and wounded in the barrack hospital, although I urged him daily, again and again, at the same time he assuring me that I might rest satisfied that everything there would be in perfect order. Much of the difficulty experienced at this time also arose from our not being in possession of our stores, especially bedding, which was daily expected from Varna. . . . . Notwithstanding numerous directions given both by Dr. Hall, the inspector-general, and myself, for their transmission, a very considerable delay took place from the want of transport; and it was not until I strongly urged Admiral Boxer to despatch a steamer for them that any decided steps were taken for sending these stores to Scutari; and when the first supply of boards and trestles arrived, they were useless without the bedding, which, by some extraordinary neglect on the part of one or other of the purveyors at Scutari or Varna, was not sent for some time after." (*Q.* 9742.)

Dr. Menzies complains also of the want of steady non-commissioned officers and proper orderlies, and in another part of the Report he states, "I had the work of three deputy-inspectors on my hands when I gave up the charge."

While Dr. Menzies appears to have worked indefatigably and conscientiously in endeavouring to discharge his duties, it is quite evident, from the whole tenor of his answers, that he was not possessed of those administrative talents which are essential to the efficient superintendence of such a vast establishment. Instead of organizing the officers under him, and allotting to each his proper duty, retaining to himself little more than the mere superintendence, he appears to have endeavoured to do more than the physical powers of any man could accomplish, and to have had no time or opportunity of seeing that his orders were carried out, and that his subordinates did their duty. He seems to have inferred that because he ordered a thing to be done, it was done. It is evident that he was not "the right man in the right place;" but that he was entrusted with duties for which he was constitutionally unqualified.

Taking this view, we are disposed to judge leniently of his failure, attaching blame not to him—for he could not, if willing, vacate the post—but to those who placed him there, and especially to Dr. Hall, who must have seen him to be unequal to the task, and yet did not replace him by a more efficient man. On one point, however, Dr. Menzies deserves censure. When applied to by Lord Stratford de Redcliffe to state if there were any wants at the hospital, and when offered an unlimited command of money to rectify these, if they existed, he reported that their wants were satisfactorily supplied, and that the sick and wounded had received every care and attention which their situation so imperatively demanded.* The only excuse for such a report is, that he too credulously believed the statements of the principal apothecary and purveyor—statements which were wholly erroneous, not, probably, from any intention to deceive, but from complete ignorance of the state of their stores, and utter incompetence to discharge their duties.

On the 3rd October, Dr. Hall arrived from the Crimea to examine into the state of the hospitals, and so far from removing Dr. Menzies, as being unequal to the duties, he reported to Dr. Smith, under date of 20th October, "that the whole hospital establishment here has now been put on a very creditable footing;" that "by the strenuous exertions and unceasing labours of first-class Staff-Surgeon Menzies, and the medical officers under him, all our difficulties have been in a great measure surmounted, and in a short time, I flatter myself, we shall have an hospital establishment that will bear a comparison with any one of the same magnitude formed under similar disadvantages, or indeed, I may almost venture to say, under any circumstances."† With the evidence before us of the actual state of the barrack hospital at that time—the want of bedding for the patients—the total absence of anything like clean linen—the defective supply of the necessary hospital furniture—the filthy state of the wards, swarming with vermin—the wretched kitchen arrangements, by which the diets were most irregularly distributed, and the food often only half cooked—the want of proper orderlies—the disgraceful state of the latrines—the uncleansed condition of the corridors—the want of sufficient storage for the apothecary and purveyor—and the entire absence of anything approaching to arrangement or knowledge of the stores under their charge—with the evidence before us on these points, we are wholly unable to comprehend Dr. Hall's Report, or to realize his ideas of an hospital on a "creditable footing." Believing as we do, that he was honest and sincere in the expression of his opinion, there are but two modes of explaining it—either that, like Dr. Menzies, he took his information at second-hand, without verifying it; or that his notions on the subject of a creditable hospital are such as to prove him utterly unfit for the appointment of inspector-general.

On the 4th of November, Miss Nightingale arrived at Scutari, with the band of trained nurses, and in a short time a marked improvement took place, by the establishment of a kitchen for the extra diets, the distribution of clean linen, the formation of a wash-house and laundry, and a more general attention to the cleanliness of the wards. At the same time, there was a considerable increase of the staff, by the arrival of medical officers from England, and the duty was thus brought more within their physical powers.

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* Letter from Dr. Menzies to Lord Stratford. Appendix to Second Report of Committee, p. 698.

† Appendix to Third Report of Committee, p. 505.

On the 6th November, Mr. Macdonald arrived to distribute the fund collected in this country for the relief of the sick and wounded, known as "*The Times*' Fund," and was the means of providing a great many of those articles of diet, clothing, and hospital furniture which tended so greatly to the comfort of the sick, and which, from the disgraceful mismanagement of the purveyor's department, were not forthcoming from the public stores.

Notwithstanding these improvements, however, the mortality in the hospital continued to increase, and it was not till towards the end of January that it began to show a marked diminution. This may have been in some measure attributable to the greatly overcrowded state of the wards, which appears to have been unavoidable, as Lord Stratford de Redcliffe was unable to obtain additional buildings, and the sick continued to pour down in great numbers from the Crimea. But it was chiefly owing to the condition to which the army before Sebastopol was reduced by the hardships, privations, exposure, and over-fatigue, to which it was subjected, and which gave rise to the worst form of scorbutic dysentery and typhus fever. From the period when the warm clothing and huts began to reach the army, may be dated the amelioration in the diseases treated at Scutari.

As allusion has repeatedly been made to the defective state of the apothecary and purveying departments, it seems necessary to make a few remarks on their condition before concluding this summary. In 1830, from motives of economy, these two departments were abolished. The duty of purveyor devolved upon the surgeon; and that of apothecary, in any of the large garrisons in our colonies, was performed by a second-class staff-surgeon. Some time before the war broke out the purveyor's department had been re-established on a different footing, and on so limited a scale that in the end of 1853 there were only three purveyors on full pay. It became necessary, therefore, when the army was sent to the East, to increase that branch considerably, which was done, partly by calling up men from half-pay, in the hope that their experience might prove useful, and partly by new appointments. The principal purveyor "was a man upwards of seventy years of age, who was exhausted with a walk between the general and barrack hospitals (at Scutari), only about a quarter of a mile. In addition to his own physical infirmity, he had only two assistants, and, I think, three boys as clerks, who were expected to do all the purveying for the sick and wounded men."* This was the officer of whom Dr. Menzies complained so seriously, as being the cause of all his difficulties. Shortly after Mr. Ward, the officer in question, went out to the East, rumours reached Dr. Smith that he was not efficient. A Medical Board was in consequence assembled, to examine and report upon him: they found him inefficient, and he was ordered home. Lord Raglan, however, at the instigation of Lord de Ros, the quartermaster-general of the army in the East, remonstrated against this decision, and, in consequence, Mr. Ward's recal was cancelled, and he remained at the head of his department at Scutari till his death in January, 1855. Not only, however, was Mr. Ward inefficient, but the whole of the arrangements were made with so parsimonious a hand, that the officers of that department had not enough of assistance to enable them to carry on their duties. Purveyor Jenner, who appears to have been an active and energetic officer, says:—"On leaving Varna I had not a clerk, sergeant, or even orderly. I had to break open my cases, issue the rations myself, and deal out *all* the medical comforts myself." In addition to these evils, there was for some time a difficulty with respect to the position of the purveyor, who conceived himself to be independent of the control of the principal medical officer, and amenable only to the orders of the Secretary at War. This was ultimately rectified by instructions sent out from the War Office, directing him "to obey such orders as the inspector-general of hospitals, or the principal medical officer in charge for the time being, may think necessary for the welfare of the sick and the interests of the public service."

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* Mr. Macdonald's evidence.

The apothecary's department was equally inefficient. A staff-surgeon of the second class, who had formerly served as an apothecary to the forces, was sent to Scutari to take the principal charge; but he appears to have been quite unfit for it, and his assistants were all men of no practical experience. Mr. Maxwell, one of the commissioners sent out to Scutari to report upon the hospitals, says, that upon examining the apothecary's stores he asked to see the books, and was shown two—a letter book, and one for entering the requisitions or the state of the stores in—"there had not been an entry made in either of them from September 24th to November 28th." "We examined the stores and the books, and we found both in such a state of confusion that we thought it impossible to obtain any trustworthy returns from such a source." (*Q.* 12,982.) The department seems to have been conducted in a hap-hazard manner, and the results were such as might have been anticipated. Requisitions from the Crimea for supplies of medicine were met by a very partial compliance—for example, on one occasion a demand for 50 lbs. of opium was answered by sending 5 lbs.—and it often happened that when a requisition for some particular medicine had been met by the answer, "none in store," a subsequent search produced abundance of it. The result of this was, not only that the sick at Scutari suffered, but at Balaklava, which ought to have been supplied from the general depôt, there was constantly a scarcity of even the most necessary medicines, and it was often impossible to furnish the medical officers in the camp with the supplies which they so earnestly demanded.

What was obviously required was some one who had been accustomed to take charge of large quantities of medicines, and who had acquired such habits of business as to enable him to superintend the issue of these, and to keep up the stores requisite to meet the probable demands upon him. A gentleman possessed of these qualifications was at length sent out as principal apothecary, but not until about the end of January.

In the preceding brief sketch we have endeavoured, honestly and impartially, to narrate the principal events in connexion with the Medical Department of the Army in the East, and to trace, so far as the evidence will permit, the shortcomings, defects, and mismanagement to their true source. Much of the blame which has been laid upon the department appears justly to lie with other branches of the service over which the medical officers had no control—especially the transport service, both by sea and land. Some part of it is due to the neglect of those recommendations which Dr. Smith had made to the military authorities, but which were not attended to by them; and a portion is attributable to the defective organization of the medical department, consequent upon the abolition of the apothecaries and purveyors, from shortsighted motives of economy. We have already stated that Dr. Smith's evidence and correspondence go far to exonerate him from blame; but in two points he seems to have failed. When he found his recommendation on *vital points* neglected, he ought to have offered the Duke of Newcastle the alternative of carrying them out, or accepting his resignation. Censure might have been directed against him for quitting his post at a moment when such exertions were required of his department, but the blame would have attached to the Duke of Newcastle for adopting that alternative. Again, when Dr. Smith found that he had been misled by Dr. Hall's report of the state of the hospitals at Scutari, he should have recalled that officer; by neglecting to do so he virtually adopted his opinion, in opposition to the mass of evidence brought against it. We are, notwithstanding this, disposed to concur with Lord Hardinge in his estimate of Dr. Smith:—"I have a very high opinion of him; I think him a very honourable and conscientious man, and he performed his duties, so far as I have been able to form an opinion, with very great precision, and a great desire to do what was right; and he is a very faithful and good public servant." (*Q.* 20,864.)

Throughout the whole mass of evidence in the Reports, there is none to show that Dr. Hall ever represented to Lord Raglan the inefficient state of the purveyor's and apothecary's departments, or the disgraceful condition of the transports for conveying the sick and wounded to Scutari. Nor does he appear to have protested against the total want of any proper conveyance for the sick and wounded when the army embarked for the Crimea. He seems to have been deficient in energy and firmness, accusing an officer, in one instance at least, of making difficulties, instead of endeavouring to supply him with those things he required as essentially necessary to the welfare of the division of which he was in charge.* Of Dr. Menzies, as head of the hospital at Scutari, we have already spoken.

Against this apparent apathy and inefficiency of the inspector-general, and want of administrative ability on the part of the principal medical officer at Scutari, it is pleasing to set off the testimony borne on all hands to the zeal and energy of the medical officers generally. Mr. Dundas, M.P., who was at Scutari in December, says, "With regard to the hospital at Scutari, I am disposed to give every credit to the medical men there; I believe that they spared no exertion to do what they could to put matters in order, only the work was too much for them." And Mr. Sidney Herbert, in the House of Commons, observed, "Every account I get, says this, the medical men in their vocation are beyond all praise, especially those at the head of the establishment; they work night and day, their tenderness to the sick, their humanity, their zeal, their energy, are mentioned by every one, friend and foe."

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* See Dr. Alexander's evidence, in the Report of the Commissioners.

The duties of the medical officers in the camp were of the most arduous and harassing nature. Suffering in their own persons the same privations and exposure as the soldier, they had not only the whole of that enormous crowd of sick to treat that afterwards inundated the hospitals at Scutari, but also many thousands of cases in the tents, which terminated before the means of transport to Balaklava could be procured. Those only who have encountered such terrible visitations as the famine-fever in Ireland, or the worst outbreaks of cholera among the destitute poor, can justly appreciate the labour and anxieties these officers must have undergone in the camp, and, knowing this from experience, will doubtless judge leniently their shortcomings, while they award to them the credit due to their patience, their exertions, and their unflinching perseverance.

The length to which our remarks have already extended compels us to be very brief in our notice of the changes which seem necessary to insure, for the future, greater efficiency in the medical department on service. In the first place, the Director-General should be in direct communication with the Minister of War, and not, as heretofore, under the control of several departments, and attended to more from courtesy than as a matter of right. Dr. Smith, in his evidence, remarked with much truth, "All the wants arose from a divided authority, the medical department being a sort of parasite department, living upon the rest." This is, therefore, the first and principal change required. Among the others may be mentioned, the organization of an efficient ambulance corps, for the removal of the wounded from the field of battle, and the conveyance of the sick on the line of march, or to the hospitals in the rear of the army. This might well be formed as a branch of the Land Transport Corps, but ought not to be available for any other employment at the wish of the quarter-master-general's, or any other department. The formation of a body of qualified hospital stewards and orderlies is, we understand, at present being carried out; and, as the men will not be drilled as soldiers, there will be no chance of their being removed from the hospital, as was formerly the case, for military duty. The Apothecaries' Department must be re-organized, and put on an efficient and permanent footing; it should be supplied with officers, not from the list of assistant-surgeons, but from persons in civil life who have had practical experience as pharmaceutical chemists, and been used to the handling of drugs. The appointment of purveyor should be abolished, and the duties devolved upon the commissariat, by whom they could be much more easily and satisfactorily worked. A branch of that service might, if deemed advisable, be formed for the "hospital commissariat." By such an arrangement much trouble, annoyance, and labour would be saved, at the same time that economy would be introduced, and a great probability of efficiency secured. On one other point an important change is necessary, —a fair proportion of the rewards and honours of the army must be granted to the medical officers. While brevet and substantive rank has been showered upon the fortunate military staff, and while promotion for "distinguished service in the field" has been conferred on not a few nothing has been done to reward the medical officers who have shared with them the dangers of the field, and encountered in a far greater degree the risk of the pestilence, which has cut off at least ten times as many as the enemy.

It is not reasonable to expect zeal and energy in a department which is forgotten in the distribution of honours, and whose only reward, and apparently deemed by the authorities a sufficient one, is the testimony of an approving conscience. The risk they run is shown by a return given in by Dr. Smith, from which it appears, that of 529 medical men, including apothecaries, dispensers, and dressers, who had gone out to Turkey, 29 died previous to the 7th March last—being 5½ per cent. in less than eleven months. The mortality since that date has unfortunately been very high, chiefly from fever; but as we have no correct information of the numbers who have died, we have confined our remark to the period included in the official documents.

The history of the department, as brought out in this investigation, furnishes an instructive lesson on the folly of that economy which reduces national establishments far below the point of efficiency. The same parsimony which deprived the medical service of its necessary numbers and organization, had so reduced the military force itself, as to oblige the government to send out large drafts of newly-raised soldiers, who, not inured to fatigue, literally melted away under the hardships of a winter campaign, in such numbers that they not only encumbered the already crowded hospitals, but furnished a very large proportion of the whole mortality of the army. A false economy thus entailed as its consequences much misery and suffering among the sick and wounded, and a deplorable sacrifice of human life.

Perhaps we cannot conclude this article better than by quoting the eulogium passed upon the medical officers by the Duke of Newcastle, when moving the thanks of Parliament to the army and navy of England:

> "I must state, in justice to an honourable profession, that never were greater exertions made by any body of men—never was more humanity evinced—never more complete devotedness to their duties, than by the medical officers of the British army in the Crimea. To one of these men I must allude. I will ask your Lordships to consider for one moment the services performed by such a man as Dr. Thomson. He was left, under circumstances of the most painful nature, upon the field of battle of the Alma, with not another person to assist him, not to attend to the wounded of his own army, all of whom had been removed, but to a large number of Russian wounded, many of whom, persuaded that an Englishman was little less than a devil, were prepared to murder any individual who might seek to render them succour and assistance. Among such men was Dr. Thomson left alone. He bound the wounds of some hundreds of these poor Russian soldiers, at the great danger of his life, but nevertheless he escaped. He returned to his duties in his own army; but it pleased Providence to remove him from his sphere of usefulness two or three days subsequently. His death was occasioned by the immense exertions he had made, and a disease which he had brought on by his extraordinary sacrifices and toils. I must say, my Lords, that if it has not been usual for Parliament to thank such men as these, at least it is not wrong for a Minister of the Crown to stand up in this House and express his admiration at such conduct." *

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* Hansard's Debates.

Review II.

1. *Pathological and Clinical Observations respecting Morbid Conditions of the Stomach.* By Dr. C. Handfield Jones, M.B. Cantab., F.R.C.P., F.R.S., Assistant-Physician to St. Mary's Hospital.—*London*, 1855. pp. 226.
2. *Observations of Morbid Changes in the Mucous Membrane of the Stomach.* By Dr. Hanfield[*] Jones. ('Medico-Chirurgical Transactions.' Second Series. Vol. xix. p. 87.) pp. 66.

The stomach is an ill-used viscus. It has been flattered in metaphor and insulted in fact. The poetical prose of the orator Menenius Agrippa makes it a type of a beneficent aristocracy; and the prosaic verse of the physician Serenus Sammonicus crowns it a king.[†] Persius dubs it a Master of Arts—*magister artium ingeniique largitor*—and Aretæus a general (ἡγεμὼν) of joy and grief. In the early part of the present generation, popular parlance referred to it all classes of ailments, bodily and mental: while medical men pronounced the viscus itself a mere bag, and all special attention to it quackery; and the scientific authors of the 'Cyclopædia of Practical Medicine' added the final insult to injury, by finding no place, save in a Supplement, for "Diseases of the Stomach and Pylorus." It will probably not be questioned, that there is no part of the body of which we hear so much from our patients, and are able to communicate so little real knowledge in return, as about the stomach. Its physiology has indeed been a good deal attended to, but its pathology has as yet had scarce a ray of light thrown upon it by modern science.

The publications named at the head of this article are the products of an attempt to clear away some of the mists from that dark region of pathology, and cannot, therefore, but strike the reader as worthy of great attention. Possibly he may expect to find in them more than the present state of science justifies, and then he will be disappointed; but, at the same time, he must acknowledge that the author is on the right road to truth, and is sure, therefore, to arrive at his goal before those who attempt the short cuts of theory and imitation—*claudus in viâ antevertit cursorem extra viam.*

The reason that induces us specially to pronounce the author "on the rails" to a correct knowledge of the morbid processes of the stomach, is, that he puts himself under the guidance of the two best engines of pathological research, as yet unapplied to this subject. The first is, *the exami-*

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[*] This spelling of Dr. Handfield Jones's name is the first in a series of 81 misprints to be found in the 66 pages of the paper. Some are of considerable importance, such as, "swollen," for "smaller;" "anterior," for "cut;" "valve," for "ventricle;" "less," for "more or less;" "appearance," for "appetite"—some ridiculous, such as describing an aged female as a "footman;" and "5 crepitations" in the lungs. For these the Society render themselves responsible, by not sending proofs to the authors for correction. It is to be hoped that the rule may be altered before the next annual issue, or that at least each volume may contain a list of the errata in the last.

[†] *Qui stomachum regem totius corporis esse*
Contemnunt, nisi verâ ratione videntur.

nation and record of the appearances of the same part in a consecutive series of unselected cases; and the second, *the skilled use of the microscope.*

The method of taking a consecutive series of cases is the only way to place a guard over that weakness of the human mind which causes positive events to take a much firmer hold on the memory than negative ones, which allows the seeing a thing a few times to outweigh the hundred times we do not see it under similar circumstances, and consequently allows false inferences to be drawn. The only way to avoid this is to take equal note of each case for a definite period or number, and to reckon up our statistics at the end of the series. We shall thus get a notion of the real frequency or rarity of the phenomena we are observing, and of their consequent importance, scanty perhaps, but ten times the worth of a collection of facts made sporadically, or with the view of proving a predetermined point.

The application of microscopic inquiry to such a series of consecutive cases is almost unique in any department of morbid anatomy. Indeed the labour involved is so great, that it scarcely surprises us to see even those best qualified by knowledge and leisure shrink from it. Certainly in the observation of the stomach nothing has been attempted at all like the task which the author of the works before us has performed. Professors Engel and Wedl have cursorily noticed that the glands of the stomach are sometimes degenerated; Professor Rokitansky has didactically suggested some differences in the forms of ulceration, &c.; but any notion of how frequent and how rare these lesions are, or with what other morbid states they are conjoined, is now first attempted to be given. It is not, of course, pretended that the one hundred cases here analysed put us in possession of the full information which is required as to the relation to one another and the comparative frequency of these lesions; but they show, at any rate, some of their bearings, and their very *great* frequency, and they are a guarantee that the threshold is passed of a most important inquiry, and one in which the aids of modern science may be most advantageously employed.

Without a microscope, very little can be done in this part of the body. The surface of a mucous membrane, unless absolutely destroyed by solution of continuity, exhibits scarce any evidence of chronic changes—certainly no evidence which is not easily masked by acute, temporary, or even *post-mortem* appearances. In painters' phrase, the accidental quite overpowers the local colouring. Dr. Jones truly remarks:

> "If any one should desire a proof of the great advances made in exact information by the aid of the microscope, he may find it in comparing the account given by Haller of this tissue, scarcely a hundred years ago, with our present knowledge. In his view it was a continuation of the epidermis, and, like it, might be thrown off and regenerated. Like the epidermis, too, its principal duty was to cover and protect the subjacent membrane, the 'tunica nervea,' *ne perpetuò doleat.*" (p. 13.)

Now, however, we know that this supposed homogeneous coat, this mere defence against accident, contains (or rather consists of) an infinite multitude of actively secreting glands, so numerous and close that there is hardly room for anything else besides them and the bloodvessels between them, yet each as distinct from the others as the separate fingers of a glove.

It is curious how few people have examined these glands, yet it is so interesting, and gives such a striking picture of the minute activity of nature, that it is worth the while of any one who can obtain access to a microscope to examine them, however busy he may be. There is no particular difficulty in obtaining this view, if you know how to set about it. Hold a piece of fresh healthy stomach between finger and thumb, lightly, so that the layers which form it may be distorted as little as possible from their natural position by squeezing. Then with a sharp pair of scissors shear it off, with a flat edge projecting just above your finger ends. Wipe and wet your scissors, and then shave off as fine a paring as possible, and lay it on the glass with a needle. Add a drop of water, and cover the specimen with a thin lamina of glass, remembering which way, upwards or downwards, you have placed the free surface of the membrane. Hold this up to the light, and even with the naked eye you will be able to discern that it is striated with fine lines, more opaque than the interspaces between them. At the base of these striæ lies the transparent corium, and beyond that the still looser areolar sub-mucous tissue with its longitudinal bloodvessels.

Place it under a glass of low power, say of about thirty diameters, and the striated part looks, if perfectly healthy, like palisading, or a collection of short staves, somewhat fatter and shorter than policemen's truncheons, of nearly equal length and packed quite close together, with a very small quantity of more transparent substance between them. These are the "gastric follicles," which in man are simple tubes of the appearance above described, but in animals divide into two, three, or more secondary tubes, and present lateral bulgings, as figured in Professor Kölliker's 'Mikroskopische Anatomie,' figg. 221 and 222, so as to destroy the truth of our rather rough comparisons as applied to the inferior creation.* Under a higher power the general effect is lost, but the contents of the glands are discerned to be a granular substance, which entirely fills up the interior of the glands. Squeeze the cover glass, and the grainy substance oozes out, still slightly cohering in a mass, sufficiently to show that it is the contents of a pipe-shaped follicle.

It is probably the thick secretion oozing from the glands which has led to the notion that the mucous membrane of the stomach has papillæ like the tongue or bowels. Dr. Beaumont speaks of seeing, in the stomach of the well-known Canadian voyageur, "*papillæ* protruding from the mucous coat, from which distils a pure, limpid, colourless, slightly viscid fluid," on the application of food or other stimulus. There is nothing in the organ discoverable by a microscope at all answering this description, except the casts from the glands, which we must suppose Dr. Beaumont saw thrown off on the application of an irritant.

Even in the glands, where it is closely massed together, the secretion gives the idea of containing globules; and if it is separately examined with a high power as it is found on the surface, these are distinct enough. They have been called "stomach-cells," or "rennet-cells," and have a nucleus and nucleolus. Besides this, there is a quantity of grainy matter in the secretion, which is just like the grainy matter which can be seen also in the cells, and therefore is considered to be the *débris* of some of those bodies broken up. If you are examining the stomach of an animal which has died while digestion was going on, you will find these stomach-cells in considerable quantities. I have seen them in the human stomach forming a multitude of little grey flakes, and they are described by Dr. Frerichs as constituting sometimes a continuous white layer to the naked eye.

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* In the human subject also, close to the pylorus, the stomach-glands have an arborescent appearance, like those of dogs or oxen figured in the work quoted. Mr. Ecker has described and figured them as such since the publication of Professor Kölliker's book. Henle's Zeitschrift für rationelle Medicin, N. F. ii. 248.

Now, whether these stomach-cells *are* the gastric ferment, or rennet, or whether they *contain* the rennet, or are simply the *concomitants* of the rennet, is truly, at present, a matter of opinion; but they very clearly have a most intimate connexion with it. When they are present, the gastric digestion is going on; when gastric digestion is not going on, they are absent. It appears to be a matter of certainty that from the gastric glands comes the principal portion of the solid animal matter of the gastric juice, the exciting cause of the digestion *in the stomach* of albuminoid substances, the main support of life.

The reason for italicizing "*in the stomach*," will appear afterwards. But it is obvious that these glands are of great importance, and that any observed changes in their appearance must possess much interest.

It is, then, a striking fact, that of the 100 cases examined by Dr. Jones, but 28 exhibited an approach to typical stomachs, the destruction of the tubular glands being in 14 very great—so as probably to render them quite unequal to their duties—and in the remaining 58 being undeniable under the microscope, but not likely to have interfered with the health of the patient.

This condition may very justly be called GLANDULAR DEGENERATION of the stomach, and is by far the most common of the organic lesions which are to be found in that part. To account for its very singular frequency, it must be remembered that nearly all the bodies examined were those of broken-down hospital patients, with all sorts of diseases and degenerations of other parts; and that it would have been strange indeed if the central viscus had not suffered along with the rest of the body. Indeed, it may be doubted whether this amount of microscopic glandular degeneration in the stomach is greater than might be found by a similarly minute examination of the kidneys, or other great manufacturing organs. All that it proves, then, is, that of all parts the stomach has the truest sympathy, in the strict sense of suffering with them, for the other members of the body.*

Glandular degeneration in the stomach, as elsewhere, involves two morbid actions—to wit, *imperfect growth of tissue*, and the *growth of imperfect tissue*. There is an absence, more or less complete, of the natural substance in its proper form, and the presence, more or less overpowering, of a substance possessed of lower organization, less life. The greater or less comparative predominance of one of these parts of the idea of Degeneration, gives birth to the infinite variety of forms under which it is pictured. The complete type of one extremity of the scale is the utter destruction of the tubular glands, and the substitution of nothing in their place; and at the other extremity would lie the deposition of granular substance, and no apparent wasting of the forms of the tubes. The exact medium is an equal proportion of wasting and granular matter, so that the weight and measure of the organ is not altered.

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* There is a volume, published by a Dr. Riga, in 1621, with the title 'De sympathiâ, sive consensu membrorum omnium corporis, et *præcipuè stomachi*.' The doctrines propounded are very like those of Broussais.

The longer and more attentively that degeneration is studied, the less real differences there appear to be between the three forms of fibroid, albuminoid, and fatty matters, which are substituted for the properly-formed tissue of glands or other parts, when the above-mentioned change takes place in the nutrition of the part. They are all evidences of a lower degree of interstitial life than ought to exist. Though different for the morbid anatomist, they offer to the physician completely identical suggestions, both for prognosis and treatment.

Besides the degeneration of the glandular and interstitial substance, Dr. Jones also notices the wasting of the epithelium alone, in some cases, which may be considered a minor stage of the more complete lesions above noticed.

It will easily be judged, therefore, that any classification of the appearances assumed by degenerated organs must be a purely arbitrary one, dependent entirely on the greater or less tendency of the observer, artist, or writer, to minute division or broad generalization. Bright's disease of the kidneys affords a striking instance of this, each author dividing its pathology differently; and acting so far wisely; but often, in the sequel, provoking a smile by an unphilosophical attack on the divisions of his collaborators. It is gratifying to see that Dr. Jones, though he has not in words set forth the doctrine of Degeneration above stated, yet in fact shows that it is influencing his thoughts by making no attempt at classification of the forms produced by its doings in the stomach.

Like all degenerations, that of the gastric glands is more prevalent as age advances—indeed, it would appear that only before full growth is attained can we safely calculate on finding these glands quite perfect in the stomach of an invalid. Yet the degeneration is not merely a physiological development; that which increases with age is not, as might be suspected, age itself; for in some few of the oldest cases examined by Dr. Jones—in one particularly, who had attained to seventy-four—the tubes were quite healthy. The probable explanation is, that the longer a man lives the more likely he is, on the mere doctrine of chances, to have some disease which leaves its traces in a microscopic injury to the tissues. The longer a man lives the more likely he is to feel half a dozen earthquakes, but his age is not the cause of the earthquakes.

As in degeneration indefinitely-shaped fibrine is formed in the tissues, and as in inflammation indefinitely-shaped fibrine is thrown out and fibrine also exists in excess in the blood, the two processes have been likened to each other, and the one at present under discussion has been shortly disposed of as "inflammation." But how different the vital phenomena in those almost contrasted processes! Watch degeneration—where you have an opportunity of watching so well all physiological acts—in the transparent eye. If arcus senilis were accompanied or preceded by congestion, heat, swelling, and pain, such facts could not escape notice: but there is notoriously nothing of the sort. If Bright's disease of the kidneys began with inflammatory symptoms, we should not be left long doubting about the fact. True there is sometimes hæmaturia, but certainly not as a rule; and that hæmaturia is not of an active sort. There is none of the pain, fever, and vomiting, well known to accompany true renal inflammation.* Then, again, degeneration attacks principally those parts which are least liable to inflammation, such as the kidneys; and avoids those which are most liable, such as lungs, areolar tissue, and skin. It is surely a pity to use any common term for such very different things.

On the causes of degeneration of the gastric tubes, Dr. Jones's cases do not enlighten us much. The lesion appears associated with all sorts of diseases, as well as with the diseases of all parts. The universal "sympathy" of the stomach seems most impartial. Some influences that moral grounds might induce us to hope would be shown to act in the production of the lesion, do not seem to have much to do with it. The 11 cases of hard drinkers had, as a rule, less the matter with these glands than the temperate. In only 3 was the amount of destruction very extensive, and in one man, whose addiction to the vice was so great as to sink him materially in the social scale, and cause his death in an hospital at forty-nine of diseased heart and liver, there was unexpectedly found a very tolerable state of stomach—not more disease, in fact, than was observed in numerous patients whose lives had certainly been very unlike his.

We come now to the main point—What effect upon the bodily economy has the loss of the services of this portion of membrane? Do we depend upon it entirely for the digestion of our meat, and are we therefore to give up the chance of supplying necessary albumen to our tissues in proportion as these glands are wasted? It would at first sight appear as if no gastric juice could be secreted, and therefore that no flesh meat could be made of use to the patient.

Now, in the first place, attention must be called to the fact that the semi-solid matter formed in these glands is by no means the most prominent ingredient in the gastric juice. Water constitutes its chief bulk, and, there is every reason to believe, is a most important ingredient therein. It is being continuously poured forth in vast quantities from the surface of the membrane, and neither the microscope nor the analogy of other glands, such as the kidneys, would lead us to think that the secretion of water was at all arrested in glands by the process of degeneration. Indeed, in many cases of advanced renal disease it is augmented.

To see the importance of this aqueous secretion, which we suppose to be undiminished, we must reflect on its very great quantity in the healthy, and, as above suggested, probably also in the unhealthy state. Drs. Bidder and Schmidt, from experiments on dogs with gastric fistulæ, inferred that in carnivorous mammalia the gastric juice secreted in twenty-four hours equals at least one-tenth of the animal's weight, and that of this, ninety-seven per cent. is water. Assuming the same to hold good of our own species, a man of ordinary size would daily secrete from fifteen to sixteen pints of this very aqueous fluid from the limited area of his stomach. It is curious to see how loath philosophers seem to receive this undeniable inference from Dr. Bidder's facts; even Dr. Jones, in the work under review, quotes the opinion with evident caution (p. 30), and seems to think that dogs are no rule for men. People view themselves as almost insulted by the accusation of having all their life been doing such an extensive business without knowing anything about it; and Dr. Lehmann, by arguments derived from chemistry, would prove that four pounds *per diem* is quite enough for a man to make.

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* The writer has had lately one of those opportunities which so rarely occurs, of keeping under his eye the whole course of a case of degeneration of the kidneys. He was obliged to have tracheotomy performed on a sailor with ulceration, probably syphilitic, of the larynx. The urine and everything else continued quite healthy during the four months it was necessary to keep the trachea open for the cure of the larynx. But unaccustomed confinement to the house, and perhaps the remedies used, broke down his constitution: he got dyspepsia, albuminuria, dropsy, and fatal diarrhœa. After death, the kidneys were found to be double their natural size, and quite yellow with morbid deposit. At no time was there pain in the loins, nor did the urine ever contain blood or fibrinous casts.

Great interest, therefore attaches to an opportunity lately afforded to Dr. Grünewaldt of measuring by actual experiment the gastric juice of our race, in an Esthonian peasant with a stomach fistula, which had existed from childhood, and did not in the least degree interfere with the general health.[*] If astonishment was excited by the large figures of Drs. Bidder and Schmidt, how must it be increased by this confirmation of them in our own species, who appear, from Dr. Grünewaldt's experiments, to secrete not one-tenth, but between a fifth and a quarter of their weight in gastric juice daily!

What is the office of all this water? It forms part of that great circulation through the alimentary mucous membrane, which might truly be called the fountain of animal life. It is constantly going its rounds like an endless chain, going out poor, and returning in a continuous stream laden with wealth; and is thus carrying on business with the outer world quite as important to existence as the more generally known and more anatomically distinct circulation of blood.[†]

It is difficult to over-estimate the importance of the watery part of the secretions of the alimentary canal, and we must not think that when the power of forming the peculiar solids of the gastric glands is taken away, that all the office of the stomach is annulled. The water still exudes, and not improbably carries with it some imperfect fluid-representative of the "rennet cells."

Besides this, were digestion in the stomach to be completely stopped, there appears to reside in the intestines a power of taking the duties refused by their leader. Drs. Bidder and Schmidt, by experiments on living animals, have shown that albuminous matters inserted into the ileum, with all access of gastric juice cut off, were dissolved in the same way as in the stomach;[‡] and the repetition of these experiments by Dr. Jones have fully confirmed their correctness.

Further experiments, too, by Dr. Ernest Schröder, render it most probable that in the human species this digestion of albuminoid matters by the intestines is not merely a power occasionally exerted in case of accidental arrest of the stomach's functions, but the normal action of the alimentary canal. He finds, by inspection of the Esthonian peasant, also observed by Dr. Grünewaldt, that albuminoid matters are never entirely digested in the first receptacle. In dogs, indeed, as he found by a repetition of Drs. Bidder and Schmidt's experiments, complete solution and conversion into peptone was always to be attained; but in the human patient, though in better health than the dogs, unaltered muscular fibre and the like always passed through the pylorus, and was never entirely absent from the contents of the stomach.[*]

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[*] Succi gastrici humani indoles physica et chemica ope fistulæ stomachalis indagata. Auctore Otto a Grünewaldt. Dorpati, 1853.

[†] The germ of the idea of a circulation through membranes, may be found in that strangely-worded collection of suggestive thoughts, Emmanuel Swedenborg's 'Animal Kingdom' (chap. xvi. § 325). He applies it to serous sacs, for the state of physiology in his time did not enable him to see its applicability to the mucous membranes. The unintelligible language in which they are couched has concealed this, and perhaps still conceals many another brave idea.

[‡] Die Verdauungsäfte. Fünfter Abschnitt. Vom Darmsaft.

It is clear, therefore, that any diseased state of stomach coming on slowly may be compensated for by increased vigour of intestinal digestion, and that in the human species this compensation is the easier attained because it is, to a certain extent, the normal condition.[†]

The loss of the services of the gastric glands is, then, less immediately influential on the vital acts than would have been deemed at first sight; and we shall not be so much surprised, after this introduction, at the evidence which runs through the whole table of cases given by Dr. Jones in the 'Medico-Chirurgical Transactions,' of the vague character of the symptoms referrible to the lesion. The effect on the bodily economy is so slight, and so easily masked by the least accompanying ailment, that it is rare in the history of the cases to find any evidence of its existence, even when the degeneration is very extensive. Neither does it shorten life to any appreciable extent; the average age of the patients with healthy stomachs was fifty-two, and of those with degenerated stomachs fifty-one. In only one case, a man of sixty-two (*No. 2 in the Table*), was the lesion the sole cause of death; and it did not prevent a workhouse nurse (*No. 69 in the Table*) from attaining the age of ninety.

Sometimes, indeed, there are notices in the history of the patients of weight at the pit of the stomach, a stoppage of food at the bottom of the gullet, occasional vomiting, unaccountable anæmia or debility, and a difficulty of rallying during acute disease; but in the great majority it is satisfactorily shown that there were no gastric symptoms at all to evidence any but very decided cases. And in one non-tabulated observation,[‡] where a woman who died of burn exhibited after death very great destruction of the tubes, there was no derangement of digestion, and the patient could eat any kind of meat.

Is it not probable that the differences between the two classes of cases, where symptoms were exhibited and where they were not, lies in the condition of the rest of the alimentary canal? Where the stomach is injured, even to a great extent, and the succeeding parts remain healthy, does not physiology teach us to expect compensation and little derangement? But where the whole of the digestive mucous membrane is even slightly affected, are not considerable disturbances of function to be looked for?

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[*] Succi gastrici humani vis digestiva. Auctore Ernesto de Schröder. Dorpati, 1853.

[†] The writer of the present article has been in the habit of using, with great success, as a main article of diet, in cases of gastric derangement, of low fever, &c., milk, prevented from coagulating by the addition of one-third of its bulk of lime-water. (An instance of the practice is quoted by Dr. Jones from the writer's clinique, in p. 61 of the book 'On the Stomach.') This has been objected to as impeding the natural digestion by the gastric juice. The answer is, That is the very thing required—the stomach is weak, so we wish to spare it. The stomach is short, and in sickness digestion is long; so we pass it on to the long bowels.

[‡] Medico-Chirurgical Transactions, p. 106.

We trust, then, that Dr. Jones's most valuable series of microscopic observations on the degeneration of the stomach will be followed up by a corresponding examination of the small intestines.

The next most frequent morbid condition of the mucous membrane of the stomach is that which is called in the 'Cyclopædia of Practical Medicine,' "Follicular Gastric Dyspepsia;" by Cullen, "Anorexia Humoralis;" by Dr. Jones (after the Germans), "GASTRIC CATARRH." The first name is anatomically wrong, for it assumes that the mucus is formed from the gastric glands or follicles—which is not the fact; Cullen's nomenclature is too narrow, for the appetite is not always deficient; and Dr. Jones's is too broad, for it includes the acute condition (the *embarras gastrique* of the French, our *bilious attack*) along with the chronic, as if one was a sequela or continuation of the other. Now this is a thing very much to be avoided, as it will introduce into the pathology of digestion all those false notions and false practices which have been occasioned by the unfortunate common name of "bronchitis" for an acute cold on the chest, and for a chronic secretion. We must confess to a prejudice against *omne quod exit in "itis,"* and would propose to use the word "catarrh" for the conditions, gastric and pulmonary, which run an acute course, *tending* to get well of their own accord; and the word "flux"—gastric flux or bronchial flux—for those whose course is chronic, i. e., which tend to confirm and aggravate themselves, to get worse unless counteracted.

In twenty-three of the hundred cases examined there was the above-named excessive secretion of mucus in the stomach; and from these twenty-three cases Dr. Jones makes the following deductions:

That age disposes to it;

That the two sexes are about equally liable;

That it is not dependent or in any way connected with degeneration of the tubes;

That drinking does not dispose to it;

That it is in the majority of cases associated with augmented secretion from other mucous systems.

In discussing the symptoms arising from this state of stomach, Dr. Jones has himself given an example of the confusion likely to arise from his name of "catarrh;" for he speaks of the chronic state, as the "later period of catarrh, *when the hyperæmia has subsided.*"* But no proof exists that chronic mucous flux begins with hyperæmia; and it cannot but lead to bad pathological inferences and bad practice to assume such a doubtful point.

The description of the symptoms is evidently drawn from nature, without any of the filling in of the rough outlines which renders our systematic works so smooth and flowing; and it is, therefore, doubly valuable to those who appreciate truth. Among them the author has of course not failed to enumerate pyrosis, and so far we cordially agree with him. But we cannot avoid dissenting from the etiology which he assigns to this latter complaint. He views the fluid thrown up as a mere variety of gastric flux, as mucus with its viscid nature diminished and its aqueous portion very much increased, thrown up from the stomach by reason of its superabundance. Now there is no evidence that "water-brash" has ever been in the stomach at all. The effort by which it is ejected is quite different from vomiting, and much nearer resembles that which takes place in obstruction of the gullet by cancer, ulcer, or contraction; there is no heaving of the diaphragm or spasm of the abdominal muscles, such as is necessary to empty the stomach, but an easy "rising" of the fluid into the mouth at a time remote from meals. Then, again, the matter thrown up contains generally none of the morphic or chemical elements peculiar to the stomach, no stomach-cells, or gastric ferment, or muriatic acid; but, on the other hand, exhibits the characteristics of saliva. It is alkaline, opalescent from the presence of the pavement epithelium of the mouth and throat; and, according to Dr. Frerichs,* converts starch into sugar, and contains cyanide of potassium. Is it not likely to be the secretion of the salivary glands, mouth, and gullet, detained in the latter by spasm, or, at all events, the saliva swallowed and detained in the stomach, rather than a watery flux of the gastric mucous membrane?

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* On the Stomach, p. 173.

It is true that sometimes water-brash occurring at a period soon after meals, is of a different character from that above described, being intensely acid, and exhibiting some of the usual contents of the stomach. But this seems to be rather an accident than a rule, and should probably be distinguished from the pure ailment, and be designated as water-brash complicated with vomiting—the said vomiting being of a slight character, and excited by the proceedings of the œsophagus.

Pyrosis is indeed a symptom of gastric mucous flux; but it is by no means peculiar to that derangement of the stomach occurring in cancer of the cardia or pylorus, in ulcer, and sometimes in hysteria—being, if the explanation above given be correct, a secondary disorder exhibited rather in the healthy than the ailing part.

SARCINÆ, though absent from the vomit during life, were found in two bodies (Nos. 33 and 67 of the Table). A remark, omitted by Dr. Jones, probably for the sake of condensation, may here be inserted—viz., that the first case, a patient of the present writer's, exhibited a proof of the true habitat of this parasitic mould; the sarcinous cells lay in considerable abundance, strongly adherent to the external layer of a gluey mucus, but none stuck to, or even touched, the membrane; thus showing that it is in some cases, and probably in all, a parasite accidental to an excess of tough secretion, and not connected with any deep-seated change. The gastric glands in this instance were normal.

ULCERATION is another lesion of the stomach to which Dr. Jones assigns greater frequency than is usually known. In the 100 consecutive cases tabulated, there are six of ulceration. The reason of its not being known to be so often present, probably is the rarity with which the stomach is opened, unless there are any special symptoms directing attention to it, and the great chance there is of the latency of these special symptoms in cases fatal from other causes.

Dr. Jones differs from Professor Rokitansky in not viewing "perforating ulcer" as a distinct class. He says he has not "been able to observe anything to distinguish them from other ulcers"—i.e., from such as do not perforate. In this he is fully concurred with by the writer of the present article, who published in the 'London Journal of Medicine,' three years ago, a statistical collection of 22 cases of gastric ulceration, from autopsies made at St. George's and St. Mary's Hospitals, in which the same doubt is thrown upon Professor Rokitansky's classification, and the perforating character attributed, not to anything in the nature of the ulcer, but to all ulcers which occur in youth.

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* Frerichs, in Häser's Archiv, x. 175—208; Lehmann's Phys. Chemie, ii. 130.

"The same tendency to strike deep and perforate, exhibited by ulcers in youth, is equally seen in ulcerations of other parts of the intestinal canal besides the stomach, and moreover appears quite independent of the disease which has given rise to the ulceration; it is the young and vigorous whose intestines become perforated, whether the first origin of the evil was pulmonary consumption, dysentery, fever, or chronic inflammation. This fact is proved by the same decennial collection of 2161 post-mortem examinations at St. George's Hospital, which I before quoted. In 128 of these, various parts of the intestines, not including the rectum, were ulcerated, in connexion with tubercular disease in the lungs and elsewhere; and in 9 of the 128, perforation had taken place. The ages of the 9 were as follows, placed in order of their youth—namely, 15, 17, 22, 22, 25, 29, 33, 34, and one young man of age unknown. In 80 other cases, non-malignant ulceration had occurred from various causes, chronic and acute, independent of tuberculosis; and perforation had occurred in several parts (exclusive of the rectum) in 16 instances, as a consequence of the extension of the ulcer outwards. The ages of these 16 patients were, 7, 16, 16, 16, 20, 21, 22, 24, 24, 28, 30, 30, 30, 35, 37, 56. These examples suffice to show exactly the same fact as our table of ulcerations of the stomach—namely, that *all* ulcers tend to penetrate the peritoneum more in youth than in declining years, but that such a tendency is not by any means confined to one period of life, nor to one peculiar species of ulcer."*

Ulceration of the stomach seems to be a state entirely independent of the degeneration before noticed:

"The tissues bordering the ulcer have not presented anything constant or to be specially noticed; sometimes they appear tolerably healthy, sometimes they are diseased in the same way as other distant parts, sometimes they are the seat of blood congestion, but this is not often the case."†

Neither does it appear connected with increased mucous secretion; for though that condition is occasionally present in ulcerated stomachs, it is by no means constant. Indeed, since ulceration is rather a disease of youth, and degeneration and mucous flux belong to old age, there is a sort of antithesis in their pathological history.

Yet both ulceration and degeneration are connected with depressed vital powers, but in the former the cause is acting acutely and destructively, in the latter slowly and with the deposit of compensating tissue.

BLACK MATTER‡ is so frequently found among and in the gastric glands, and, indeed, throughout the whole intestinal mucous membrane, that it becomes a matter of considerable interest to know what it means. Some boldly and vaguely assert that it is an evidence of former "inflammation," and if they agree to use inflammation in the very broad signification given to it by Professor Virchow, viz., any structural change with or without exudation, they are probably right. But if increased afflux of blood, tumefaction and exudation are to be viewed as necessary parts of the process, they have no evidence in the case before us that this necessarily occurs when black matter is seen. Dr. Jones well remarks:

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* On Ulceration of the Stomach, and Ulceration of the Œsophagus, by Thos. K. Chambers, M.D.: London Journal of Medicine, July, 1852.

† Medico-Chirurgical Transactions, p. 97.

‡ Sometimes unphilosophically called "pigment," as if its whole business was to daub the tissues for the amusement of anatomists. One might as well speak of the lily being painted white and the trees green by Nature's colour-box.

> "The very frequent formation of black pigment in the interstices of the air-cells of the lungs, in the substance of various tumours, and the frequent occurrence of chloasmata in the skin, are sufficient to show that abnormal pigmentary deposits are no very wide or extraordinary deviations from the type of healthy nutrition, and that they are by no means necessarily dependent on hyperæmia." (p. 146.)

Dr. Jones is inclined to view black matter as an evidence only of the exudation of fluid containing hæmatine.

About SOFTENING of the stomach our author has no new observation to make, except that the parts of the membrane undissolved show no deviation from the healthy structure, even under the microscope.

THICKENING OF THE SUB-MUCOUS COAT, he follows Andral and others in thinking, has often been mistaken for cancer, and so the number of malignant diseases unfairly swollen. Such may have been the case; for long-continued ulceration, especially if complicated with local inflammation and adhesion of the peritoneum, will sometimes produce very great increase of bulk in the subjacent and neighbouring fibrous tissue. But on the other hand, have not some cases of a real cancerous nature been set down as mere thickening since the observations of Andral? It is by no means uncommon to find in corpses, dead of indubitable cancer, a thickening of the pylorus and neighbouring parts, which, without the distant collateral evidence, no microscopist or pathologist could possibly pronounce to be malignant. If the stomach chanced to be the only part affected, would not this be accounted non-cancerous hypertrophy?

Where are the limits of cancer? What is its essential difference from other abnormal tissues? When Dr. Jones describes the new-formed substance in thickened stomach as "manifestly a growth, augmenting by assimilative power its own tissue, just as a normal tissue does," is he not encroaching on the definition of cancer? Is there any real line of demarcation between it and fibrous tumour? and is not malignancy a question of degree, of more or less cancerous nature, and not of absolute difference? Dr. Jones seems by no means disposed to follow the bare path of imitation in his pathology, and we should have been glad to have seen him take the opportunity presented by the juxtaposition of "thickening" and "cancer," to have placed some notions at the public service. That the so-called "cancer-cell," as a distinctive mark of malignancy without regard to quantity, must be given up, is becoming evident, and suggestions on the subject are much required.

The ground hitherto travelled over has been the same in the two works quoted at the head of this article—viz., the paper in the 'Medico-Chirurgical Transactions,' and the volume which followed it; consisting of deductions made almost entirely from the 100 consecutive post-mortem examinations. In the separate volume there is added a chapter of "Clinical Observations," and some more detailed histories in the previous pages of the work. These are clearly and graphically written, and exhibit well the difficulties which a practitioner has to contend with in the diagnosis of gastric symptoms. Perhaps it would have been better if the name of the physician who attended the patient had been appended in each instance; but in spite of the want of such aid to identification, the writer of the present article has been able to recognise some of his own patients by the history alone—no slight proof of its general correctness.

In the treatment detailed, the most important point seems to be a more general use of astringents than is common in gastric cases. Tannic acid, nitric acid, nitrate of silver and alum,* are freely employed, and with obvious benefit. We should have learnt more of the efficacy of the various drugs had they been given separate, instead of in compound prescriptions. Nitrate of silver, for instance, lies under the serious imputation of being rendered inert by saliva and mucus long before it gets to the stomach, and certainly clinical experience goes far to confirm the accusation. Dr. Jones often gives it, but never *alone*, so that one cannot say whether the benefit which follows is due to the drug or not.

Another important point, to which attention is properly called, is the use of mercury. It may be doubted whether the author does not overdo this, in actually producing salivation, but no one who has tried it can doubt of the efficacy of alterative doses of the drug, such as a few grains of grey powder every night. It probably acts by augmenting the destruction of effete tissues, especially of the blood, and so promoting healthy secretion and absorption. And of course the beneficial alteration is first seen in the most failing part, in this case in the stomach.

Leeches to the epigastrium are also very efficacious in many gastric complaints, and may without inconsistency be combined with astringents. They act probably partly as alteratives, improving the blood by removing effete matters, and promoting absorption of new; but at the same time they relieve the morbid congestion of the viscera beneath the point of application. Applied in moderation, they do not seem to reduce the vital powers. The reviewer had lately under his charge a young woman to whose epigastrium he thought it desirable to apply four leeches every night, yet she continued all the time to increase in flesh, and during one week gained four pounds in weight.

The extracts made from Dr. Jones's work are sufficient, probably, to justify our readers' hope to see him again in this department of science, employed, as now, in illumining the darker regions of pathology with the modest light of truth, instead of the *ignis fatuus* of speculation.

Thomas K. Chambers.

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* The present writer has used with great success in a few stomach cases lately, "*iron* alum," in doses of from three to six grains. It fulfils the double indication of a powerful astringent and of a martial tonic, peculiarly adapted to the anæmia usual in chronic instances of these lesions.

[illegible]

[illegible] *Series of* [illegible] By [illegible] D. [illegible] College. [illegible]

[illegible] of puer[illegible] The [illegible] of puerperal [illegible] a [illegible] but [illegible] to be [illegible] the [illegible] more favour[illegible] with [illegible]

[illegible] suffering [illegible] dangers of [illegible] responsible and so [illegible] attendant [illegible] we need say [illegible] feel [illegible] fever should [illegible] obstetrician. [illegible] the subject. And [illegible] is far from [illegible] and equally extensive [illegible] upon the nature, [illegible] are the views [illegible] and soundness, as to [illegible] we are left either to reconcile [illegible] to make a selection of one [illegible] the thorough under[illegible] after much reading and [illegible] which cannot be [illegible] directly opposed to each [illegible] It is possible that the [illegible] require more caution and [illegible] and perhaps a wider [illegible] Lastly, the reader may reject [illegible] as being, in most cases, worthless, [illegible] or even opposite patho[illegible] fever will always partake of the prevailing character of disease at the time, so that two writers may be describing the same *nosological* disorder, though practically and pathologically a different disease; the opinions and practice of the one will be at variance with the experience of the other, although strictly correct and sound as regards his own.

We would wish to impress upon our readers this important fact, as a check upon a shallow and exclusive system of reasoning, and as the only possible way of reconciling the contradictory descriptions of puerperal fever—viz., that *the prevailing constitution of disease influences an epidemic of puerperal fever, so that different epidemics may exhibit different or even opposite characteristics.*

This axiom is now so incontestably established, that we entered upon the perusal of the work at the head of this article fully expecting its recognition by the author, and anxious to examine, from his point of view, the epidemics he has witnessed in America. In this expectation, however, we have been sadly disappointed, as all his reasoning is based upon the pathological identity of the disease under all circumstances.

No one can deny the eminence of Dr. Meigs' position, nor the influence he wields on the other side of the Atlantic; as little can any one doubt his great experience, shrewd talent, or facility of writing, although his style is spoiled and made quite barbarous by the use of new, foreign, and bombastic phraseology. We concede to him at once, and willingly, that which he claims—the right to form and to maintain his opinions, to see things with his own eyes according to the light which is in him, and to state decidedly his own views, no matter from whom he may differ. Nay more, we accept his facts—i.e., his descriptions of disease as he saw it, although we may neither agree with his reasoning nor with his generalizations. We do not, however, yield to him the right to speak disparagingly or disbelievingly of the opinions of others, because they differ from his own, nor to dispose of their evidence *ex cathedrâ.*

Claiming the "right of private judgment" in its fullest extent, Dr. Meigs sometimes forgets that an equal right exists for his opponents. Frankly and fully admitting and claiming this right, we shall without scruple examine his opinions with freedom and courtesy, but without hesitating to express our dissent from many of them.

The volume is written in the form of letters to his class, as affording greater freedom of expression; but essentially, it is divided into the consideration of "milk metastasis," the state of the blood in childbed fevers, contagion, description of the disease, etiology, diagnosis, and treatment. The chapter on milk metastasis is of interest historically, as showing how easily an assumption may be mistaken for a fact, and how the error is perpetuated by successive writers. It is executed very ably, but we cannot agree with what seems to be the conclusion of the author—viz., that because the opinion that the blood in certain puerperal attacks was rendered impure by the transference of the milk, is unfounded, therefore that all supposition of blood deterioration is equally baseless. Such a notion is rejected and ridiculed in many parts of the book, and is treated as quite inconsistent with the author's pathological views.

"What difference does it make to you," he observes, at page 79, "whether our class shall receive and adopt the milk dogma of Thomas Willis and the Frenchmen" (the "milkmen," as he calls them in another place), "from Goubelly to

Puzos, and from Doublet to Vigarous: or whether they shall prefer to fill the blood with various products of absorptions and resorptions, and then attribute to the noxious presence of those putative foreign matters a host of diseases, whose evolution is made by them to depend upon these alone or in chief?"

But Dr. Meigs' opinion on this subject does not seem so much the result of a careful examination of facts, as the consequence of a preconceived theory of disease. which we are told has prevailed for some time in Philadelphia, and one of whose ablest champions was the late Professor Chapman. Thus, when considering the state of the blood in childbed fevers, he commences by explaining his views of the mode of blood-formation in the fœtus in utero, which he attributes to the endangium. or blood membrane, and whose inflammatory condition (endangitis) is the only possible way by which the blood may be deteriorated. Then passing on to the state of the blood during gestation, he observes:

> "A woman pregnant is often observed to labour as to her circulation; she becomes sometimes plethoric and hyperinotic, at other times she is to the last degree hydræmical. The force of her hæmatosis is exaggerated or exhausted, as the case may be, and the direct fault, the pathological fault, is to be traced to a state of the endangium, which is her blood-making tissue, her blood membrane, and which has the same relation to the function of hæmatosis as the gastro-enteric mucous membrane has to the process of her digestions. If the digestive organs become diseased, the power of digestion fails proportionally; if the hæmatosic tissue becomes diseased, the hæmatosis fails in like manner. We habitually speak of the digestive mucous membrane and of the respiratory mucous membrane,—blood membrane is equally a true and honest word, one that conveys an accurate and concise meaning or idea. Well, then, when I speak to you of blood in diseases as depraved, as vitiated or dissolved, as ruined, and incompetent to carry on the functions appurtenant to it—as the antagonism of the solids—I desire you to understand me as speaking of diseases of the membrana communis (endangium). I do not more conclusively refer the dysenteric diarrhœas, croups, &c., to a state of the gastro-intestinal, pulmonary, or laryngeal mucous membrane, than affections of the blood to affections of the blood membrane—endangium.
>
> "All scarlatinas, measles, variolas, varicellas, erysipelas, gout, rheumatism, and many forms of childbed and other fevers, have their prime seat in the blood membrane (endangium), and are but so many varied expressions of its diseased conditions." (p. 77.)

We have introduced this quotation, that our readers may learn from himself the basis of all Dr. Meigs' pathological views, and not in order to discuss their merits, because it appears to us that whether his opinion of the first step of the morbid process be true or not—and we confess that we cannot regard it as the truth—does not materially affect the most important question at issue, which is rather the constitutional and local effects of depraved blood, no matter by what means it has become impure.

Our limits necessarily prevent our going through the volume *seriatim;* we must therefore select a few of the more salient and important of the questions discussed, and confine our observations to them. Such we may well consider the nature of puerperal fever, its communicability or contagion, and its treatment. By way of giving our readers a *carte du pays,* we may state shortly, that Dr. Meigs denies totally the existence of a primary puerperal fever; he regards the disease, so called, as a local disease with constitutional symptoms, in all cases; he laughs to scorn all notion of contagion; and deems large bleeding the only effectual remedy.

I. Let us now inquire what is the nature of puerperal fever, and whether, under all circumstances and in different epidemics, it is always pathologically the same? Is it always a primary local affection with consecutive fever, or may the fever in some cases or epidemics be primary and the local affection secondary? Is it always of a sthenic-inflammatory character, or occasionally adynamic and typhoid; and if so, are we to assume an identity of morbid process in both instances? Dr. Meigs announces his own opinions clearly enough. For example:

> "At present the question is, is there such a thing as a childbed fever? To this question I am compelled to answer in the negative; wherefore I must consider the word a false and misleading one, since it implies that the disorder is a fever, when, in fact, it is not a fever, but a phlegmasia, or pure inflammation." (p. 121.)

Again:

> "Why, this is truth, that childbed fever is a phlegmasia and not a fever. If you can cure the inflammation, all the rest will be cured *ipso facto.*" (p. 139.)

And again:

> "Whenever these inflammations occur in pregnant or lying-in women, then the disease is childbed fever, and it is nothing else; nor are childbed fevers ever anything else, except when they occur as accidents in typhus, small-pox, plague, and other typhous diseases, of which they are in some instances mere complications." (p. 129.)

The latter clause seems to have been the result of a misgiving that his theory would hardly cover all the cases met with in practice, and it is the only approach in the book to an admission of varieties of disease in different epidemics. The arguments or evidence in support of the author's views consist of quotations from his own experience or that of others, showing that local diseases of various kinds were found, and that they were inflammatory. Now, no one doubts that there are almost always local diseases, and that in many cases they are the result of inflammation. In Dr. Meigs' cases we are quite prepared, on his own showing, to believe that they were so; but this does not prove that it is always the case, nor does Dr. Meigs adduce any adequate evidence to this effect; but he answers evidence and conclusions opposed to his own views with a sneer, speaking of "Doublet, Puzos, Vigarous, Sellé, and the whole host of the milkmen!" and of Dr. Fergusson's valuable essay as "the most misleading and weakest book on childbed fever that has appeared since 1795," which is neither graceful nor conclusive. Throughout the entire examination of the subject, Dr. Meigs assumes that the epidemics he has witnessed were identical in character with all other epidemics, and consequently that his interpretation is equally applicable to all. Now, this is exactly the point on which we must beg leave to differ with the Professor. As we have already said, the evidence afforded by the carefully-recorded experience of others, and some experience we have had ourselves, has satisfied us that epidemics of puerperal fever share in the changes of constitution noticed in other epidemic diseases, some being characterized by sthenic-inflammatory action in the fever, and local affections, while others exhibit an opposite or asthenic type, and the local affections show very little trace of inflammation; and we give Dr. Meigs the benefit of this wide comprehension. We do not doubt his accuracy as regards the cases *he* has seen; we take his statement without question, that they were acute sthenic local inflammations with consecutive fever, and were cured by bleeding, when early employed. But we can as little deny, if we are to judge according to the evidence, that epidemics of an opposite character have occurred, and that of late years this type has prevailed, in this country at least. In coming to this conclusion, we are obliged to do that which is not particularly agreeable to human nature—that is, avow a change of opinion. Formerly we adopted the views so clearly set forth by Dr. Robert Lee in his valuable papers, nor was it until personal observation had convinced us of their inadequacy, that we admitted the modification we have just stated. It would be easy to quote a host of witnesses to the existence of epidemics presenting different characters to those described by Dr. Meigs, but we must content ourselves with two or three. For example, Mr. C. White, of Manchester, describes the cases he witnessed as being "a malignant fever of the same genus as the gaol or hospital fever."

Dr. John Clarke, of London, who was among the first to recognise the prominence and distinction of the local diseases of childbed fever, and whose essay is still one of the most valuable we possess, has also a separate chapter devoted to the consideration of the "low fever of childbed," an affection very different from the acute inflammations which occur at the same period, and which he considers as exhibiting many analogies with typhoid diseases. The local affections he regards as secondary, and the fever as primary or essential.

Dr. Gooch observes, that—

> "The most remarkable circumstance that the experience of the last few years has taught us about peritoneal fevers is, that they may occur in the most malignant and fatal form, and yet leave few or no vestiges in the peritoneum after death. The state of this membrane, indicated by pain and tenderness of the abdomen, with rapid pulse, appears to be not one uniform state, but one which varies so much in different cases that a scale might be formed of its different varieties: this scale would begin with little more than a nervous affection, often removable by soothing remedies, and when terminating fatally, leaving no morbid appearances discoverable after death."

Dr. Meigs' comment upon this statement of so eminent a physician is very characteristic of his mode of disposing of the testimony of an opponent:

> "I wholly deny that a woman can have a childbed fever, according to Dr. Gooch's first scale, and should deeply lament to find a student of mine taking up such an idea; since I cannot conceive of his entertaining it, without at the same time admitting a childbed fever to be a *fever*, which is a doctrine both false and dangerous; false as to the pathology, and dangerous as to the practice—that is, false both exegetically and practically." (p. 143.)

We are satisfied that our American friends will require something more profound and logical than this before rejecting Dr. Gooch's experience. Dr. Douglas states, that judging from what he has himself observed—

> "The contagious puerperal fever of Dublin is, I venture to pronounce, neither more nor less than a malignant fever of a typhoid type, accompanied with an erysipelatous inflammation of the peritoneal covering of the stomach, intestines, and other abdominal viscera."

Dr. Collins has exactly expressed the conclusion to which we are anxious to bring our readers, in the following record of his experience:

"The extreme difference of opinion and the opposite measures recommended by practitioners, arise chiefly, I am satisfied, from their treating of every variety of puerperal fever as one and the same disease; whereas there is perhaps not any other which exhibits a greater diversity of character in different situations, and even in the same situation, at different periods. In some the fever is accompanied by symptoms indicative of the most active inflammation, such as to forbid the least delay in the free use of venesection, and the decided employment of antiphlogistic measures. This form of disease, *which is by far the most manageable, is generally met with in private practice*. Puerperal fever, when epidemic in hospital, is directly the reverse; at least, in *four* epidemics which I have witnessed, the symptoms were usually of the lowest typhoid description, the pulse being so feeble and indistinct, as to make you dread in many even the application of leeches; the patients, in several instances of this form of the disease, exhibiting somewhat the appearance of those labouring under cholera."

Dr. Copland states—

"That a most rapidly fatal and most malignant form of puerperal fever is occasionally developed in lying-in hospitals, which is certainly not characterized by uterine phlebitis, nor by purulent collections in the uterus or its appendages, nor even in some cases by peritonitis; the chief lesions often being merely a remarkable alteration of the blood, general lacerability of the tissues, or loss of their vital cohesion soon after death, with a dirty, muddy, offensive, and sometimes a scanty effusion into the serous cavities."

The last evidence we shall adduce is of the more value from its recent occurrence. Since December, 1854, an epidemic of puerperal fever has prevailed more or less in Dublin and its neighbourhood. An account of this epidemic, as it appeared in the Lying-in Hospital, was read by the present master of the hospital, Dr. M'Clintock, before the Obstetrical Society, last March, from which we have been permitted to lay before the reader the following extracts. After stating the difficulty he felt in classifying the cases, Dr. M'Clintock proceeds:

"There were two features, however, common to them all—viz., a very rapid circulation, the pulse ranging from 120 to 140; and a marked adynamic type, so marked, indeed, that in two cases only did I feel justified in making trial of phlebotomy, and these, as you may suppose, were selected cases; yet in each of them, the supervention of syncope rendered it necessary to discontinue the bleeding—one losing about seven, the other nine fluid ounces; and what is still more worthy of attention, is the fact, that in neither of these instances did the blood exhibit, after some hours' standing, any of the characters indicative of inflammation.

"I think I am justified in asserting, that the prevailing character of the tongue in the late epidemic was a close approximation to what is usually called the 'typhoid tongue,' and this is one symptom wherein it differed from the epidemic of 1845, in which the tongue presented most usually a broad, soft, creamy appearance.

"There seemed to exist throughout the epidemic a strong tendency to putrescence or sloughing of the uterus and vagina, and this, too, quite irrespective of the length or character of the labour. In six cases we had direct proof of the existence of this gangrenous condition; two of these were patients who had sloughing of the vagina, and recovered.

"It has been already stated, that in every instance the pulse was found to be very rapid. At the commencement of an attack it was rarely below 112, occasionally much higher; and as the symptoms became more developed, and the disease made progress, the pulse commonly rose to 130 or 140. The other characters of the pulse were sufficiently marked to render them deserving of notice. In no one instance could we have applied to it the epithet 'incompressible.' On the contrary, it was invariably soft and yielding, and gave to the finger a sensation that is best described by calling it liquid or undulating.

"I believe it may with truth be affirmed, that bleeding in this epidemic was inadmissible. The only cases in which it was tried proved it so, and both of them died; the disease seeming to be wholly unaffected, if not aggravated, by the measure."

After some explanation of the difference between his experience and that of those who have found bleeding efficacious, Dr. M'Clintock continues—

"In support of the supposition here thrown out, I would beg to draw attention to the significant fact, that each of these four authors, Gordon, Hey, Armstrong, and Meigs, the great champions for the lancet in puerperal fever, derived their experience of the disease from private practice only; and it is now well established, that no parallel as to the mode and results of treatment can be fairly instituted between the disease as it presents itself in hospital and in general practice."

Again—

"Wine was ordered to nearly all our cases, and to some from a very early period of the disorder. All the patients who recovered from a bad attack of the complaint, got wine to the extent of eight, ten, or twelve ounces in the twenty-four hours, and this from the second or third day of their illness. Some of them, too, got brandy along with the wine. In forming an estimate of the utility of stimulants, I would wish to express myself with the strictest caution and reserve; but I can with truth say, that on no occasion did I see reason to regret their exhibition, whilst in some cases their good effects did not admit of doubt. If I had to encounter another outbreak of puerperal fever similar to that first described, I should, with my present knowledge, give wine much more freely to my patients.

"After a calm and deliberate survey of the symptoms, treatment, and other attendant circumstances of the late epidemic, viewed in relation to the all-important subject of treatment, the practical conclusion at which I have arrived is embodied in this short precept—'to leech promptly, to purge actively, and to stimulate freely.'"*

During the period embraced by Dr. M'Clintock's report, there were 182 women delivered in the hospital, of whom 38 (1 in 5) were attacked by the disease; 21 died and 17 recovered.

Much more evidence of the same kind might be adduced, but we have surely collected sufficient to show that there is a form of puerperal fever, very different from the form truly and graphically described by Dr. Meigs and others, unless we are prepared to deny either the powers of observation or the truthfulness of the authors who have described it; indeed, a glance at the different rate of mortality would alone be sufficient to justify this conclusion. For example, Dr. Collins lost 56 out of 88 cases, Dr. M'Clintock, 21 out of 38, whilst Dr. Meigs lost only 3 out of 13, and Dr. Rutter (quoted by him) 18 out of 70 cases. What, then, is its real character? It is not truly inflammatory, not sthenic, and the local lesions are trivial at the outset of the attack, and for hours or days afterwards; nay, in some cases there seems scarcely any local disease. For our own parts, we are constrained to believe it to be a genuine essential fever, as much so as typhus, occurring after delivery, and accompanied, though not necessarily, by local affections, naturally to be expected under the circumstances. There are several considerations which seem to confirm this view.

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* Dublin Quarterly Journal, May, 1855.

1. It exhibits a very close resemblance to typhus fever in its general type, its symptoms, its course, many of its complications, and in its treatment, as has been observed by several authors. This analogy has been the subject of a paper (unpublished) by Dr. H. Kennedy, read before the Dublin Obstetrical Society, Jan. 1849, from which I am permitted to make the following extract:

> "If we leave out," he remarks, "Dr. Fergusson's first division of puerperal fever, we shall find that the remaining three are exactly represented in typhus fever. The 'gastro-enteric' form of puerperal fever has, as far as I have seen, an exact analogue in typhus, and the same may be said of the 'nervous;' and should we include a 'cerebral' form, who does not know that it is to be met with in puerperal fever, and may so simulate typhus as to render it next to impossible to distinguish the one from the other, taking only the existing symptoms into account? The 'complicated form,' which is Dr. Fergusson's fourth division, what is it, in point of fact, but what any one in the habit of seeing typhus fever must recognise at once—that is, a fever which is attended or followed by secondary inflammations, coming on usually with great rapidity, and consisting of effusions of serum, lymph, blood, or pus into any or all parts of the frame? If the morbid appearances which the uterus presents be excepted, every other peculiarity has come under my notice as a complication of typhus fever, including peritonitis, and all the varied forms of what is known as diffuse inflammation. I have repeatedly also seen the affection known as phlegmasia dolens, coming on after fever, and in men. As to the general features of the two affections, they are in all respects analogous—that is, the types of the fever which one presents are to be seen in the other."

Let it be remembered also, that it often happens that the two diseases prevail epidemically at the same time, that to a certain extent they are promoted by the same causes, and are under similar atmospheric or climatic influences.

2. The analogy between puerperal fever and erysipelas is so close that by many the former is regarded merely as an erysipelatous affection of internal structures. They are influenced by the same climatic causes, occur epidemically at the same time, exhibit the same type of fever, and may co-exist in the same patients.

3. But we may go a step further, for we possess evidence to show that either of these diseases may excite puerperal fever in women in childbed. Take the following statement by Dr. Collins, as regards typhus fever.

> "When Dr. Labatt was master of the hospital, puerperal fever commenced its attack on one occasion in the following striking manner. A patient was admitted at a late hour at night into one of the wards, labouring under a bad form of typhus fever, with petechial spots over her body; when observed next morning, she was removed into a separate apartment, where she died shortly after. The two females who occupied the beds adjoining hers while she remained in the large ward, were attacked with puerperal fever, and died. In October, 1827, when I was resident as master, an occurrence precisely similar took place. A patient in typhus fever was admitted at night into one of the labour wards, where she remained for some hours; the ward contained four beds. The three women occupying the other beds were attacked with puerperal fever, of whom two died."

Dr. Gordon lately related the case of a woman who remained in the Hardwick Fever Hospital for some time after recovery from a slight attack of fever. She became ill again, with symptoms of puerperal fever, was prematurely confined, and shortly died of that disease. These cases appear to us decisive of the power of typhus to excite puerperal fever.

The evidence on record showing that the contagion of erysipelas can excite puerperal fever is equally, if not more, conclusive, as Dr. Copland has shown in his valuable dictionary.

> "Dr. Holmes (an American author) notices, in his instructive memoir, that Dr. S. Jackson went from a case of gangrenous erysipelas, which he had been dressing, to the first of a series of cases which took place in his practice.
>
> "Another physician, who had a series of five successive cases of puerperal fever, states that for two weeks previously to the first case of puerperal fever he had been attending a severe case of erysipelas."

Dr. Ramsbotham also mentions several cases, and Dr. Copland concludes

> "That the evidence is altogether satisfactory, that some of the series of the more malignant states of puerperal fever, have been produced by an infection originating in the effluvia proceeding from erysipelas, or by the contagion of the matter or contaminating material produced by erysipelas."

We have received permission to quote the following cases by the medical gentleman who attended them. This gentleman was in attendance upon a bad case of erysipelas, and had just dressed the part affected, when he was summoned to a midwifery case in the country, without being allowed to return home to change his clothes. The lady had been previously in good health, had a favourable labour, but on the second day she was attacked by puerperal fever, of which she died.

Another lady, whose labour was tedious and difficult, was delivered by the forceps, and on the seventh day was attacked by severe erysipelas of the neck, back, and legs, of which she died, but without abdominal tenderness or any other symptom of puerperal fever. Previous to her death, the nurse, who was also a midwife, was sent for to attend another lady in her confinement. On the second day after delivery she was attacked by genuine puerperal fever, of which she died.

Dr. Ramsbotham has shown, further, that erysipelas may be the result of exposure to the contagion of puerperal fever in the nurses, as in the following example:

> "In 1841, when erysipelas was prevalent in Rotherhithe, a medical friend had six cases, and whilst attending these he delivered a lady, who was speedily seized with puerperal fever, and very soon afterwards died. Her nurse was attacked by erysipelas of the hand, and was attended by another surgeon. One day, after having made an incision and dressed the wound, this latter surgeon was called to a case of midwifery: puerperal fever supervened, and the patient sunk very rapidly."

Now, here we have puerperal fever produced by the contagion of typhus fever and of erysipelas, but we can scarcely infer from that its identity with both these diseases; or if so, we must go a step further, and admit the identity of all three diseases. What element, then, is common to all three, for this may lead to a solution of the problem. It can neither be the local affection, nor the mode in which the exciting cause is applied. The most striking characteristic common to all is the low fever of a malignant type; the fever is certainly accompanied by local affections, but these are very different in their seat, however much they may resemble each other pathologically.

4. There is good reason for believing, and this opinion is held by Rokitansky and Semelweiss among others, that puerperal fever may be excited by morbid matter adhering to the hands of those pupils who leave the dissecting-room to attend midwifery cases, and that irrespective of the disease of which the subject has died. We know, also, that puerperal fever has been apparently caused by those who attended cases after making a post-mortem examination of women who have died of the disease. Thus, in addition to general causes communicated by way of infection, we have local causes transmitted by contact internally, and to be satisfactory, our explanation should embrace all.

Now, if puerperal fever has this close relation to typhus and erysipelas, so that it is capable of being produced by either; if the primary character of the fever is the characteristic common to all; and if the state of the blood in typhus and erysipelas be the most essential pathological change, it is fair to conclude that this is shared also by puerperal fever. But some similar change in the blood is the effect of poisoned wounds, and it is not unreasonable to suppose that morbid animal poison, applied by the fingers of the accoucheur to the vaginal mucous membrane, will likewise act by inducing a morbid change in the blood, and, if so, we arrive at a new pathological element, common to all the cases we have mentioned—viz., that puerperal fever consists essentially in a morbid condition of the blood, whether produced by contagion or infection *ab extra*, or by absorption within the body.

Dr. Meigs sees an objection to the absorption of pus or morbid matter from the uterine surface, in the fact that the pus globules are too large to enter the minute vessels; but he forgets that the *liquor puris* may be absorbed, or that, as Dr. Bennett has suggested, the morbid poison may be gaseous, resulting from the decomposition of pus or other morbid matter.

But another question remains, what is the relation of the local lesions, and their pathological character? We have adduced what appears to us very strong arguments to show the essential character of the fever, which of course involves the supposition that the local diseases are secondary; and if, in addition, we recal what Drs. Gooch, Copland, and others have observed—that fatal cases may occur, though rarely, without local lesions, yet with all the other characters of puerperal fever—this conclusion seems almost inevitable. Dr. John Clarke, who so admirably described these affections, observes, when speaking of the low fever of childbed, that "another question arises, whether the affection of the abdomen should be considered as the primary disease and the fever symptomatic, or the fever the disease and the affection of the abdomen symptomatic? I own that I am inclined to favour the last supposition." Dr. Fergusson considers that the local diseases, "phlebitis, or peritonitis, or metro-peritonitis, are only secondary effects of one cause."

If these local affections be secondary in this peculiar form of epidemic puerperal fever—and we are concerned at present with no other—are they all of an inflammatory character, or may some of them at least be the result of other modes of morbid action? It would perhaps be rash to speak dogmatically upon the subject, which is a very difficult one, but when we consider the nature of the secondary lesions in typhus fever in the light which recent researches have thrown upon them, it does not seem improbable that the analogy to which we have alluded between malignant puerperal fever and typhus may extend beyond the question of the primary character of the fever; and that at least some of the local changes—for example, purulent deposits, softening and putrescence of the uterus or pelvic viscera—may not always be due to inflammatory action; and certain it is that the post-mortem appearances do not contradict this assumption. As an example, we quote the following case, the details of which, with the morbid specimen, were recently submitted to the Dublin Obstetrical Society by Dr. M'Clintock. A young woman in her first labour was delivered by the vectis of a living child, after having had two well-marked convulsions, with an interval of some hours between them. The urine drawn off by the catheter during labour was highly albuminous. The pulse remained quick from the time of delivery. In twenty hours the belly was full, tympanitic, and extremely tender over the uterus, which was large; pulse 120. Twenty leeches were applied to the hypogastric region, which removed the pain and tenderness, but did not improve her condition, and she died in thirty-six hours from the time of delivery. Autopsy:—No trace of peritonitis. Kidneys in second stage of Bright's disease. Uterus large, soft, and flaccid. The whole interior surface presented a red, soft, gelatinous appearance, as if smeared with red currant jelly. There was no sign of inflammation or putrescence. The ovaries were pale, and softened to such a degree as to be almost pulpy.

On this very interesting subject we cannot resist quoting the remarks of one of the most philosophic physicians of the present day:

> "Notwithstanding all that has been done," observes Dr. Stokes, "in that branch of pathological medicine which treats of the local affections secondary to essential disease, many practitioners consider them as original inflammations, and to this is to be attributed a large portion of the errors in diagnosis and practice, so much to be deplored at home and abroad. If we inquire why it is that so many do not receive in its full extent the doctrine that a vast number of acute diseases cannot be explained by the theory of primary inflammation,—and again, that when inflammatory action does occur in them, it is of a secondary, reactive character, acting on tissues already altered by a process of a different kind, which is itself subsidiary to a general and essential condition,—we find that the reasons are manifold. The opinions to be overcome are the growth of many years, and may fairly be dated from the period when accurate anatomical investigation was applied to elucidate diseased structure. As might be expected, an almost exclusive attention was directed to those manifest changes induced by inflammation, so striking, not only in their earlier stages, but also in the successive periods of the process. And thus the doctrine of solidism, while it replaced the humoral theory, came to mean more than its name would imply; for while it referred diseases to an alteration of the solids, it taught that these changes had a common character dependent on some stage or phase of the inflammatory process. Alterations unattended with increased vascularity, tumour, or ulcerative action, were naturally overlooked, and this the more readily when no symptom of pain or disturbed action had been observed during life. Thus the facility which presented itself from the multiplication of instances of inflammatory action in all the great organs, and of connect-

ing symptoms with some stage of these changes, paved the way to the doctrine of Broussais, which referred so many diseases, both general and local, to irritation or inflammation. Naturally captivating from its being, as it were, the first fruits of so much past labour in a new direction,—specious in its application to a general theory of medicine, and supported by the bold writing and great experience of its author,—this doctrine soon became on the Continent an accepted medical creed. In the British Isles, too, though more cautiously received, it influenced the minds of students and of the younger members of the profession. . . . . The unhappy division of the profession into separate corporations of medicine and surgery operated strongly in extending the adoption of an inflammatory theory of disease, for in the wards of a surgical hospital the surgical student saw little or nothing but the effects of inflammation. Thousands of practitioners were created who had never seen a case of typhus fever, and who, even when other forms of adynamic diseases presented themselves, were misled by the specious terms of diffuse or erysipelatous inflammation. It is difficult to change the opinions in which we have been brought up, and it often happens, especially in medicine, that when an opportunity is given of extended experience, the mind, from age, habit, or indolence, has become unfit to profit by that opportunity. When we reflect on this, and that for many years a large proportion of British practitioners have received an education almost wholly surgical, and have entered on practice with but scanty knowledge of the non-surgical and the essential diseases in this country, and still less of those met with over the wide surface of the British dominions, we can understand how extensive must have been the application of antiphlogistic treatment to disease which was itself antiphlogistic."*

Having examined this very interesting subject as fully as our limits will permit, we shall, in conclusion, lay before our readers a summary of the conclusions at which we have arrived.

1. That the characteristics of puerperal epidemics vary as widely and as essentially as do those of other epidemics, and may be either of a sthenic or asthenic nature; from which it follows, whilst they may be grouped together on the ground that they all occur after delivery, and involve certain organs equally, these pathological peculiarities require a different classification, and all attempts to apply the same theory and the same rules of practice to all must necessarily fail.

2. That we recognise, upon adequate testimony, and from our own experience, epidemics of puerperal fever, of which the general type was sthenic or inflammatory, and the local affections the result of inflammation: nor do we deny that the local disease may have been primary and the fever secondary, whilst the result of early and active antiphlogistic treatment was in accordance with such character of the disorder.

3. That we should be rejecting testimony of the very highest authority, and throwing aside the results of our own observations, did we not also admit that epidemics of puerperal fever do occur, especially in lying-in hospitals, of quite an opposite character, in which the general disease is of an adynamic type, presenting typhoid symptoms, and in which the local lesions exhibit a subordinate and secondary character.

4. That judging from analogy, from the mode of production and extension, and from certain peculiar exciting causes, it appears highly probable that the essential pathology of the "typhoid," "malignant," or "low childbed fever," consists in a morbid condition of the blood, whether induced by morbid influences applied by the way of contagion or infec-

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* Stokes on the Diseases of the Heart and Aorta, p. 436, note.

tion, or by the absorption of morbific matter, either fluid or aëriform, within the body.

5. It seems most probable that the local lesions in the typhoid epidemics of puerperal fever hold an analogous position to the local affections of typhus fever and other typhoid diseases—i.e., are pathologically secondary, and the consequence of the general disease.

6. As in the local affections of typhus fever, so in those of puerperal fever, there is at least room for doubt how far they are the results of inflammatory action.

II. Let us now turn to the question of contagion—a subject of great interest and difficulty in itself, but which has been rather obscured than explained by the loose and general way in which it has been treated by medical writers, and which, by the popular view taken of it, is often the cause of great distress to the friends of the patient, and often of great injustice to the medical attendant. At present the popular opinion is, that the disease is a contagious one, *therefore* every case that occurs is attributed to the accoucheur having conveyed it from one patient to another.

Under the term contagion, or infection, is included not merely the possibility of the disease being communicated, by a patient labouring under it, to a woman recently delivered, but also its communicability by a third party not suffering from it,—as the accoucheur, or nurse. In both aspects the question is one of extreme interest, but our object now will be rather with the latter view, as one which has a practical though very painful bearing on the profession. We are met at the outset by the difficulty which attends every attempt to define or prove the contagious nature of any disease which is admitted to be epidemic—viz., that the latter influence will explain equally well almost all the examples adduced in favour of contagion, inasmuch as the conditions requisite for contagion or infection involve those calculated to promote the spread of an epidemic: viz., breathing the same air, and being exposed to the same atmospheric influences. Under such circumstances it is very hard to draw a marked line of separation, and moreover it is quite possible, as in the case of cholera, that an epidemic may be first lighted up by contagion, but afterwards spread independent of it. The question has divided the highest authorities in obstetric literature: on the one side we find such names as Hulme, Leake, Hull, Baudelocque, Tonnellé, Dugès, Jacquemier, Kiwisch, Dewees, &c., denying its contagiousness; and on the other, Gordon, Clarke, Denman, Burns, Hamilton, Blundell, Gooch, Ramsbotham, Locock, Douglas, Rigby, Channing, Holmes, Copland, &c., affirming it.

We would venture to suggest, that possibly some explanation may be found of this contrariety of opinion, in the different character of different epidemics, to which we have so often alluded. May it not be that whilst the typhoid or malignant childbed fever is contagious, the sthenic or inflammatory variety may be less so, or perhaps not so at all?

It should also be kept in mind, that even if it could be demonstrated that the disease may be propagated by contagion, this is but one mode of its extension, and certainly the least influential.

Dr. Meigs ranges himself among the anti-contagionists, and charges à *la Zouave* at all who differ from him. His arguments, however, have rather the appearance of being the consequence of a preconceived opinion as to the nature of the disease, than the result of an impartial examination of evidence. Thus, he observes:

> "If, as some pretend, childbed fever is a fever indeed, and not a mere topical inflammation, that gives rise to febrile phenomena by means of the irritating power of the local disease, there might remain some chance of a probability that such an affection should be a contagious one. I know not what ideas you may have imbibed upon these points, but it is the object of this work to prove that childbed fever is a simple state of inflammation in certain tissues of pregnant women, and of women lately confined, and that the fever that attends it is a natural effect of intense constitutional irritation from the local disorder. So far as I have been able to investigate the subject by reading and by clinical observation, as well as by microscopic researches, I rest deeply convinced that the fever does not take the initiative, except in very rare instances; but on the contrary, that an area of inflammation being first established, the reactions issue thereupon: and I beg you to observe, that in all truly contagious disorders the constitutional affection leads the train, and brings on the topical lesions after an indispensable period of incubation." (p. 87.)

Now, if there be any truth in the conclusions stated in the former part of this review—viz., that in certain cases, and in certain epidemics, childbed fever is an essential disease, it follows, according to Dr. Meigs, that it may in such instances be contagious; we do not contend for more than this, and the reader will decide for himself how far we have shown grounds for this conclusion.

Dr. Meigs' next argument is, that contagious diseases are always specific, and can only give rise to similar diseases, "and further, they affect the race of mankind without respect to age or sex of those who become exposed. There is no presumable exception among men." Both these propositions are true to a great extent, especially if we confine our attention to the local affections; but to both there are many exceptions. We are by no means sure that male attendants may not take a fever from puerperal patients; one fatal case came under our observation, in which a medical student, in attendance at the lying-in hospital, was attacked by fever exhibiting all the general characteristics of the epidemic which then prevailed; though if Dr. Meigs insists upon the presence of the local affections to constitute similarity of disease, we must admit that they were absent!

Even Dr. Meigs admits that pregnant women may take the disease, and we have also seen that nurses may be attacked by erysipelas when attending upon puerperal fever.

Again, Dr. Meigs observes that "it is an absolute condition of contagion, that it must undergo some certain incubation. If it is a ferment, it must have time to ferment; if a spore, it must have time to develop spores; if infusorials, or infusorial ova, there must be time for incubation. Such is the law of variola, vaccinia, measles, varicella, &c." And then he quotes a case from Tonnellé, in which death took place in twenty hours, and another from Kiwisch, fatal in eight hours.

> "Many a woman has entered upon her labour in apparent health, and has scarcely given birth to her offspring before she is attacked by metrophlebitis, and then been deprived of existence within less than twenty-four hours. Forty-eight

is frequently time enough to effect the dissolution. Are these inoculations? Are these contagions? Are these infections *ad distans?* It is nonsense to say so." (p. 90.)

Of course, if Dr. Meigs could fix definitely the period necessary for incubation, his argument would be conclusive against those cases in which no such period elapsed. But he must know that in the majority of cases the disease does not show itself till the third or subsequent days, which does allow an interval for incubation, so far as we know, long enough. However, we are far from contending that the disease is chiefly propagated by contagion, although we think that sufficient evidence has been adduced to show the probability of this being one mode by which it is extended. We believe that its spread is far more due to its epidemic character, although we agree with Professor Dubois that it is almost impossible to say how much is due to contagion, and how much to epidemic influence, though few who have had much experience in hospitals will be inclined to deny the influence of contagion altogether.

Leaving the general question of contagion from one patient to another, let us for a short space examine into the evidence in favour of the communicability of the disease by a third party from a patient labouring under it to another during or after her delivery. The exact value of the facts on record will be better estimated by a little classification.

1. It seems impossible to doubt that contagious matter capable of exciting puerperal fever may be conveyed by a third party unaffected by it; for example, in the cases already mentioned of puerperal fever following the services of medical men and nurses who were in attendance upon erysipelas immediately before. The cases are too remarkable and too numerous to be regarded as coincidences, nor would even the prevalence of an epidemic of puerperal fever at the time invalidate our conclusions; it might certainly render the cause more influential.

2. It is the recorded opinion of Rokitansky and Semelweiss that morbid matter acquired in the dissection of subjects not dying of childbed fever may be conveyed by the dissector and excite the disease in a patient delivered by him; and to this, among other causes, has been attributed the prevalence of puerperal fever in the wards of the Vienna Lying-in Hospital. A celebrated foreign practitioner attributed two outbreaks of this disease among his private patients to his having handled morbid specimens just before attending the patient in her accouchement.

3. We should, therefore, have less difficulty in believing that similar effects may be produced by those passing from the dissection of puerperal patients to the delivery of healthy ones, especially if the most rigorous precautions were not observed. For instance, in the autumn of 1821 Dr. Campbell, of Edinburgh, attended the dissection of a married woman who died of the disease, after an abortion of the early months; he removed the pelvic viscera and external parts, and carried the whole in his coat pocket to the class room. The next morning, dressed in the same clothes, he assisted with some of his pupils at an instrumental delivery at Bridewell. This woman was seized with the same affection, and died. The same night he accompanied Dr. Orr to the delivery of a woman residing in the north back of the Canongate; she was equally unfortunate; and three other poor women shared the same fate in quick succession. In a

subsequent year, 1823, he assisted at the dissection of a childbed fever case, but could not wash his hands with the care he desired; thence he went to attend two other women in labour, both of whom died.

At a meeting of the College of Physicians, Philadelphia, U.S., Dr. Warrington stated that after assisting at an autopsy of puerperal peritonitis he was called upon to deliver three women in rapid succession. All these women were attacked with different forms of what is commonly called puerperal fever.

> "Mr. Davies states that in the autumn of 1822 he met with twelve cases, while his medical friends in the neighbourhood did not meet with any, or, at least, with very few. He could attribute this to no other cause than his having been present at the examination of two cases, and his having conveyed the infection to his patients, notwithstanding every precaution."
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> "A young surgeon shortly after examining the body of a sporadic case that had died, delivered three women, who all died of puerperal fever."
>
> "Mr. Ingleby states that two gentlemen, after the post-mortem examination of a case of this disease, went in the same dress, each respectively, to a case of midwifery. The one case was attacked in thirty hours afterwards, the other in three days. One of the same surgeons attended, in the same clothes, another female, and she was attacked on the evening of the fifth day, and afterwards died."*

Now with regard to the cases attended immediately after the post-mortem dissection, there seems little room for doubt as to the exciting cause of the fever. It may have been conveyed in the clothes or on the hands of the accoucheur, but it is, at any rate, adequate to the effect, and the sequence is too simple and too close to be rejected.

4. Can we venture to say the same of the following case:

> "Dr. Merriman mentions in the 'Lancet' for May 2, 1840, that he was present at the examination of a case of puerperal fever at two P.M. *He took care not to touch the body*. At nine o'clock the same evening he attended a woman in labour; she was so nearly delivered that he had scarcely anything to do. The next morning she had rigors, and died in forty-eight hours."

We do not know whether puerperal fever was epidemic at the time, but the cause suggested seems so inadequate that we should be inclined to look for some other explanation.

5. So far, then, we have seen medical men engaged in handling morbid matter, their dress and persons exposed to the effluvium from dead bodies, and passing directly to attendance upon lying-in women; here we have a distinct appreciable exciting cause, adequate to the production of disease in healthy persons, and which may have been, and probably was, conveyed to the patients who were first attended, and in whom puerperal fever appeared. But in several instances the disease was not confined to the first women attended, but appeared in others delivered successively. How are we to explain this, and how can we explain the pertinacity with which puerperal fever seems occasionally to track the footsteps of one or two practitioners, whether at first lighted up by morbid matter derived from dissection or not? Take the following examples. Dr. Gooch mentions that—

> "A general practitioner, in large midwifery practice, lost so many patients from puerperal fever, that he determined to deliver no more for some time, but that his partner should attend in his place. This plan was pursued for one month, during

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* Copland's Dictionary. Art., Puerperal Fever.

which not a case of the disease occurred in their practice. The elder practitioner being then sufficiently recovered, returned to his practice, but the first patient he attended was attacked by the disease and died."*

This latter fact seems to us to prove that the disease was epidemic at the time. Similar instances have come to our own knowledge more recently.

"Dr. West, of Philadelphia, states that seven females delivered by Dr. S. Jackson, in rapid succession, were all attacked with puerperal fever, and five of them died. These were the only cases that occurred in that district, for the women became alarmed, and sent for other assistance."

"A physician in Boston, U.S., had the following consecutive cases:—On March 24th, April 9th, 10th, 11th, 27th, and 28th, and May 8th, seven in all, of which five died. He then left town."†

Another physician writes to Dr. Holmes as follows:

"The first case was in February, 1830, during a very cold time. She was confined on the 4th, and died on the 12th. Between the 10th and 28th of this month I attended six women in labour, all of whom did well except the last, as also two who were confined March 1st and 5th. Mrs. E., confined February 28th, sickened and died March 8th. The next day, March 9th, I inspected the body, and the night after attended a lady, Mrs. G., who sickened and died on the 16th. The 10th I attended another, Mrs. B., who sickened but recovered. March 16th I went from Mrs. B.'s room to attend a Mrs. H., who sickened and died on the 21st. The 17th I inspected Mrs. G. On the 19th I went directly from Mrs. H.'s room to attend another lady, Mrs. G., who also sickened and died on the 22nd. While Mrs. B. was sick on the 15th, I went directly from her room, a few rods, and attended another woman, who was not sick. Up to the 20th of the month I wore the same clothes. I now refused to attend any labour, and did not until April 21st, when, having thoroughly cleansed myself, I resumed my practice, and had no more puerperal fever. The cases were not confined to a narrow space. The two nearest were half a mile from each other, and half that distance from my residence. The others were from two to three miles apart. There were no other cases in their immediate vicinity."‡

Dr. Ramsbotham has known the disease to spread through a particular district, or to be confined to the practice of a particular person, almost every patient being attacked by it; whilst other practitioners had not a single case; and he considers the distemper as being capable of conveyance not only in common modes, but through the dress of the attendants on the patients.

In Sunderland, 40 out of 53 cases occurred in the practice of one surgeon and his assistant.

Dr. Roberton, of Manchester, states, that between the 3rd of December, 1830, and January 4th, 1831, a midwife attended 30 patients of a public charity, 16 of whom had puerperal fever, and all died. Other midwives of the same institution attended 380 women during the same time, and none suffered from it. He also mentions the case of a practitioner, who introduced the catheter for a poor woman in puerperal fever, late one evening, and attended a lady in her confinement during the same night, who was attacked with puerperal fever on the second day.

Analogous cases have been recorded by Dr. Pierson, of Salem, U.S., Dr. Peddie, and Mr. Beecroft; and such examples are, doubtless, very startling, and require a careful examination, to ascertain their exact value as bearing on the question at issue; but we shall first hear what Dr.

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* On the more important Diseases of Women. † Copland's Dictionary. ‡ Ibid.

Meigs says on the subject. His first argument is from personal experience.

"I have practised midwifery for many long years. I have attended some thousands of women in labour, and passed through repeated epidemics of childbed fever, both in town and hospital. After all this experience, however, I do not, upon careful reflection and self-examination, find the least reason to suppose that I have ever conveyed the disease from place to place in any single instance. Yet for many years I carefully considered whether such a transfer by a third person might be possible, and carefully read the statements of various authors to that effect. In the course of my professional life, I have made many necroscopic researches of childbed fever, but never did suspend my ministry as accoucheur on that account. Still I certainly never was the medium of its transmission. I have, in numerous instances, gone from the bedside of women dying of childbed fever, whether sporadic, or the most malignant degree epidemic, without making my patients sick. I have also endeavoured to assist my brethren, when they had such cases and I had none.

"In a series of labours, 468 in number, and beginning with No. 1, I find that Nos. 18 and 19 were affected, and that No. 18 died with childbed fever; No. 31 was sick, but recovered; Nos. 195 and 259 were sick, but recovered; but 291 died, as did also 293. Nos. 332, 339, 435, 444, and 445 were attacked and recovered. The above cases—viz., 18, 19, 31, 195, 259, 291, 293, 332, 339, 435, 444, 445, 455, are, in all, 13 cases in 468 labours, of which 3 died and 10 recovered. Now, if I was the medium of contagion for any one of that series of 468 confinements, why did I poison them in the ratio and order above set forth; and why did I not communicate the disease in more than 13 out of 468 cases? What became of my nebula from 31 to 195; to 259, and between 291 and 445, and so to the end, or 468? Such a table is far more easily explained by regarding the falling-out of the cases as coincidences and accidents, than as material causations, through a private pestilence." (p. 102.)

Again, as regards the singular limitation of the disease to the practice of one person, Dr. Meigs observes:

"At page 631 of my work 'On Obstetrics,' second edition, I have related the circumstances attending the practice of a physician of Philadelphia, who, in one of our epidemic seasons, lost a considerable number of women in childbed. His patients were scattered over a great superficies of the city and districts, some of them being more than two miles from the others. At that time many women were attacked, in various parts of Philadelphia, as well as in the State of Pennsylvania; yet so far as has come to my knowledge, no other medical gentleman happened to encounter such a great number of childbed fevers as he did. I visited, in consultation with him, some of the very worst of the cases, and touched the patients, and was as liable to imbibe or to be clothed with the effluvia from their bodies as he was; nevertheless, I did not carry poison or other cause of disease to any patient of mine; and if not I, then how should he become capable of doing so? He is a gentleman who is scrupulously careful of his personal appearance, of great experience as a practitioner, and well informed as to modern opinions on the contagion of childbed fever. Still those of you who are contagionists will say that he carried the poison from house to house, and if so, then you ought to give some *rationale* of the fact. Did he carry it on his hands? But a gentleman's hands are clean. Did he carry a nebula or halo about him? Then why not I also? If the nebula adhered to his clothing, it might as well have adhered to mine.

"What will you say, young gentlemen, of the experience of my friend, Dr. D. Rutter, formerly of Philadelphia, but now of the city of Chicago, who passed through terrible scenes here, in an epidemic of childbed fever, some years ago, when he had a most extensive midwifery practice in town and country? During that sad time, I saw several fatal cases with him in consultation; and though he seemed to be tracked by the cause of the disease, to judge by the numerous attacks of it in his lying-in patients, I was not tracked by it. I took no precaution, except such as every decent man should be supposed always to take; yet I never did carry the disease from his cases to any houses where I visited lying-in women. But he was charged with being a carrier of contagion. How could he carry the cause? What was the cause? Was it some ozone that stuck to his hands or coat? Was it a nebula, a halo, or a miasm that mixed with the hairs of his head or the woollen or cotton fibres of his dress? or an exhalation from his skin, or a halitus from his lungs, like the fiery breath of Cacus? And can you say of him, as Virgil sings—

> " 'Faucibus ingentem primum, mirabile dictu
> Evomit.'—*Æneid*, lib. viii. p. 252.

Come now, was not such a poison more sticky than bird-lime, seeing that Dr. Rutter, worn out with fatigue, and wounded in spirit by his cares for the unfortunate victims of an epidemic disease, left the city for the purpose of gaining some strength, and to escape from the repetition of such disheartening labours, and that even a quarantine could not liberate him from this poisoned cloud? One might hope it would have been blown away by the wind, or that it would have evaporated or become too dilute to kill, after a ride of seventy miles, and an absence of ten days. But it happened, after this rustication of ten days, at a distance of thirty-five miles from the city, that your bird-lime or cloud still adhered to him, as your contagionists would say. And more than that, he could not even wash it away or shave it off; for upon coming back to the city, and to his professional toil, before he engaged in practice again he caused his head to be close shaved; he entered a warm bath and washed himself clean; he procured a *new wig, new clothes, new hat, new gloves, and new boots.* He did not *touch* anything he had worn, and took the precaution to leave his pencil at home, and his watch. Well, what do you think happened next? He went out to attend a lady in labour, who had a favourable parturition, yet was next day assailed by a horrible childbed fever, of which she died, in spite of all his efforts, and mine to help him; for he called me in consultation immediately after being summoned himself to her chamber. I know that that lady died with peritonitis. I was a great deal with her in her illness, but she did not poison me or my clothes; for although I went on with my practice, I poisoned nobody, and made nobody have even so much as a finger-ache.

"Dr. Rutter repeated this attempt at personal disinfection at a subsequent period, which was two years later, and with the same ill-success. The gentleman was much and disparagingly spoken of on account of the above-mentioned events in his practice, which I cannot but regard as both cruel and unjust, particularly as his success in the treatment was most brilliant; for during the epidemic he had charge of 70 cases, of which he lost only 18, and I know not the man who can boast of a higher triumph of his art of healing in this malady." (p. 102.)

Let us now look a little closer into this matter. The broad fact apparently established by the foregoing observations is, that puerperal fever does sometimes prevail chiefly, or is altogether limited, to the patients of certain practitioners, and the question arises, To what is this owing? The question is *not,* whether contagion is the only, or the chief, or the ordinary means by which the disease is propagated; for it is admitted on all hands to prevail epidemically. Nor is it the question, whether, under favouring circumstances, contagion may not be conveyed to the patient by the accoucheur, for we have related cases in which it seems impossible to doubt that this took place. We must therefore eliminate from the foregoing examples the case of the physician who wrote to Dr. Holmes, because, having made post-mortem examinations, his experience may rather be referred to Section 3. In most of the instances, we are not told whether the practitioner examined the bodies after death; if they did, we cannot deny that there was a possibility that they might have carried the infection.

Again, if, as Dr. Rigby remarks, "the discharges from a patient in puerperal fever are highly contagious," it is at least possible that the case of the midwife mentioned by Dr. Roberton may be thus explained, inasmuch as her duties about her patients would necessitate more or less contact with the excretions.

Excluding these classes of cases, evidence enough remains to show that the fever does sometimes follow in the track of particular accoucheurs; and the real question before us is, whether it does so by contagion conveyed by him from other patients, in spite of the ordinary precautions, or in certain cases, notwithstanding the extraordinary precautions of baths, change of air, change of clothes, &c., or whether in such cases the prevalence of an epidemic of puerperal fever is a sufficient explanation, admitting it to exhibit caprices similar to other epidemics. It is impossible to bring the matter to a demonstration either way, difficulties meet us upon either supposition, and perhaps the best plan to adopt will be for us to weigh these difficulties separately.

Against the explanation which attributes, with Dr. Meigs, all to epidemic influence, is the fact of its greater prevalence in the practice of certain medical men, and its being in some cases apparently limited to them. That one man should see more cases than another of any epidemic disorder is common enough, and would be no difficulty in the present case; but that one should see all, and others none, does seem rather startling. But is the proof of the latter sufficiently conclusive and sufficiently extensive? Dr. Gooch does not tell us whether the disease was epidemic or not, nor does Dr. West. The gentleman who wrote to Dr. Holmes states that no other cases occurred in the vicinity, but we have rejected his example as being one of *possible* contagion on the ground of his post-mortem examinations. In Sunderland there were at least thirteen cases which occurred in the practice of others, besides the surgeon and his assistant. The two examples related by Dr. Meigs occurred during epidemics. So that it must be confessed that the evidence we possess to show the insufficiency of epidemic influence as an explanation, and the necessity of finding some other cause for its greater prevalence in a particular direction, is neither extensive nor positive.

The explanation which attributes this peculiarity to contagion has the merit of being simple and apparently adequate, but the difficulties on examination are more numerous and fully as great. Assuming for a moment that the disease can be only communicated during labour, let us recall to our readers what takes place during an ordinary visit to a patient in puerperal fever, during which time the infection is to be taken. The visit may occupy five or ten minutes, the physician stands by the bed, feels the pulse, examines the abdomen, but does not come in contact with the discharges. Having made his investigations, he washes his hands carefully, and then pays more visits, passing through the air, until evening, or until he is called to a labour. If many hours elapse, he must have washed his hands several times. Yet, in spite of all this, we are to suppose that he carries morbid matter on his hands, or clothes, acquired from the fever patient, enough to poison the lying-in woman. And not only this, but the explanation is supposed to be equally valid even though he change his clothes, thus limiting the infection to the hands, and even though he use chloride of lime or potash.

If the morbid matter be conveyed on the hands, the infection, we suppose, and such seems to be the general opinion, must be imparted during labour; but if on the person or clothes, the effect might of course be produced subsequently, and hence another difficulty. During the visit, the consulting physician is as close to the fever patient, examines her, handles her quite as much at the visit as her ordinary attendant, and, it may be assumed, adopts afterwards much the same precautions. Yet we do not hear of his conveying the fever to his own patients in any case, and we have Dr. Meigs' positive statement that such an occurrence never took place in his practice. The advocates of contagion should explain this.

Again, in all contagious diseases, the intensity of the contagion imparted to, and conveyed by, a healthy person (as in scarlatina, for example) must surely be in proportion to the shortness of the time which elapses between his visit to the sick person and to the party to whom he conveys it: in other words, that his chance of so conveying it would diminish with the lapse of time. For example, an accoucheur visits a patient in puerperal fever, suppose, and acquires this contagious property; if this rule be true, the first patient he attends will be more liable to take the disease than the second, and the second than the third. How then, explain the fact on the principle of contagion, that no such sequence of attacks is observed? The cases affected observe no such order, as the reader will see by turning back to Dr. Meigs' registry.

Moreover, in two of the most striking cases we have quoted, Dr. Gooch's and Dr. Rutter's, there is a circumstance which is not reconcileable with, or explicable by, the doctrine of contagion, as we understand it. In the one case a month, and in the other ten days, of absence elapsed, and the latter was accompanied by a complete renewal of clothing, and yet the first case attended by both was attacked by puerperal fever. Are we to attribute this to remaining contagion, and if not, does it not point directly to some other influence which may have operated previously as well?

Thus, a belief in the contagiousness of puerperal fever under ordinary circumstances, and excluding the cases in Sections 1, 2, and 3, must involve, on the one hand, the conclusion that it is of all contagious disorders the most virulently contagious, inasmuch as it assumes that it can be conveyed by a healthy person exposed for a few moments only to its influence, to a third party hitherto in health, and this notwithstanding that the hands, the only part in contact with the sick person, have been carefully washed, the clothes changed, and the entire person exposed to the air, it may be for hours; and yet, on the other hand, that this contagious property limits itself to the ordinary attendant, and does not affect the consulting physician. Admitting that we cannot fully and satisfactorily explain the limitation of the disease on the supposition of epidemic influence only, we ask our readers whether the difficulties attendant upon the explanation by contagion are not more insuperable?

In conclusion, therefore, whilst we feel compelled by the evidence on record to admit the possibility of puerperal fever being conveyed and communicated or excited by those who attend midwifery cases after being employed in dissection or post-mortem examinations, and also by those who are much in contact with the patient or the discharges, especially if strict precautions are not adopted as to cleanliness and change of dress, we do not feel that in other cases, where no such conditions exist, the evidence at all justifies our attributing the spread of the disease to contagion, and we think fewer difficulties and contradictions are incurred by attributing its extension to epidemic influence, and its limitation to conditions or circumstances of which we are at present ignorant.

III. *Treatment.*—We have occupied so much space with the consideration of the two first portions of our subject, that our observations upon the treatment must be very brief indeed. If we have succeeded in showing that certain epidemics have a typhoid character, it follows as a necessary consequence that the treatment which is successful with cases of a sthenic inflammatory type, will be quite unsuitable for them. In the epidemics witnessed by Dr. Meigs, he found early and ample bloodletting the principal, almost the sole remedy:

"I hope, my dear young gentlemen, you will rely, then, upon venesection as the most effectual, and indeed only trustworthy power vouchsafed to you in these trying circumstances; and I pray you, in executing this delicate task, to observe the following methods:—Let the woman be raised in bed, upon pillows that may support the trunk in a highly inclined position, though not in an absolutely upright one, unless she be still possessed of considerable strength. Select the most proper, which is the largest and most turgid vein in the arm, and open it by a free incision, to produce a copious jet. A clear, bright light should invariably fall on the patient's countenance, to enable you to judge, by its hues and psychical expression, of the influence of your operation during the time while it is in progress.

"The design of the operation, in childbed fevers, should never be merely to fulfil some purpose of presumed expediency, to lessen a little some pain, to diminish a somewhat troublesome cephalalgia or a vexing heat,—it is to be done with a view *jugulare febrim*, by impressing upon the motive powers of the circulation such an overmastering influence that the inflamed tissues may afterwards successfully resist the overbearing power of the general vascular reaction, which was first aroused by them alone.

"Leave off with eight, twelve, or sixteen ounces abstracted, and you may go away from the bedside, saying, 'She will surely die;' but if you will courageously persevere until twenty-four or more ounces, not too many more, are taken away, you may retire after your ministration, feeling assured that the duty is well done, and believing that the life of the patient will be saved." (p. 256.)

We do not doubt the evidence of Dr. Gordon or Dr. Meigs as to the results of this method in the cases they saw, but we contend that in other epidemics it would simply hasten the death of the patient: and of late years, and in these countries, this typhoid character has chiefly characterized the disease. Contrast Dr. Meigs' directions and views with the opinions of Dr. Collins, as they are stated in a letter to Dr. Meigs, and published in his treatise 'On Obstetrics:'

"At pages 609-10, you compare the mortality in puerperal fever under my treatment, and that of my distinguished friend, Dr. Robert Lee, of London; to prove

> the greater success when general bleeding has been more frequently adopted by him. The great and markedly distinguished feature between Dr. Lee's cases and mine has, however, been overlooked; as mine were all *hospital patients*, whereas his were treated *at their own dwellings*. This was also the case with the late Dr. Gordon's patients, to whom you so deservedly allude. The disease with us, and I believe universally, is as different in hospital and out of hospital as it is possible to imagine. Please look to my observations, pages 390-1-2, &c., where I have stated the patient to be little more than a shadow, and to exhibit the appearance of those labouring under cholera, so as to make us dread even the application of leeches. The fever is of the lowest typhoid character, with the pulse so feeble and indistinct as to totally prohibit general depletion. This form of the disease is singularly intractable, and truly fatal; whereas the inflammatory form of puerperal fever, such as is usually met with out of hospital, may be treated with considerable success.
>
> "I should have stated, that few physicians have witnessed the results of general bleeding to a greater extent than I have done, as the master of the hospital who preceded me, and to whom I was assistant, was a strong advocate for it; but the mortality was so frightful he was forced to abandon it. He bled instantly and copiously, but with the most fatal results. Such is the character of almost all our hospital epidemics."

The experience of the present master, Dr. M'Clintock, we have already seen, is precisely similar in the recent epidemic; and we have only to add, that of late years, so far as our own limited experience enables us to judge, the same typhoid type and intolerance of general bleeding characterizes, more or less, the cases met with in private practice; following in this the change of type, and consequent change of treatment, necessary in fevers and febrile diseases generally.

Fleetwood Churchill.

REVIEW IV.

1. *Traité de la Syphilis des Nouveaux-Nés et des Enfants à la Mamelle.* Par P. DIDAY, ex-Chirurgien-en-chef de l'Antiquaille (Hôpital des Vénériens de Lyon).—*Paris*, 1854. pp. 439.
Treatise on the Syphilis of Newborn Children and Infants at the Breast. By P. DIDAY.

2. *Histoire et Thérapeutique de la Syphilis des Nouveaux-Nés et des Enfants à la Mamelle.* Par — PUTEGNAT (de Luneville), Docteur en Médecine et en Chirurgie de la Faculté de Paris, &c.—*Paris*, 1854. pp. 216.
History and Therapeutics of the Syphilis of Newborn Children and Infants at the Breast. By — PUTEGNAT, M.D.

3. *Ueber die Syphilis der Neugeborenen.* ('Journal für Kinderkrankheiten:' BEHREND, Band xvii. s. 17; GUBLER, Band xix. s. 171; LUZSINSKY, Band xx. s. 274.)
On the Syphilis of Newborn Children. By Messrs. BEHREND, GUBLER, and LUZSINSKY.

4. *De la Méningite Tuberculeuse, étudiée au point de Vue Clinique.* Par HENRI HAHN, Médecin de l'Hospice Joséphine à Aix-la-Chapelle, &c.—*Paris*, 1853. pp. 246.
On Tubercular Meningitis, examined in a Clinical Point of View. By H. HAHN, M.D.

5. *Traité Clinique et Pratique des Maladies des Enfants.* Par MM. Rilliet et Barthez. Deuxième édition. Tome troisième. *Scrofule et Tubercules*, &c.—*Paris*, 1854. pp. 928.

Clinical and Practical Treatise on the Diseases of Children. By Messrs. Rilliet and Barthez.

6. *Klinische Beobachtungen über die Gehirnkrankheiten der Kinder,—besonders über Gehirntuberculose.* Von Dr. Hauner. ('Journal für Kinderkrankheiten,' Band xviii. s. 321.)

Clinical Observations on the Cerebral Diseases of Children, and especially of Cerebral Tuberculosis. By Dr. Hauner.

7. *Erlebnisse aus der Kinderpraxis.* Von Dr. Joseph Bierbaum. Erste Abtheilung. *Krankheiten des Gehirnes und der Sinnesorgane: 1. Hydrocephalus Acutus.* ('Journal für Kinderkrankheiten,' Band xxiii. s. 167.)

Results of Medical Practice among Children. By Dr. J. Bierbaum, Part First. *Diseases of the Brain and the Organs of Special Sense.*

8. *Lehrbuch der Krankheiten des Kindes in seinen Verschiedenen Altersstufen.* Von Dr. Carl Hennig. Erste Lieferung.—*Leipzig*, 1854. pp. 160.

Manual of the Diseases of the Child at Different Ages. By Dr. C. Hennig.

9. *Practical Treatise on the Diseases of Children and Infants at the Breast; including the Hygiène and Physical Education of Young Children.* Translated from the French of M. Bouchut, with Notes and Additions by Frederick Hinckes Bird, F.R.C.S., &c.—*London*, 1855. pp. 776.

Our first and earliest intercourse with the *arcana medica* of children, made through the mothers and nurses of the little patients of a public institution, went far in an endeavour to indoctrinate us into the popular belief that the whole and true nosology of the diseases of early life might be thus represented—*worms*—*teething.* When we refused this axiom, "water on the head" was not unwillingly added to the former; and when we again rebelled, the addition of *consumption* was thought equal to the surmounting of all emergencies. Had *we* been of this opinion, remained orthodox, and introduced no heresy and schism, we believe that with the above "quarternion" and the ordinary whooping-cough, chicken-pox, measles, &c. (the diagnosis of which is at once taken out of our hands), we might have gained as fair a reputation as did numerous others within the range of our out-patients' district for being "so famous for children." But neither our knowledge nor experience could permit it. We gave up lancing the gums and prescribing anthelmintics, and awoke to a full perception of the force of the true yet quaint saying of Goelis, of Vienna.* The next step saw us confessing to the truth that the diagnosis of pulmonary tuberculization in the child was occasionally one of the most difficult problems which came before us for solution. At this period, when we were so indignantly rejecting "worms" and "teething," "water on the head," and "consumption," as the chief sources of the sufferings of infancy and childhood, we were little aware how potent an ally the *four* might have brought into the field to attempt once again our definite subjugation. The support was not sought; motives of delicacy perhaps prevented the junction. That ally was *syphilis*. Time has passed on since we first commenced the warfare, and as we have become acquainted with the reputation of the weighty power of mischief alluded to, we feel glad to have escaped its influence on our minds until we had accumulated some experience of our own wherewith to balance it. Once (and but lately too) we confess to have been sorely puzzled as to whether or not, after all, our nosology was to be cut down to offshoots from the universal poison, syphilis. We read* with trepidation that the poison of syphilis "being once received into the blood, it remains there for years, and possibly—indeed *certainly for the rest of existence*"—that "the infected child must remain infected until death"—that "a considerable proportion of those diseases which pass under the name of scrofula are the produce of the syphilitic poison; are, in fact, *not scrofulous but syphilitic*;" and that lupus, kelis, lepra, and psoriasis, have their source in syphilis, which, in fact, is "a widely-spread, almost universal animal poison." We have recovered the shock, however, and although we dissent to the propriety of seeking the genesis of more than half "the ills that flesh is heir to," in the ineradicability of the venereal poison, we are not the less willing to admit the unfortunately too evident fact that it does "visit the sins of the fathers upon the children." Wide-spread and severe as such visitation is, however, we see the result of imagination rather than of truthful experience in such doctrines as the following would imply:

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* Es ist ein verdammenswerther Schlendrian grauer Unwissenheit, gewiss zwei Drittheilen aller Gebornen schon bei der Wiege das Schwanenlied zu singen: das Kind hat Wasser im Kopfe.

> "Can," asks M. Diday, "hereditary syphilis manifest its action by any morbid effects other than the symptoms characteristic of the pox?
>
> "This question has already been discussed when alluding to pemphigus; it might equally come under consideration when treating of almost all the diseases of infancy, for there are few in the etiology of which syphilis has not been accused of taking a part more or less direct. Doublet attributed induration of the cellular tissue to it; Astruc, rachitism and mesenteric disorder; Bertier, certain hepatic affections; Pitschaft, obstinate wakefulness; Campbell, convulsions; Haase, hemi- and hydro-cephalus; Lamauve, acidity of the *primæ viæ*, apoplexy, and worms; and Levret, the dropsies of early age. In fine, I shall worthily conclude this list in recalling to mind that Sanchez perceived the results of the syphilitic virus in imperforation of the anus, hypospadias, and even in the green colour of the stools of the new-born child!!!" (p. 193.)

High in favour as syphilis stood in the opinions of the early writers as causative of very many of the diseases of children, for some years past it had lost ground in this respect, its aid being sought only to explain certain cutaneous manifestations, and some of the more obstinate evidences of scrofula occurring during the first infancy. But lately a revulsion has ensued, and we now find the dyscrasic poison under question not only appealed to as the source of a very numerous and diversified series of integumentary diseases, but of certain pathological changes in the tissues of the lungs, thymus gland, liver, and peritoneum, and as a poison which, when once received into the current of the circulation, necessarily continues in the system until the end of life.

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* On Syphilis, Constitutional and Hereditary, and on Syphilitic Eruptions. By Erasmus Wilson, F.R.S.

Since the time when Gaspard Torella (in 1498) affirmed that he had often seen the unweaned child infected by the sore breasts of its mother, and then bestow upon its nurses, who fondled and kissed it, the same unfortunate endowment, up to the late animated discussion at the Academy of Medicine of France, so wide a field of investigation has been gone over relative to congenital syphilis, as would demand a space for its analysis far beyond that we have here at command. We shall therefore confine ourselves within a narrow limit, laying the opinions of MM. Diday and Putegnat upon some prominent questions before our readers, and dwell a little in detail upon the late researches of MM. Dubois, Depaul, Cazeaux, and others, relative to the influence of the syphilitic poison in producing certain lesions of the abdominal and thoracic viscera.

That the whole subject is fraught with great interest is apparent, and as the forensic and "state police" relations of the question are becoming daily more urgent, the Medical Society of Bordeaux lately offered a prize for an essay upon the general matter. The treatise of M. Diday, now before us, was successful in obtaining it; whether that of M. Putegnat was one of the less fortunate we are not sure, but from some internal evidence we are inclined to think it may have been offered in competition. Both authors are known as men of ability, but in the present instance M. Diday has certainly outstripped M. Putegnat. The latter having taken for his motto the Horatian precept, *Quidquid præcipies esto brevis*, has so closely followed its spirit as (in our opinion) to have prevented himself from doing justice to the wide and, in relation to many points, much litigated questions before him. He is well acquainted with the literature of his subject, but is very far from having sufficiently unravelled its intricate web. On the other hand, M. Diday's statement, though by no means exhaustive, is yet as full, perhaps, as the form under which his treatise comes before the public would entitle us to expect. His book is a very fair *prize essay*. He has examined the evidence recorded in the literature of his own country, added to it the result of his own experience, and lays down his general laws accordingly. We need scarcely say, too, that the opinions and experience of the ex-chief-surgeon of the venereal hospital of Lyons should have some value in respect to our present subject.

M. Diday divides his work into five parts, under which are arranged the *etiology*, *symptomatology*, *prognosis*, *medico-legal relations*, and *therapeutics* of "infantile syphilis." He directs attention at the outset to the necessity of a definite nomenclature, and of drawing a rigid line of distinction between that *congenital* syphilis the child contracts during its intra-uterine existence from the formative or nutritive elements received from the parents, and that *acquired* taint it may receive at birth or afterwards, from absorption of the virus from any extraneous source. The two orders of phenomena thence arising are (strange as it may appear) constantly mixed up together, under the common term of "infantile syphilis." The laws of "hereditary transmission" having lately been discussed in these pages (vols. ix. and x.), when the works of Mr. Whitehead and Mr. Wilson passed under review, we shall at present merely acquaint the reader with the doctrines expressed by MM. Diday and Putegnat.

Both agree to the proposition (M. Diday, p. 24, M. Putegnat, p. 106) that a father may bestow the syphilitic poison on the child without first of all infecting the mother, and, according to the former, (p. 26) though the immunity of the fœtus is *possible*, when engendered by a father who having had syphilis is then, after a mercurial course, free from all open manifestations or symptoms, it would be an error to maintain its immunity to be certain. It is also M. Diday's opinion (p. 33) that if an attainted man has cohabited with a pregnant female, more particularly during the earlier periods of her gestation, we cannot affirm the immunity of the child, though the mother herself has not presented any signs of having been infected. The latter doctrine M. Putegnat (p. 107) is opposed to, believing the mother must be infected before the poison germ can be transmitted to the child. Relative to the influence of the *mother*, the first axiom of both writers is, that when infected before conception she exerts "a clear, patent, undeniable" (Diday), "almost certain" (Putegnat) deleterious action on the child. In stating their next and indisputable law—viz., that the mother becoming infected after conception attaints the child, they ask whether there is a period beyond which the risk of this attaint becomes lessened or negatived? M. Diday regards the cases he appeals to as supporting the doctrine that "syphilis contracted by the mother before the completion of the *fourth week*, or after the *seventh month*, has never been the cause of syphilis in the fœtus" (p. 48). M. Putegnat thus writes:

> "The fœtus will be almost certainly attainted during the first three months of its existence.
>
> "If the constitutional syphilis ensues only during the latter months, the fœtus may escape, thanks to its own strong constitution, obtained from that of its father." (p. 112.)

Both writers, in admitting the law that father and mother alike being infected at the time of a fecund connexion, the chances are manifold that the offspring will be tainted too, yet deny the truth of the usually-received opinion that it must *necessarily* be so, or that the syphilization of the child is *obligatory*. Two later writers, MM. Maisonneuve and Montanier have even asserted that "the child has no chance of escaping the infection—it is fatal to it."

Passing to *acquired syphilis*;—it is believed by our authors that infection of the child by the mother during labour, or as the infant is passing *per vias naturales*, is, if not an impossible occurrence, yet a very rare and exceptional fact. Relative to the power of the lacteal secretion of an infected nurse in tainting the suckling, M. Diday enters at some length into the question, but arrives, we regret to say, at no more definite conclusion than the following:

> "As regards myself and this litigated point, I neither absolutely admit nor reject anything. If, on the one hand, theory leads me to admit the reality of the influence, on the other, experience has not yet lent sufficient support to her suggestions. I await, therefore, the future, only appealing, in the first place, to the impartiality, and in the second, to the zeal, of after-investigators." (p. 78.)

M. Putegnat is more decided (p. 102), agreeing with Bertin, Bell, and

Ratier, &c., that there is no doubt about the power of the milk in conveying the poison germ of the syphilitic virus. The child may of course be said to be open to all the accidental causes of infection to which the adult is, still the state of infancy is found to be more liable to certain modes of infection than to others. The mechanism of the propagation is, under all circumstances, *contact;* but the conditions of the contact may be peculiar. M. Diday gives the following as common modes of infecting a child:—*a.* A nurse having a primary genital chancre touches her excoriated nipple, the latter becomes inoculated; the syphilized nipple is afterwards sucked by the child. *b.* Infected strangers incessantly, and with passion, fondling and embracing children, sleeping with them and pressing them next their bodies to keep them warm. *c.* Washing children with saliva, "the possible vehicle of a contagious principle," with a sponge which has come in contact with a chancrous sore, or with water, previously used by an infected person.

A question of some import in its forensic as well as pathologic relations lately discussed and answered definitely in the negative by MM. Maisonneuve and Montanier, is the following:—Can a fœtus, deriving the syphilitic poison from the father, afterwards, or in a reflex manner as it were, infect its mother during its intra-uterine existence? For or against says M. Diday—

> "Neither party being able to invoke facts absolutely conclusive, one is obliged to substitute quantity for quality, trusting that a happy chance may bring together before the observer the circumstances—so rarely united—from whose combination may spring, not a proof (that is here almost impossible), but a *probability.* . . . . But would intemperateness at length invade this corner of science hitherto closed against its suggestions? I almost fear so, from the perusal of the most recently published work on venereal diseases, the treatise of MM. Maisonneuve and Montanier. I shall not follow their example; I do not intend opposing to their negative and so absolute conclusions, others equivalently affirmative." (p. 238).

M. Putegnat (p. 114) appears to be of one mind with M. Diday.

That the primary sores and chancres of a child may communicate their virus to the nurse, just in the same way as the poison passes from the organs of the female to those of the male during the frictions of coition, is a doctrine admitted by all syphilographers. But have the lesions of secondary syphilis the same communicability? or, to use the words of M. Diday—

> "A child hereditarily affected with 'mucous tubercles' of the mouth, for example—can it bestow the same disease on a healthy woman who suckles it?" (p. 249.)
>
> "Those who do not believe in the transmissibility of secondary accidents are obliged to explain *all* the facts bearing on our present question through the previous existence (forgotten or dissimulated) of a primary chancre in the infected individual. Their opponents are not forced to appear so exclusive . . . . . they admit the value of the doctrine to a certain extent, but deny that the latter can afford an explanation of *all* the instances in which syphilis is said to have been transmitted from the suckling to the nurse. And powerful reasons, it must be admitted, plead in favour of this eclectic opinion, which I myself adopt and feel necessitated to inculcate." (p. 257.)

According to M. Putegnat, the transmissibility in question is *possible,* but that is all (p. 119).

We pass over the manifestations of syphilis in the child by the cutaneous and mucous surfaces, detaining the reader for a short time only in connexion with "pemphigus."

At birth, or a few days afterwards, some children are observed to have *bullæ*, chiefly on the palms of the hands and on the soles of the feet. In a ratio with the severity and extent of the eruption, the child gets weak and emaciates, cries continually, and refuses the breast. It appears to suffer much, and the general health is often so profoundly and quickly affected, that death results in a very few days. This disease, "infantile pemphigus," was formerly regarded as a syphilitic affection. But in 1794, Osiander protested against this view of the matter; the question excited no interest, however, until 1847, when M. Stoltz drew attention to the circumstance, that not only was "pemphigus" syphilitic in its nature, but he affirmed that it was the most common manifestation of the congenital vice that could be seen. The question was once more argued, and again forgotten until 1851, when it became the subject of a grand dispute between MM. Dubois, Ricord, Cazeaux, &c., in the French Academy. M. Dubois may be regarded as the representative of the affirmative, and M. Cazeaux of the negative phase of the argument. The main points rested on by the former are the traces of syphilis in the parents, and the frequent simultaneous existence in the child of other and characteristic manifestations of the venereal virus. M. Cazeaux, on the other hand, maintains that the description given of the so-called syphilitic pemphigus does not differ from that of the simple pemphigus of adults; that it appears at or soon after birth, when the known signs of hereditary syphilis do not become evident, but evince themselves at a later period; and that at the Ourcine, where a great many women are delivered syphilised or fecundated by attainted men, no infant has yet, within his experience, been attacked by pemphigus. M. Diday remarks also:

> "It must nevertheless be admitted, that the number of cases of infantile pemphigus, in which the existence of venereal disease has been substantiated in the parents, is sufficiently great to make an impression on unprejudiced minds. Although the majority of the Academicians appeared fixed in doubt as to the nature of this eruption, did the mediate opinion of *conciliation*, we would ask—which regards it not as an immediate result of syphilis, but as an indirect one of the debility induced by the diathetic affection—obtain many suffrages? We ourselves adopt this solution; we shall endeavour to explain it, and give it precision." (p. 120.)

M. Putegnat appears to think the affection in question is frequently a direct result of the syphilitic poison, but admits the difficulty of distinguishing the specific from the simple form. Ample details will be found in the work of M. Diday, and in M. Bouchut's treatise the subject is also discussed, and in particular the differential diagnosis between the two forms of the disorder.

Those who have been curious in the history of syphilis, must have remarked that many of the older writers insisted on the liability of the liver and of the lungs to become involved in the syphilitic disease. From the time of Nic. Massa, in 1563, to that of the publication of the Essays of MM. Depaul and Dubois, in 1850-51, this fact, though frequently insisted on, yet made in the schools little impression. It is to these latter writers, and to M. Lagneau, that the credit is due for fixing the attention of modern pathologists on this branch of the subject. Their inquiries, as well as those of M. Gubler, relate to the changes which the lungs, thymus gland, and liver undergo in consequence of the infection of the fœtus with the syphilitic poison. For the historical details of these questions we may refer the reader to the able papers by M. Gubler, in the 'Gazette des Hôpitaux,' 1848, and 'Gazette Médicale,' 1852, and which are given also in the 'Journal für Kinderkrankheiten,' as referred to at the head of this article, and from whence, if necessary, we shall quote. On reference to the work of Dr. Mauch,* previously reviewed in our fourteenth volume, it will be seen, that as far back as the middle of the seventeenth century, a Copenhagen physician, Simon Pauli, drew attention to the influence of the syphilitic virus on the *thymus gland;* and Lieutaud, 100 years later, renewed the observation. The matter was disregarded until 1850, when M. Dubois being struck with the death, a few days after birth, of children whose parents had had syphilis, and being unable to explain such result from the severity of the diseased manifestations of the cutaneous and mucous surfaces, determined to submit the viscera to a more attentive scrutiny than was usually bestowed upon them. Having done so, he recognised the existence of a particular disease of the thymus gland. Its characters may be gleaned from the following description of M. Diday:

> "The thymus presents nothing extraordinary as regards its colour and volume; but on pressing after having incised it, little drops of a semi-fluid matter, of a yellowish-white colour, having all the appearances of pus, easily escape. M. Donné (to whom M. Dubois sent a little of the liquid for analysis) found, indeed, in it all the characters of true pus.
>
> "In all the cases cited by M. P. Dubois, the purulent matter seemed disseminated in the tissue of the organ, and was not circumscribed or collected. M. Depaul has also met with this form in one instance; but in another, has remarked the thymus to have shown in each of its lobes a small cavity, filled with a grumous, yellowish, rather thick matter. In the latter case, the gland was likewise a little larger than usual; of five children affected with the disease, one was born dead, two lived but a few minutes, one survived six days, and one eight." (p. 147.)

In 1851, a memoir was presented to the French Academy by M. Depaul, giving rise to the well-known discussion we have before alluded to.† The author reminded the Academy, that as far back as 1837, he had, in the 'Proceedings of the Anatomical Society of Paris,' endeavoured to show, that the numerous small and scattered abscesses found in the lungs of new-born children, were but the signs of congenital syphilis. Since then he had met, not only with ample proof of the syphilitic nature of pemphigus, but with not less than *fifteen* cases of syphilitic abscess of the lung.

> "I might say," said M. Depaul, "with almost certainty, that all, or at least most of, the cases of tubercles of new-born children, alluded to by Billard, Baron, and Husson, have been nothing but instances of congenital syphilis. What was taken for tubercle, were either single nodules of purulent infiltration, or true abscesses with more or less thickened walls."

M. Dubois, and those who side with M. Depaul, recall to mind how it has long been admitted, that a great number of children dying with syphilitic pustules, mucous tubercles, &c., have succumbed to pneumonia, or have pulmonary lesions anterior to birth. They affirm that this pneumonia is not the simple lobular pneumonia of Baron, Billard, Husson, Cruveilhier, and Sestier, nor the "pulmonary collapse" of other writers, but a special affection, associated with other syphilitic phenomena, in children congenitally tainted. That the simultaneous presence of pulmonary abscess, thymic abscess, and frequently of pemphigus, in the same subject, is sufficient to prove the existence, at the least, of a cachectic condition of the whole frame. In 1852, M. Gubler carried this department of the pathology of congenital syphilis yet a step further, and placed before the profession an elaborate memoir on hypertrophy, or "plastic induration" of the liver, "not found as yet connected with any other diathetic malady than with syphilis." M. Gubler, and those who have followed him, describe the particular hepatic lesion as both of a general and partial character, and as evincing an alliance both to the secondary and tertiary periods, as the case may be. As we before remarked, these views (which we have but space thus cursorily to develop) of MM. Dubois, Depaul, and Gubler, have given rise to much discussion, though it has been chiefly, but not entirely, in connexion with the asserted syphilitic lesions of the lungs and thymus gland. M. Diday, alluding to M. Gubler, remarks—

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* Die Asthmatischen Krankheiten der Kinder. Erster Theil. Vom Verhältnisse der Thymus beim Asthma. Berlin, 1853.

† See also the previous number of this Journal, p. 33.

> "Happier than M. Depaul, he has found no difficulty in gaining admission for this new disease among the more incontestable effects of the syphilitic virus. The cases of it are so numerous, so satisfactory, that there has not occurred the slightest hesitation as to the position which should be accorded to the affection in the syphilographic catalogue." (p. 149.) . . . .
>
> "The venereal character of the hepatic affection is, *primâ facie*, rendered very probable from the constant coincidence of some of the preceding symptoms in all the children in whom it has been observed; but what completes the demonstration is, that M. Gubler has not met with it in union with any other diathetic disorder than with syphilis. Like him, MM. Trousseau, Horteloup, Cullerier, Depaul, Lenoir, and Lebert, have also met with analogous cases." (p. 152.)

On one point, M. Diday differs from M. Gubler:

> "M. Gubler has attentively studied that particular alteration of the liver which co-exists with syphilis, but he has not assigned it a distinct place in the hierarchy of the successive accidents of this affection. In his eyes it is simply a lesion of the tertiary order, though at the same time it is one of the first symptoms which appears in new-born children, though, according to our author's own admission, no tertiary phenomenon (properly so called) is found simultaneously with it, and though it is the proto-ioduret of mercury, and not the iodide of potassium, which appears to be its specific! Are these considerations sufficient to admit of the very different interpretation which I have proposed of the same lesion? In my opinion, the induration of the liver is absolutely analogous in the fœtus to the induration of the chancre in the adult. It is the result of the transport of the virus which proceeds from the blood of the mother, and gives rise on its passage in the liver to that same organic reaction expressed by induration, that the virulent pus absorbed during coition determines around the chancre, and then in the first lymphatic gland it traverses. In this view of the matter, there could be no induration of the liver when the infection proceeds from the father. It is an hypothesis which will, of course, derive its future value from facts of this latter character. However, I shall at present abstain from giving a decided opinion as to its value. I shall only remark, that it meets with considerable support from the very great similarity that is established by appearance, feel, and microscopic examination between this condition of the liver and the induration of the primitive chancre." (p. 156.)

M. Gubler's paper terminates with the following remarks:

> "From what has been stated, it will be seen that it is quite impossible to lay down the principles of a good and sure diagnosis. This much may be said, however, that when in a young syphilitically-tainted child we find considerable disturbance of the digestive functions, with marked general anæmia, enlarged volume, and altered consistency of the liver, we have reasons for thinking that 'plastic induration' of this organ is present. If with these symptoms those of *peritonitis* are associated, all doubt is removed."*

As a circumstance peculiarly worthy of record here, we may remark that, as far back as 1838, Dr. Simpson, of Edinburgh, drew attention in an able paper† to the occurrence of *peritonitis* in the fœtus *in utero.* Twenty-three cases (more or less detailed) of peritonitis, fatal either before or soon after birth, were recorded. In two of them (5, 9) the mothers had an attack of gonorrhœa during the period of utero-gestation, along with a syphilitic eruption in one instance (9), and ulcers in the other (5). A third (6) confessed that she had suffered from venereal disease; and the line of life pursued by others of the number (3, 4, 7) was such as appeared to expose them to syphilitic infection:

> "Indeed," remarks Professor Simpson, "it appears to me highly probable, from the investigations which I have already made upon this point, that a great proportion of those children of syphilitic mothers that die in the latter months of pregnancy may be shown to have perished under the attacks of peritoneal inflammation."

The paper of Dr. Simpson's here alluded to is known to the Continental writers, but MM. Gubler and Cullerier affirm this syphilitic peritonitis to be, not a single primitive affection, but merely an accompaniment or sign of their "plastic induration" of the liver. According to the former—

> "Nothing is rarer in early infancy than to meet with peritonitis divested of all association. It is a malady scarcely seen unattended by umbilical phlebitis or with syphilis, and, according to my experience in the latter case, it is almost always connected with the hepatic alteration."

Now it is but fair to Dr. Simpson to point out that complications of this *peritonitis* are fully alluded to in his paper, but that, as regards the nature of the hepatic lesions, there is, no doubt, a difference between the views of the Scotch and the Continental writers. The former remarks:

> "In a second case, the peritoneal inflammation was accompanied by hepatitis in the stage of softening, and incipient purulent infiltration; in another, the morbid changes in the same organ were of a more chronic character, the coats of the liver being opaque and somewhat thickened, and the organ itself reduced in size. In one instance there were found some of those small masses of inflammatory induration in the lungs which form the most common type of pneumonia in the fœtus and infant." (Op. cit.)

In the discussion at the Academy on the import of the changes in the lungs and thymus, M. Cazeaux, representing the dissentients to the new doctrine, first inquired whether the collections of pus in the lungs of newborn children might not be otherwise explained than through the aid of syphilis, and then endeavoured to negative the particular support relied upon by M. Depaul in favour of his own theory:

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* Journal für Kinderkrankheiten, Band xix. p. 194.

† Edinburgh Medical and Surgical Journal, vol. xv. No. 137, p. 390: Contributions to Intra-Uterine Pathology, Part I.

"It is quite unnecessary," said M. Cazeaux, "for the support of my opposition, that I should prove to which group of diseases the pulmonary changes under discussion must belong, though not to syphilis I say, it is not necessary I should prove this, but it is necessary that I should enforce this observation, that, without any overstrained and hypothetic explanation, perhaps a simple ordinary inflammation may be accepted as the true origin of these formations of matter."

Dubois, Ricord, Gibert, Danyau, Lagneau, and others, entered into the debate, but we must content ourselves with the following reply of M. Bouchut, as rendered by Mr. Bird, p. 730:

"M. Cazeaux appears to me to have far too much sacrificed the probabilities in favour of this exigency to the bare anatomical facts. He demands that a lesion should exhibit the nature of its first cause, whilst he himself knows very well that lesions, the result of constitutional syphilis, are not absolutely and always recognisable at first sight. If we reasoned in this way, we should return to the dark ages of Broussais, when every disease, even syphilis, was classed under the title of inflammation. We must not, then, tax anatomical facts too far, for fear of injuring them in the estimation of surgeons; and when they have no absolute signification, it is to the antecedent and concomitant circumstances to which we must look to determine their true nature. This M. Depaul has done; he has often noticed suppurating pulmonary nodules accompanying hereditary syphilis—he concluded that one might be the cause of the other, and he was right." (Op. cit.)

We must here bring to a close this department of our subject, strongly recommending the perusal of M. Diday's treatise to all inquirers into the interesting, important, though somewhat involved question of syphilis in the child.

We have so frequently in the pages of this journal* maintained the doctrine (first promulgated distinctly by MM. Rilliet and Barthez) of the rigid demarcation to be kept between that form of meningitis developed under the influence of the diathetic disorder, scrofula—granular meningitis—and that of a simple non-specific character, that it would be mere supererogation to enter now into the argument. In the concluding volume of MM. Rilliet and Barthez' great work, lately received by us, this doctrine is continued to be taught, and receives farther elucidation from the increased experience of the authors. To this volume we can refer with pleasure, as not only giving an admirable account of the meningeal disease just mentioned, but such an exposition of the general subject of tuberculosis in the child as is nowhere else to be met with. It appearing to M. Hahn that the attention of modern practitioners has been too exclusively directed to the morbid anatomy of infantile meningitis, that the therapeia of the disease has been regarded as if of secondary consequence, undertook "to place the treatment of tuberculous meningitis on a rational foundation, based on modern pathologic inquiry, and on the results of a lengthened experience." When about commencing his task, the Society of Medicine of Bordeaux proposed "a clinical inquiry" into the subject as a prize question. M. Hahn responded to the appeal, and the Essay now lying upon our table was the successful one in competition. The main endeavour of the author is to show that "It is not difficult to demonstrate that tuberculous meningitis is capable of a more or less perfect cure in a very great number of cases and at various periods of the malady." (p. 58.) Now this is a very bold and, we think, erroneous assertion, and we should be disposed to reverse the statement, and say it is incapable of cure in by far the majority of instances and at all periods of the course of the disorder. This opinion coincides with that expressed by MM. Rilliet and Barthez in their last volume:

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* See, in particular, vol. xiv. p. 482 et seq.

> "Without going so far as Camper, who speaks of hydrocephalus as the *immedicabile vitium*, we think that the scepticism of Frank has more to support it than has the exaggerated confidence of Gölis, Heim, and Formey; and that the practitioner called upon to undertake the treatment of hydrocephalic patients will have far more defeats to lament than victories to celebrate." (p. 508.)

We can yet well understand how loath many will be to accept so forlorn a prognosis as we feel called upon to give in reference to this malady, when men of high reputation and apparently ample means for observation, have maintained the contrary. On both sides may be ranged names of high repute. M. Hahn remarks:

> "The prejudice which attributes a character of incurability to tuberculous meningitis serves only to shackle the progress of medical art. But we have sufficiently elucidated this question in our fourth chapter, and have there demonstrated that the disease is, in a very great many cases, susceptible of cure." (p. 110.)

Now, where is the cause of such discrepancy of opinion to be sought? Is it in the greater therapeutic ability of the practitioner, and a more successful method of treatment, or in a difference of diagnosis? We believe in the latter. Cerebral congestion and erethism, simple meningitis, &c., have been mistaken for the more formidable disease, and recoveries from them recorded as if from the latter. MM. Rilliet and Barthez thus express themselves:

> "The generally admitted danger of the malady obliges us to regard as apocryphal the great number of cases of cure published by authors. In truth, a rigid analysis proves that a considerable amount of them relates to diseases very different to meningitis, and having only a very coarse symptomatic analogy with it." (p. 510.)

Dr. Bierbaum reduces "acute hydrocephalus" to *three* forms—viz., simple meningitis, granular meningitis, and "a cerebral affection offering the same symptoms during life, but after death neither the lesions of simple nor of tuberculous meningitis. This affection appears to have, as its basis, a dynamic disturbance of the cerebral activity, induced by irritation." (p. 169.) Thirty cases are detailed, and the *prognosis* afterwards thus spoken of:

> "We have already seen that this cerebral disease had a fatal issue seventeen times in our thirty cases. There died, consequently, more than one-half of the affected children. In other instances I was more successful, and saved a half. But even from this rate of mortality it must be concluded that this cerebral affection yet belongs to the most lethal of infantile maladies." (p. 225.)

The author admits that the granular variety of his "acute hydrocephalus" is by far the more fatal one, yet he afterward (p. 248) asserts that the differentiation of the general disease into a granular or tuberculous and a simple meningitis, has been of no especial advantage to the treatment, as the same indications are present in both varieties. Now, we believe there is a general opinion prevalent among modern pathologists, cognizant of the difference between the two, that the simple form is by far the more amenable to treatment, and notwithstanding the not very cheering opinion given by MM. Rilliet and Barthez, we continue to think so.

> "If," say these writers, "the absence of the tubercular element, and the outbreak of the disease in the midst of perfect health, offer legitimate hopes of seeing the meningitis terminate favourably, it must not be disguised, on the other hand, that the extent of the inflammation, and the rapidity of its progress, leave but few resources to the practitioner, and even less to time." (vol. i. p. 126.)

We will now revert to M. Hahn, who comes to offer us a more favourable opinion as to the curability of the granular variety. The author allows no *invariable* form of therapeia to be possible, but, making three divisions of the disease according to its form and progress, lays down a treatment most conformable to each. It may be thus indicated:

A. Antiphlogistic regimen and diet, depletion by bleeding or leeches, cold applications to the shaved head, sinapisms and pediluvia, calomel, blisters to distant parts of the body, blister to scalp or nucha, and use of revulsives.

B. Occasional leeches, cold lotions to head, calomel, iodide of potassium, digitalis, revulsives, &c., as before.

C. No treatment of avail.

One of the most particular points in M. Hahn's treatment is certainly the prominent use of powerful revulsives, and his advice to cause a suppurating sore on the vertex or at the *nucha*, is peculiarly his own, so far as the stress upon it is concerned, although M. Bessières some time ago drew attention to the importance of the sore from a blister, if suppurative.

> "We are convinced," writes M. Hahn, "that we possess in the antimonial ointment a therapeutic measure still more efficacious; and that, in the latter stages of the malady, its employment offers out far more chances of success than the use of blisters." (p. 128.)

Severe pustulation of the scalp is induced, sometimes, as we have ourselves seen, even gangrene of it, and M. Hahn states that "it may happen that the bones of the skull shall exfoliate in one or two places. But this is of extremely rare occurrence, and we are aware of only one example." (p. 131). Without acceding to M. Hahn's general proposition, we willingly do so to the particular one concerning the value of suppurative derivation. We have used it freely—twice even to the extent of causing a portion of the scalp to slough—and have no hesitation in saying that we consider the therapeutic measure of counter-irritation to the nucha and vertex as having the most—little as it may be—hold over the progress of granular meningitis. In answer to the asserted cruelty of the treatment, M. Hahn makes answer "Aux grands maux les grand remèdes." (p. 133.)

MM. Rilliet and Barthez thus refer to the practitioner of Aix-la-Chapelle:

> "Dr. Hahn has published some interesting observations relative to the cure of very severe cerebral diseases, from the result of frictions with antimonial ointment on the previously shaved head. Relative to the greater portion of the facts reported by the author, the diagnosis is not sheltered from all contestation." (vol. iii. p. 524.)

As supporting his views on *curability*, M. Hahn refers to the recent observations of Romberg, in the 'Deutsche Klinik,' and concludes his essay by some remarks on "white softening," a point more fully considered in some papers in the 'Lancet,'* by the writer of the present article.

Dr. Hennig's work may be found by the German student a useful manual. It is quite a compilation, written in a dry, compressed style, but seeks to take a complete view of the present state of infantile pathology.

The English reader is much indebted to Mr. Bird for bringing M. Bouchut before their notice in so complete and satisfactory a form. We have before passed M. Bouchut in review, and as the chief additions to the present work consist in quotations, in the form of notes, from the various articles on pædiatrics which have appeared since 1849 in this Journal, and from the 'Lectures' of Dr. West, we have nothing left us to perform but to recommend very strongly Mr. Bird's edition of M. Bouchut, to the student of medicine in particular.

W. Hughes Willshire.

REVIEW V.

Report of a Committee assembled by General Orders, Commander-in-Chief, dated Sept. 16*th*, 1845, *under Instructions from the Right Hon. the Governor-General of India in Council.—Agra,* 1847.

AT first sight it may appear rather *moutarde après diner*, to draw attention to a Report made, no less than eight years ago, for the purpose of determining the propriety of constructing a canal in the north-western parts of our Indian empire. The Report, however, contains facts of importance bearing upon the whole question of endemic influences, which we think fully justify our introducing a summary of its contents to our readers, though at so late a period. The heading in no way indicates the duties of the committee; these are given as follows in the General Orders:

> "The Committee are to assemble at such places and on such dates as may be fixed by the President, for the purpose of reporting on the causes of the unhealthiness which has existed at Kurnaul and other portions of the country along the lane of the Delhi Canal; the Committee will also report whether an injurious effect on the health of the people of the Dooab is or is not likely to be produced by the contemplated Ganges Canal."

The question of the healthiness or unhealthiness of the localities alluded to, is not one of sufficient general interest to justify its being brought before our readers, were it not that the medical member of the committee, Mr. T. E. Dempster, has suggested a test for the presence and intensity of malarious miasm, which, in the inquiry before us, seems to yield positive and satisfactory conclusions, and the value of which appears to have been fully established in the district in which the investigation was instituted. The object was to ascertain whether the Great Ganges Canal, which now unites the Ganges and the Jumna, but which, at the time of the inquiry, was in contemplation, would tend to increase disease, and thus prove not a blessing but a curse to the inhabitants of the country through which it was to pass. Opinion was at the time much divided on the subject; one party, chiefly composed of medical men, held that all the unhealthiness of certain districts was solely to be ascribed to the influence of existing canals and canal irrigation; another as confidently maintained that no sanitary question at all was involved in the construction of works of this nature.

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* For October, 1853.

In a country where there are no means of obtaining any medical or vital statistics, and where any sanitary research is regarded by the inhabitants with such suspicion that they use all their efforts to conceal facts or mislead the inquirer, nothing but a test that was beyond the control of the natives, could serve to elucidate the presence or absence of the marsh poison and its effects upon the system. Mr. Dempster proposed to examine the spleen in a certain number of individuals, and by the presence or absence of an enlargement to determine whether or not the residents in a certain locality had previously suffered from the endemic fever. The results of a laborious and extensive inquiry have most satisfactorily proved that the test is a reliable one. Major Baker and Mr. Dempster—

> "Examined the irrigated and unirrigated districts on both banks of the Jumna, and followed the proposed course of the Ganges Canal for eighty-three miles—viz., from Hurdwar to the latitude of Meerut. In the course of this inquiry they travelled about 1400 miles. They visited more than 300 inhabited localities, and personally examined upwards of 12,000 individuals of all ages." (p. 1.)

The mode in which the examination was conducted consisted in selecting, at each place visited, twenty children and twenty male adults, taken at random from the population. Those avowedly diseased were not encouraged to come forward, and only admitted into the lists when there were no others. Subjects from all castes were taken; and while Mr. Dempster conducted the medical examination, Major Baker entered the results in his note-book. Mr. Dempster observes, that—

> "As the great object was to make use of an unequivocal but easily-applied test, no case was ever registered as 'spleen,' *unless I had so distinctly felt the enlarged organ, that it could not be confounded with any other disease.* When the abdomen was natural, and the muscles soft and yielding, a satisfactory examination was generally obtained in the erect position; but if the belly was rigid, and the region of the spleen tumid, but not clearly defined, the subject was put flat on his back, with the knees bent and raised. If an enlarged spleen was not discovered after a *moderately careful* examination so conducted, the person was registered *free from the disease.* Sometimes, though rarely, really doubtful cases were met with, which could not at once be pronounced upon; these were put aside, and others examined in their stead."

Five different degrees of size were noted, according as the spleen projected more or less below the hypochondria. Numerous tables, embracing a minute analysis of the results obtained by this elaborate process, are appended, while a coloured map of the localities traversed shows at a glance the per-centage of enlarged spleens occurring at each of the places visited. We are unable to reproduce all the tables as we would wish to do, although extremely valuable in showing the accuracy of the conclusions arrived at, and the care with which the data have been collected. We have examined them all, and cannot but express our opinion, that "the chain of induction" formed by the writers of the Report is complete, as regards the locality and the morbid influences in question. The following table contains a summary of the whole inquiry:

			Per-centage of enlarged spleens. Adults and children of all classes.	Per-centage of adults suffering from fever. 1844.	1845.	1846.	Average depth of water from surface of ground, in feet.
Irrigated from the Western Jumna canals.	Delhi branch	Within half a mile of the canal	58	51	45	41	11
		Distant more than half a mile	49	51	49	40	18
	Rohtuk branch	Within half a mile of the canal	44	47	38	27	28
		Distant more than half a mile	29	34	34	27	48
	Bootana branch	Distant more than half a mile	16	41	36	22	102
	Hansi branch	Within half a mile of the canal	39	50	41	22	92
		Distant more than half a mile	18	40	31	16	118
Irrigated from the Eastern Jumna canals.	Northern division	Within half a mile of the canal	20	27	39	27	8
		Distant more than half a mile	22	37	47	30	13
	Central division	Within half a mile of the canal	59	63	54	31	8
		Distant more than half a mile	47	60	53	38	14
	Southern division	Within half a mile of the canal	25	48	40	17	24
		Distant more than half a mile	18	47	30	14	34
Irrigated from wells in the high land of the Dooab			8	37	31	20	24
Unirrigated.	Sikh states	Connected with the canal	44	47	52	26	0
		Unconnected with the canal	29	43	61	30	0
	Delhi territory	Unconnected with the canal	11	32	28	11	88
	Northern Dooab	High or bungor land*	3	32	30	13	46
		Ganges khadir†	21	41	42	28	25
		Near head of Eastern Jumna canal	6	85	43	27	0
	Naturally malarious localities	Nujufgurh jheels‡	44	42	59	57	15
		Valleys of Jumna and Hindun	34	46	42	31	14

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* *Bungor:* The high and firm bank of the river bounding the khadir.

† *Khadir* is the belt of moist, low, and often fertile land found alternately on one or other side of large rivers in these provinces. During the rainy season much of this land is submerged, and in it the river frequently alters its channel.

‡ *Jheels:* Shallow pools of water ,often very extensive during the rainy season.

It will be observed that in each subdivision of the table the smallest number of spleens and the correspondingly small number of previous fever cases, occur in the inverse ratio of the proximity to the river or the canal. The ratio of the previous fever cases does not quite agree with that of the number of enlarged spleens, but possibly if one could determine the intensity of the attacks, we should find that a more definite ratio could be established between the attacks and the spleens.

It is scarcely possible, on looking over this table, and knowing the care used in making the inquiries upon which it is based, to refuse to admit that there is a direct relation between malaria, the amount of splenic enlargement, and canal irrigation, in the north-western provinces of the Bengal Presidency. The facts thus elicited are of general interest, and may serve to suggest in other parts of India, other colonies, and in European countries, the utility of ascertaining whether the human body offers similar indications with regard to the influence exerted by definite poisons. There are other glandular organs which might be employed as tests as well as the spleen: thus, Professor Alison, in a former number of this Review,* observes:

> "It would seem to be nearly ascertained, by the observations chiefly of a clerical philanthropist, M. Billier, Archbishop of Chambery, that the origin of the poison producing goître and cretinism is nearly analogous to that of malaria; and no doubt the special conditions necessary to its very partial development will one day be made out."

We do not find that Mr. Dempster rides his hobby too hard. On the contrary, the great cautiousness with which he expresses himself with regard to the spleen test, strengthens his case considerably. He distinctly says that he does not assert it to indicate the presence of the remote causes of *all fevers,* or even of all pure endemic diseases of this class. While he admits that there may be different kinds of malaria, giving rise to fevers of different types, and having different complications and consequences, or that common continued and typhoid fevers become mixed up with and modified by fevers of local origin, he is fully persuaded that *marsh malaria* in its extended sense may be measured by the spleen-test, and that canals and canal irrigation have a proper connexion with that poison alone.

Having shown that the country was comparatively free from the marsh malaria previous to the formation of the Ganges Canal, it would be of extreme interest to know in what proportion it had manifested itself since the canal has been dug. It would not necessarily present exactly the same degree of intensity which it was found to manifest in the region of the Delhi Canal, because much depends upon the character of the soil and the extent to which at the same time drainage is introduced. On this point the committee remarked as follows:

> "In the course of our inquiries on the existing canals, we have found salubrity to depend, in a great measure, on the nature of the soil and the efficiency of the surface drainage. . . . . If attention to drainage be made an absolute condition in the benefits of the canal, an improvement, rather than a deterioration, of the general salubrity may in many instances follow the introduction of canal irrigation." (p. 5.)

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* April, 1854, p. 337.

The Committee considered it evident that the Dooab, through which the Ganges Canal was to pass, offered facilities for drainage not possessed by the region of the Delhi Canal.

A very remarkable instance proves the general accuracy of the spleen test, and the peculiar relation it bears to the marsh poison: the following observation, while it shows that poison to be unconnected with the poisons that result from the presence of effluvia generated where large bodies of human beings are congregated together, but unexposed to marsh effluvia, also confirms the fact that the marsh miasm is the real cause of the splenic enlargement:

> "The city of Delhi appears, at first sight, an instance in which the test failed; but on careful examination I think it will only be found to furnish a striking confirmation of its general accuracy. The medical topography of the city, civil station, and military cantonment of Delhi, is an extremely complicated subject, and involves a great variety of important considerations; but I need only briefly notice in this place the following particulars:
>
> "Within the walls, and especially in the most dense and crowded quarters of the city, there were comparatively few indications of pure malarious disease. This accords with what has often been remarked in other countries—viz., that the high walls and narrow, crowded, smoky streets of large cities are frequently a safeguard against marsh miasma, although other causes of disease may abound in such situations.
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> "In the cantonment bazaar and suburbs outside the walls, a considerable amount of spleen disease was found. But when we proceeded to examine the villages situated on the verge of the low moist 'khadir' land, immediately in front of the sapper lines (a position now abandoned, in consequence of its extreme insalubrity), the test at once pointed out malaria in its highest intensity."*

We cannot conclude this notice of the valuable inquiry carried out by Mr. Dempster and Major (now Colonel) Baker, without expressing a hope that we may again hear from them, and that the above summary of the results arrived at by those gentlemen may excite others to institute similar investigations, even though they may not have the means or the opportunity of operating upon so large an area, or examining an equal number of human beings.

REVIEW VI.

1. *On Stricture of the Urethra, and Fistula in Perineo.* By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, &c. &c. Second edition.—*Edinburgh*, 1855. pp. 109.
2. *Traité Pratique des Rétrécissements du Canal de l'Urètre.* Par M. le Dr. J. F. REYBARD. Ouvrage couronné par l'Académie Impériale de Médecine qui lui a décerné en 1852 le grand prix d'Argenteuil.—*Paris*, 1853. pp. 600.

A Practical Treatise on Strictures of the Urethral Canal. By Dr. J. F. REYBARD. The work selected for distinction by the Imperial Academy of Medicine, which decreed to him in 1852 the great Argenteuil prize.

IT was the recorded opinion of an old and experienced hospital surgeon who flourished at the commencement of the present century, that organic stricture of the urethra is a disease of considerable rarity. Without giving an unqualified assent to this proposition, expressed as it is in terms which are at least sufficiently vague, it may perhaps be correctly affirmed that the vulgar belief in the commonness of the affection is an extremely erroneous one. This is not a remark made without consideration, but a conclusion based upon a tolerably large experience of the misapprehension which exists in the minds, not only of patients, but sometimes even of members of our own profession, respecting its existence in particular cases. How frequently it happens that we meet with individuals who, having for some time experienced symptoms indicating more or less of undue irritability about the urethra or bladder, labour under the impression that they are the subjects of stricture, and have, perchance, been treated pretty extensively for it too, especially if they have fallen into the hands of extra-professional pretenders, while they are, nevertheless, perfectly free from any narrowing of the urethral canal, either of a permanent or a temporary character. On the other hand, it is manifest that a disease, some veritable examples of which may always be found in the wards of any one of our large metropolitan hospitals, cannot be esteemed a very uncommon one.

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* Appendix B, by Mr. Dempster, p. vi.

A glance at the numerous works which have been written during the last few years, both in this country and in France, respecting urethral stricture and its consequences, might certainly tend to confirm the popular impression. A prolific literature in connexion with any particular subject in pathology is not, however, to be accepted as absolute proof that the matter discussed either possesses very ample limits, or is presented for investigation in a very extended field. It quite as commonly indicates that considerable difference of opinion exists in relation to some question arising out of the subject; and thus we may observe, that as difficulties and discrepancies become in course of time cleared up, the still remaining points of difference alone afford themes for authorship and debate, until they in their turn are settled to the satisfaction, at all events, of the existing generation. Yet most certain it is that, when these discussions are lost sight of and forgotten, a future age will again witness revivals of the old contest, and many a dogma bearing the hitherto unquestioned seal of ancient authority will be again examined, disputed, and perhaps overthrown, by later and better-informed investigators.

At the present moment, notwithstanding all that has been done and written concerning the subject in question, we can point to no fact more strikingly illustrative of these remarks than the nearly cotemporaneous appearance of two works, the titles of which are placed at the head of this article—the productions of two labourers in their respective fields—no theorists merely—both undoubtedly seeking truth with zeal and earnestness, yet arriving apparently at the most opposite results—each in the strongest confidence that his own achievement will alone bear the test of time and experience. And yet with great seeming—nay, real difference—it is pleasant to observe—or to think, at least, that we can trace—the one unchangeable form of truth in both, overshadowed though it may be by much of error, as we believe, in one of the two productions before us. We make no apology for thus placing, but for a moment only, in one category, as it were, our illustrious *confrère* of the north, to whom modern surgery is so greatly indebted, and the French author whom the acquisition of a literary prize has for the first time rendered famous. Both assume to have made a large advance in relation to the pathology and treatment of the subject of which they treat; and the claim of the latter, although not enhanced by any weight hitherto attaching to his own name, is endorsed by the favourable verdict of no less a body than the Imperial Academy of Paris.

We shall attempt to give a brief sketch of each of the works before us, taking first the volume with which, it may be presumed, we are in this country least acquainted.

It may be known to most of our readers, that the late Marquis d'Argenteuil devised property in France, affording annual proceeds of nearly 2000f., for the purpose of founding a prize to be devoted to the object of encouraging and rewarding improvements in the mode of treating stricture of the urethra, and that he gave authority to the Imperial Academy of Medicine at Paris to adjudicate upon the claims of all who might become candidates for this distinction. This body decided on admitting competitors to the concours once in six years, so as to afford a fair interval for permitting the annual income to form, by accumulation, an attractive prize. The first concours was held in 1846, when M. Leroy d'Etiolles was the successful competitor; the second took place in 1852. On this latter occasion, with which we are now concerned, the commission of the Academy appointed to decide on the merits of the respective candidates, was composed of MM. Ricord, Robert, Roux, Gerdy, Bouvier, Huguier, Langier, Larrey, and Grisolle. The prize amounted to 12,000f., and was awarded to M. Reybard, formerly of Lyons. This gentleman's essay, enlarged from its original dimensions, forms the work in question, and contains an exposition of his views and experience of stricture in general, but relates, for the most part, to his own peculiar mode of treatment, extending altogether to about 600 pages.

The foundation of M. Reybard's method is to be traced, at the outset of the work, in the views which he takes of the causes and constitution of organic stricture; and these must be briefly explained, in order to appreciate the *rationale* of his mode of treatment. Moreover, he himself lays considerable stress upon the necessity for carefully following his exposition of this part of the subject.

Passing over the first book, in two chapters, devoted to the consideration of the anatomy and physiology of the urethra, and containing nothing requiring remark, we find the second book opening with the usually accepted definition of stricture, and an enumeration of the causes of the affection. The first class of these comprehends traumatic lesions of all kinds, whether occasioned by accidental sections, lacerations, and bruises of the urethra, or by surgical operations with instruments and chemical agents. This is divided into three groups—Ulcerations, Incised Wounds, and Contusions. Under the first head, in order to show the relation between ulcerations of the urethra and the subsequent formation of stricture, the author details certain experiments which he made upon the urethra in dogs. He passed small quantities of nitrate of silver to a certain spot and left it there, noting the results during life, and killing the animals six weeks afterwards to observe the pathological changes which had been induced. As might be expected, the loss of substance had resulted in the formation of a cicatrix, and the production of considerable narrowing of the canal. On three other dogs he performed the same experiment, subsequently employing dilatation, with little or no benefit, during a considerable period. In discussing the second group, that of incised wounds, M. Reybard is at some pains to show, that while transverse sections of the urethra are commonly followed by considerable narrowing, longitudinal incisions of the canal have no such effect. In his remarks upon the third group, that of contusions, it is not necessary to follow him.

The second class of causes to which M. Reybard invites attention, is that which includes urethral inflammations of all kinds. This morbid action, he says, all authors have agreed to consider as the chief and most common cause of organic stricture; but they have not, he affirms, inquired into, or at least have not discovered, either the relation which subsists between that action and the production of the abnormal tissue constituting stricture, nor the physical characters and properties which it possesses. He claims solely for himself the merit of having supplied this desideratum in our knowledge, and devotes about sixty pages to an elucidation of his views of this subject.

The sum of these may thus be very briefly stated, and, for the most part, in the author's own words:

> "Inflammation is a cause of stricture upon one condition only—viz., that of giving rise to an abnormal tissue of new formation, and this with such an invariable certainty, that the same cause in the same circumstances will always produce an identical effect." (p. 94.)

The inflammatory process, in order to give rise to stricture, must extend beyond the mucous membrane; it involves, also, the sub-mucous tissue, and, it may be, the inner sheath of the corpus spongiosum, or even the entire structure of that body. Stricture is, therefore, the more likely to succeed a urethritis "in proportion as the inflammation is profound, localized, and chronic." (p. 97.) Having declared it to be impossible that mere congestion of the part can form the condition which we understand as stricture, and that the latter has no existence except in the presence of an organized deposit in or around the urethral walls, and resulting from inflammation, he next examines its anatomical and physiological characters:

> "The tissue which enters into the formation of stricture is," according to Reybard, "that of granulations (*tissue inodulaire*) as met with in ordinary cicatrices. . . . . It forms itself at the expense of those plastic products which inflammation has evoked and deposited in the substance of the urethral walls." (p. 112.)

The thickness of the deposit is generally very inconsiderable, less than it is customary to suppose (p. 116); its consistence is more considerable than that of the natural structures, and it augments with time. It exhibits certain physiological properties, which possess much interest and importance in the estimation of the surgeon—viz., "retractility," "extensibility," and "elastic retractility." Thus it is asserted by Reybard, that all strictures have a natural, inherent, and unconquerable disposition to contract in the course of time, or become confirmed by age, whether subjected to dilatation or otherwise, and this he designates "retractility;" which contraction is described as "an atrophic process," common to it and to ordinary cicatricial tissue. By "extensibility," he intends that property by virtue of which all, excepting contractions of the most confirmed character, may have their calibre temporarily increased by dilatation; and by "elastic retractility," that tendency to return, more or less speedily—which all strictures exhibit after treatment by sounds or bougies—to their original abnormally-diminished calibre. All cases, therefore, exhibiting these properties are termed by Reybard "dilatable strictures," while those confirmed examples which do not admit of extension are called "non-dilatable." In using the term "dilatable," he desires it to be clearly understood that he does not intend to describe by it a stricture *curable by dilatation*, believing that none are, or can be, so cured—that is, placed beyond liability to return; a doctrine deduced from two propositions involved in the foregoing—the first, that all organic strictures are invariably constituted by the tissue above described; the second, that this tissue invariably possesses an inherent tendency to contract or shorten itself.

This deduction leads him, therefore, to propose as the only cure for strictures, whether slight or considerable, old or recent, a free incision of all the tissues composing the walls of the urethral canal, so as to divide completely the strictured part, and a portion of the healthy structures, both before and behind it.

Before, however, entering further into the details and *rationale* of the operation proposed, it is only fair to English pathology to remark, that the intimate nature and physiological properties of stricture, as expounded by M. Reybard, possess no novelty for surgeons on this side the Channel. The deposition of plastic matter in and around the urethral mucous membrane, and not a condition of "vascular engorgement," which, according to Reybard, had hitherto been considered (in France?) the proximate cause of stricture, has long been recognised in this country as the uniform and essential condition which produces permanent contraction of the canal. This fact was demonstrated by Sir Everard Home in an elaborate paper published in the 'Philosophical Transactions' in the year 1820. The same fact has been subsequently insisted on and illustrated by Sir B. Brodie and others. We may perhaps be also permitted to add, that in our own work, presented to the Royal College of Surgeons in 1851, the histological elements of stricture were carefully examined and described, and it was shown that they were identical with those of the cicatrices of burns, and of the fibroid tissue which results from inflammatory action occurring in internal organs; an additional proof of the identity of the material in the two cases, not adopted by M. Reybard. Indeed, this author is obviously quite independent of any aid which microscopical investigation might afford him in the study of the minute anatomy of the stricture elements, as may be learned from his account of them in a case examined by himself, and cited in elucidation of this very subject. Thus he writes: "The stricture was fibro-cartilaginous; and its tissue—of a lustrous white—had the consistence and, without doubt, *the same organization* as that of cartilage!" (p. 108.) But notwithstanding that we have long entertained here these views of the constitution of stricture, we have not been led to accept as a necessary consequence the therapeutical conclusion at which he arrives. Admitting that all organic strictures are constituted by this abnormal tissue, and that its obnoxious property is to exhibit a natural tendency to increase rather than to disappear, it by no means follows that a complete section of the tissue should be the only mode of cure remaining in our hands. That the judicious use of dilatation is a perfect cure for some cases of organic stricture, the most experienced surgeons of this time will attest. They do not explain the fact by attributing to the bougie or catheter an action which is mechanical merely, but are impelled to the belief by facts which experience has made patent, that some action, so-called "vital," must arise from the pressure exerted upon the abnormal elements of the stricture; an action which may be presumed to dissipate them, as it has been supposed, by absorption, and, consequently, to destroy the contractile tendency which before existed in or about the affected portion of the urethra. This influence of the sound M. Reybard does not recognise; he believes that dilatation is never anything more than a mechanical agent, and therefore temporary only in its action; and that its employment rather tends to augment the stricture, by inducing fresh chronic inflammation, and consequent renewed deposit of the obnoxious material around. Hence he proposes an invariable resort to the knife for the cure of stricture. But researches in the morbid anatomy of stricture, a branch of inquiry pursued much more extensively in this country than in France, as our respective museums testify, bid us demur to the dogma that the organic obstruction is invariably constituted by a layer of the plastic matter described. A stricture which is recent and slight, but which, nevertheless, M. Reybard would treat by incision, and by a long train of subsequent manipulations, in order to keep that incision open, as we shall hereafter see, is not necessarily the result of plastic matter *surrounding the canal*. A much less considerable deposit than this—indeed, the mere thickening of the mucous membrane of the urethra, or a deposit affecting only one of its sides, or a little cord of fibroid matter stretching beneath the membrane for a short distance, are, either of them, sufficient to cause a very notable and inconvenient contraction of the canal. These conditions we have verified by autopsy, and have elsewhere described as constituting the most simple, perhaps incipient forms of stricture, each probably constituting an example of the disease completely curable by dilatation: in other words, there exists good ground for believing that many of those cases which we are satisfied do completely disappear under dilatation, and do not return, are cases possessing the anatomical characters described.

Of late years, however, surgeons in this country have been more and more inclined to admit that dilatation fails to effect a complete cure in a considerable number of cases, and have therefore resorted to various contrivances for extirpating an obstinate obstruction by some method of cutting or cauterizing; but we think few, if any, will be inclined to coincide with M. Reybard's absolute rejection of the bougie as a therapeutic agent, and will naturally feel somewhat startled at his proposition, and his practice of reserving it solely for the purpose of making room for the knife, or for "accustoming," as he has it, the urethra to the presence of foreign bodies, previous to his operation of urethrotomy. On the other hand, most practical men will agree with M. Reybard's repudiation of certain methods which, as they were originated in his own country, have always found the greatest acceptance there—viz., those of attempting to estimate the nature of a stricture by means of "model bougies," particularly the "bougies à empreintes" of Ducamp. It is not without satisfaction that many will observe the sign of a return to a simpler and sounder practice on the part of our brethren beyond Channel, in the following notice of these instruments: "Nothing," says M. Reybard, "can be more deceptive than this method of appreciation, and we shall see further that it has led to the most deplorable abuse of caustic" (p. 144): an agent the employment of which, let it be remarked, he denounces on several occasions in very forcible language. But he regards as a useful means of estimating the extent and situation of a stricture, the small sound with a bulbous extremity, made in various sizes, such as has long been employed here, of a form somewhat modified from the original pattern of Sir Charles Bell.

In entering upon the much discussed subject of treatment, M. Reybard announces his method in the following somewhat positive terms:—"My principal aim is to demonstrate that urethrotomy practised from within outwards, and, according to the method which I have proposed, constitutes the *only curative treatment* of stricture." (p. 205.)

After premising that all intra-urethral incisions, such as those made by Stafford and others in this country, by Amussat, Leroy, Ricord, Tanchou, Delcroix, and others in France, whether described as scarification or urethrotomy, have been radically defective, at the best palliating, but never effectually removing the complaint, he repudiates their employment except when necessary to ensure space in the urethra for the passage of his own cutting instrument, which, as will be hereafter seen, is almost large enough to fill a healthy urethra of average capacity.

The practice of M. Reybard's method is conducted as follows. Having passed a sound through the stricture, and dilated it, until it will admit with tolerable ease an instrument of the size of No. 9 or 10 of our scale, he passes his urethrotome, which consists of a canula, containing a rod having attached to its distal end a slender blade nearly or about an inch in length, so arranged that this latter can be exposed with ease and certainty to any required extent. This blade having been carried, while concealed in the canula, completely through the stricture, is then exposed and drawn towards the operator, so as to divide the whole of the stricture, together with the entire thickness of the urethral walls, for an inch behind and an inch before it, making usually a wound about three inches in length. He advises that the blade should then be thrust backwards and forwards three or four times, so as to ensure the effectual division of the tissues, both in respect of length and depth. The hæmorrhage, which is frequently considerable, is to be arrested if necessary by passing down to the incised portion of the urethra a hollow bag of caoutchouc rolled into the form of a bougie, and capable of being inflated with air or water while *in situ.* The subsequent treatment consists in passing a full-sized bougie, or some special dilating instrument, twice a-day on not less than thirty or forty consecutive days, in order to maintain apart the borders of the wound in the urethra, to prevent union by the first intention, ensure the production of granulations, which shall afterwards constitute a long "intermediate cicatrix," and thus, by the formation of a piece let into the side of the urethra, as it were, produce a permanently enlarged canal. This operation M. Reybard appears to have first practised upon dogs, in which he had previously artificially produced a condition which he calls stricture, by wounding or cauterizing the urethra; after a suitable time he performed his operation upon them, and after another interval killed and dissected them, in order to observe the production of the "intermediate cicatrix."

The instrument which M. Reybard employs presents no kind of novelty in the principles of its construction, as compared with one of those used by Stafford. In form and some of its minor details it is modified. Thus, the blade is much larger, on account of the much greater depth of incision it is required to make. A dilating apparatus is also attached to its sides, consisting of two thin flat rods of spring-tempered steel, which can be made to project and dilate the urethra by a very ingenious mechanism, in order to stretch the mucous membrane before commencing the incision, and so ensure its complete accomplishment, in case the contraction is not sufficiently narrow to grasp and steady the instrument in its place! The size of the instrument equals No. 9 of our scale, but the action of the appended dilating rods is capable of increasing this enormously. Altogether the apparatus is a remarkable specimen of ingenious mechanical contrivance and finished workmanship, which no description, much less the very rough drawing of it in M. Reybard's work, can convey. A specimen of it lying before us at this moment, by the well known Charrière, of Paris, must be regarded as one of the *chef-d'œuvres* of that celebrated instrument maker.

M. Reybard admits the occurrence in his practice of very serious accidents consecutive to his operation. He describes a case in which he very nearly lost a patient from bleeding into the bladder. Hæmorrhage to a considerable extent he regards as a frequent and natural consequence of the operation. It will be considered in England that this is not a circumstance very unlikely to happen, since he advises the section of the contracted part to be directed towards either *side* of the urethra, and not in the median line along the floor, in order to avoid cutting the artery of the bulb (p. 385). In this country we lay great stress on the importance of the exactly contrary practice of cutting *in the median line in order to avoid those arteries*, and find it tolerably successful. M. Reybard's appreciation of the situation of the arteries in question has excited not a little astonishment in some quarters in this country, as may perhaps be already known to some of our readers.* Infiltration of urine he regards as another accident which may occur, although it has not happened more than once in his practice. In order to avoid it, a gum catheter is to be tied in for the first forty-eight hours. Violent febrile attacks are not infrequent consequences of the operation, but he denies that any objection arises out of the fact, because they are known to occur also after cauterisation, and even sometimes after simple dilatation; adding, that he has only lost one case in that way. Considerable infiltration of blood into the cellular tissue of the penis and scrotum, causing great enlargement, has occurred in a considerable number of cases, so much so as to have been regarded by the author at one time as the necessary and characteristic sign that a sufficient division of the urethra had been made!

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* The manifest anatomical error committed by M. Reybard in relation to the situation of the arteries to the bulb, unnoticed as it was by the Commission of the Imperial Academy, was pointed out by Mr. Syme, in a letter to that body, which communication, although acknowledged and referred to a special commission for consideration, was ultimately allowed to pass without reply.

The manipulations necessary for the performance of this operation, in such a manner as to fulfil accurately the indications necessary to its success, are said not to be easy, and a recommendation is given that they should be carefully practised several times upon the dead body before attempting them on the living subject. When a stricture presents itself which defies the skill of the surgeon to pass any instrument through it, M. Reybard proposes to puncture it a few times with an instrument from which a lancet can be made to project at the extremity, precisely after the first method of Stafford, and afterwards, when overcome and sufficiently dilated, to apply the urethrotome in the ordinary manner. This last proceeding—the mode of treatment, in short, which the author proposes to apply to those forms of the disease which constitute the really difficult cases—it surely will be unnecessary seriously to discuss, as it was long ago practised and since exploded here.

In endeavouring to arrive at a fair appreciation of this mode of treatment, claiming, as it does, to mark an era in the history of the therapeutics of this often very obstinate and serious complaint, and to be the only mode yet discovered of curing organic stricture, we must first inquire what are the essentials of the proceeding so characterized; in what respect they differ from those of operative measures hitherto employed; and lastly, whether our pretty extensive experience of stricture in this country permits us to acquiesce in the belief, that any single method of treatment at present known, can fairly lay claim to be the only curative one, to the exclusion of all others.

First, the essentials of M. Reybard's proceeding are, (*a*) The production of a long incision through the strictured part of the urethra, and the adjacent healthy parts, made in a direction from within outwards, and within the urethral canal, with no external wound, but sufficiently deep to divide thoroughly all the morbid tissue constituting the stricture; an incision which may err in not being sufficiently free, but can scarcely err, according to its author, in the opposite direction. (*b*) The production of a granulating wound by mechanically opening up the incision twice a day, so as to ensure the formation of the intermediate cicatrix, and thus enlarge the urethra permanently, to an extent corresponding with the breadth of the fibroid tissue which has intervened between the margins of the wound, as the result of the granulating process. This proceeding differs from all operations by internal incision of the urethra previously adopted, in the extent both as regards length and depth which characterizes the cutting operation in which it commences. It differs in its after-treatment, both in rationale and practice, inasmuch as it aims at the formation of the cicatrix described.

But, secondly, the complete division of the most inveterate forms of stricture by the knife, although quite permeable to instruments, has nothing of novelty in it for British surgeons, albeit it has hitherto been foreign, except by report, to France. Mr. Syme, as nearly all the world might know by this time, has practised and recommended complete division in the cases denoted, although, not like M. Reybard, as an application for all forms and degrees of the complaint. He has, for reasons identical with those adduced by M. Reybard, advised the employment of a moderate amount of dilatation after the operation, on the express ground that union of the wound by the first intention does not produce such favourable after-results as the slower process of union by granulation. So far the principles are identical, but the great distinction between the two proceedings consists in this:—M. Reybard works in the dark, and makes incisions within the urethra visible to no mortal eye. Mr. Syme, cutting from the surface inwards, performs a free division of the stricture; and having guarded the surfaces of the wound from contact with urine during forty-eight hours, by a catheter tied in, encourages the urine, as well as the products of action in the wound, the purulent secretion, &c., to pass freely off through it—a method which ensures the desired union by granulation, and saves the thirty or forty days of dilating, to say nothing of the occasional introduction of a hooked instrument ' like a lithotrite," invented and employed by M. Reybard for opening up the wound effectually, by driving the beak into it, when mere dilatation fails to keep the margins asunder. (p. 386.)

Will any one designate M. Reybard's proceeding the less considerable, the less difficult, or the less dangerous of the two operations on *primâ facie* grounds? Probably no one in this country. Not so, however, M. Robert, the reporter of the Commission to the French Academy. In slightly adverting to the operation of Mr. Syme, in his report on M. Reybard's paper, he represents it as the more serious, because in it "division of the soft parts in the perineum" is practised. But, provided that this incision lies wholly in front of the deep perineal fascia, from what source is the additional danger to arise, the urethra being freely divided in both cases? Do we not constantly inflict wounds of this character in the perineum in cases of extravasation of urine, and thereby save life, by permitting the escape of pent-up matter from the vicinity of a damaged urethra, the dangerous result which we always most fear when lesion of the urethral walls has been produced, unless a free external opening also exists—this being the very danger, moreover, to which M. Reybard's procedure most exposes us? I conceive it must be admitted that the internal incision of M. Reybard is considerably more formidable than the external incision of Mr. Syme, on two accounts—first, because its extent (about three inches) is so great, that for strictures situated near to the posterior limit of the bulb, the deep fascia must inevitably be divided; and, secondly, because the necessary confinement of the products of inflammation, augmented as they are by the frequent introduction of various instruments subsequently, involves a state of greater hazard than one in which there is a free channel of escape not only for them, but also for the urinary secretion.

As to the question of whether or no stricture can be cured by dilatation, it is one which scarcely requires discussion here. M. Reybard, in one part of his work, lays it down as an axiom, that if a stricture does not appear within "one or two months" after any treatment, the patient is safe from relapse, and the treatment must be declared successful. (p. 484.) This remark, it is true, is intended to relate to the result of his own cases of urethrotomy, enabling him to report them "cured" at a very early period. But is not this general admission altogether fatal to his system, grounded as it is upon the alleged inadequacy of dilatation to cure? Indeed, it saves us the trouble of disputing that point; for the daily practice of every surgeon who has had much to do with stricture, will produce cases without number, in which the patient has exhibited no relapse, within one or two months after treatment by dilatation, whatever he may have subsequently done. But it is easy enough to adduce examples of even narrow stricture, which during years since the treatment by dilatation, have exhibited no tendency to return. We do not hesitate to say, that one or two months is a period by no means long enough to test the lasting value of any mode of treatment. A much more extended term is necessary, in order to decide whether the freedom from contraction which a patient has obtained by means of any operative proceeding, is really permanent or otherwise.

In bringing to a conclusion this brief notice of the work before us, we feel bound to repudiate in the strongest terms the invariable appeal to the knife which M. Reybard declares to be necessary for the cure of stricture. In respect of internal incisions generally, as performed by any of those modifications of the primitive instruments which have sprung into existence during late years, each presenting some exceedingly slight difference from its predecessor, in accordance with the taste and fancy of its advocate, the opinion is certainly gaining ground that such measures, when applied to strictures affecting the posterior part of the spongy portion of the urethra—their most usual situation—are either useless or hazardous; the former if the urethral walls are not freely cut, the latter if they are. If the stricture be so confirmed that it will not yield to dilatation when carefully applied, so as not to occasion or augment irritability of the urethra, it may be taken for granted, as a rule, that a slight notch will not facilitate its disappearance. If it be granted that an extended section of the urethra is required in order to effect that which other means have failed to accomplish, then that section which is unconnected with any external outlet, and permits the occurrence of internal hæmorrhage and infiltration, must be held to involve a risk we are not justified in encountering.

Notwithstanding the pretentious claim set up on behalf of M. Reybard's method, we are satisfied that the most experienced surgeons here would feel themselves extremely ill-provided for the treatment of stricture did they possess only his single remedy, in the form of a cutting instrument, so large as to require a urethra almost of the natural size for its reception. So dangerous, so unnecessary, even so futile an application do we consider it to be, that it is really difficult to deal seriously with a proposal to employ it. And we feel compelled to say that we have done so only because serious men, as we have seen, have seriously presented the matter to the profession.

At this point we may naturally turn to the short monograph of Mr. Syme. It requires but a slight acquaintance with it to learn that its object is a special one—viz., to explain and advocate the peculiar mode of treatment which its author has adopted in obstinate forms of stricture. First issuing in a distinct volume in the year 1849, it appears now as a second edition, but is nearly re-written. In few words we will state in what Mr. Syme's method consists, as a good deal of misapprehension appears still to prevail respecting it, both at home and abroad.

As a rule, then, Mr. Syme, in common with surgeons generally in this country, treats stricture by simple dilatation, eschewing the use of caustics and internal incisions. So firmly persuaded is he of the efficiency of the catheter, that he believes there is no stricture, however narrow, which will not admit an instrument, provided it be sufficiently small, and be employed with care and patience. He therefore believes that it is wholly unnecessary to resort to the operation of dissecting through what have been termed impermeable strictures, preferring to insinuate a catheter rather than to employ a knife in these circumstances. Hence he disapproves of that operation usually termed "the perineal section," which has been frequently resorted to in this country for the last thirty-six years. Having proceeded to employ dilatation, if he finds that the stricture rapidly re-appears in spite of it, or that the process involves much constitutional disturbance, he prefers to divide freely the stricture from the perineum, upon a grooved director, performing the incisions in the median line, tying in a catheter for forty-eight hours, and subsequently passing it a few times at about weekly intervals. This proceeding, to which he gives the name of "external division," is stated by him, on the ground of an extensive experience, to be devoid of danger, and generally to be attended with a successful result, a conclusion with which our own experience of it leads us, without any hesitation, to coincide.

The observations which first attract our attention in the work before us, have reference to the old question of impermeable stricture. Setting aside actual obliteration, Mr. Syme simply affirms that when urine passes out by the urethra, a catheter of appropriate size may always, with sufficient care, dexterity, and perseverance, be sooner or later passed into the bladder. On this point we yield adhesion to the principle announced, regarding a belief in it as a valuable and important acquisition to the surgeon. No doubt but that the man who most trusts his catheter will handle it most successfully, while he who uses it under the abiding impression that the trocar or the scalpel may be always employed to expiate a failure, will not overcome difficulties which the former will surmount. We are therefore glad to learn what has been Mr. Syme's later experience in relation to difficult catheterism. He candidly tells us that since he has made his well-known statement respecting impermeability, he has been unable, in the cases of three patients, to pass an instrument of the smallest size from the external meatus to the bladder, by means of manipulation only. This admission exhibits a fact of great interest. Did he puncture the bladder, or dissect through the stricture, in these cases? By no means: let us hear his own account of the proceeding:—"Indeed, on three occasions—one in private, and two in public —I found it necessary to open the urethra anteriorily to the stricture, so as to obtain the assistance of a finger placed in the canal, to guide the point of the instrument." (p. 36.) And we subsequently find details relating to a case in which the stricture being complicated with a false passage, it appeared impossible to insinuate the smallest instrument through the contracted channel, until, the urethra being opened immediately anterior to the stricture, the operator was enabled to guide onwards with his finger the point of the catheter through it (pp. 93-6). We are bound to regard the extreme paucity of exceptions as a striking confirmation of the rule announced by Mr. Syme.

There is another observation which it is impossible to pass over in silence, relating to the part of the urethra which is affected by stricture. The author has on various occasions questioned the accuracy of writers in general who have treated of this subject. It is quite certain that we are even now constantly hearing of "strictures in the membranous portion," and not long ago it was common to speak of them in the prostatic part, or even "at the neck of the bladder." Such remarks always rested upon fallacious impressions received by operators in the use of instruments upon the living subject, and not upon anatomical observations, which, it need hardly be said, can alone determine the question. Discussing the merits of his operation, Mr. Syme writes the following respecting this matter:

> "The only sources of danger that can be attributed to the operation, are bleeding and extravasation of urine; and in order to estimate the importance due to them, it is necessary that the true position of strictures should be ascertained. If they existed in the prostatic or membranous portion of the canal, extensive incisions, involving the deep fascia of the perineum, would be requisite; and, accordingly, this has been made a serious objection to my proposal by writers who quote the authority of Sir B. Brodie and others to prove the occurrence of stricture behind the bulb. But the fact is, that the seat of contraction is never so far back, and may be positively limited to that portion of the urethra which extends from the bulb to the orifice. The ground upon which I make this statement is, that in all my experince I never found it necessary to cut further back than the bulbous portion, for the conveyance of a full-sized instrument into the bladder." (p. 41.)

Now, with a view to the elucidation of this question, we undertook, some few years ago, a careful and laborious examination of every preserved specimen of stricture in the museums of London, Edinburgh, and Paris, comprising above 300 preparations, the results of which were published in our work before referred to. This examination confirmed Mr. Syme's observation to a remarkable extent, so far as it related to the non-existence of prostatic stricture, and to the *extreme rarity* of the affection in the membranous portion; but three or four preparations there are in this metropolis, in which, beyond all doubt, it does exist in that situation. And this, as a pathological fact, however slight its importance, may not be altogether lost sight of or ignored.

Any addition to our knowledge of the circumstances which may have occasioned failure after the performance of the operation, is extremely acceptable. Subsequent contraction at the site of the incision has been attributed by Mr. Syme to various causes, for the most part preventable. Among these, he points out more forcibly than ever the undesirable influence of union by the first intention in the incisions which have divided the obstruction. Thus he says—

"The most obvious and certain causes of relapses would appear to be adhesion by the first intention, between the edges of the incision made through the strictured part, which must restore the state of matters that existed previously to the operation. It might be expected, indeed, that the stream of urine passing over the raw surface would effectually prevent any such occurrence; and so I believed must be the case, until taught a different lesson," &c. (p. 56.)

In closing this necessarily short notice of the two works before us, we will advert to a hint given at the outset, respecting the existence of *one common principle of action* as the basis of the two proceedings referred to. Differing as those two methods do in application, *toto cœlo*, each rests solely upon recognition of the same fact, one which, until recently, was almost everywhere unacknowledged or unknown. Both aim at one result; both are alike valueless, nay more, are mischievous and dangerous, if the following proposition is not now proven—viz., that there are some cases of stricture in which either the deposit occasioning the contraction is so considerable in extent or density, or is associated with so much irritability of the urethra or of the system at large, that mere dilatation does not remove, and sometimes scarcely palliates the complaint; and that in these cases a free division of the diseased structure, not permitted to unite by first intention, affords a fair probability certainly of greatly diminishing, and often of completely curing, the disease.

For such cases, and for such only, we advocate that mode of division which Mr. Syme has practised, as the safest and most efficient treatment which experience has yet brought to light.

Henry Thompson.

REVIEW VII.

1. *Medical Testimony and Evidence in Cases of Lunacy.* By THOMAS MAYO, M.D., F.R.S.—*London*, 1854.
2. *On Medico-Legal Evidence in Cases of Insanity.* Being the Third Lettsomian Lecture. By FORBES WINSLOW, M.D., D.C.L.—*London*, 1854. pp. 77.
3. *Tableau Analytique des Maladies Mentales à l'Usage des Jurisconsultes et des Médecins.* Par Dr. J. PARIGOT, Professeur Hon. de l'Université de Bruxelles, &c.—*Gand*, 1854. 12 t. Folio.

Analytical Table of Mental Diseases, for the Use of Lawyers and Medical Men. By Dr. J. PARIGOT, &c.

THE true relation existing between crime and insanity is a question of the deepest importance to every member of every society in which laws exist and in which crime is possible; for every man is liable to become the subject of insanity, and thereby to become a criminal. A slip of the foot causing a blow on the head, an attack of fever with cerebral symptoms, and many other of the commonest accidents of life, may not only upset the balance of the stoutest minds, but may result in placing the most innocent and conscientious of men in the position of a criminal before his fellows. Dr. Johnson said that the saddest and most humiliating subject of thought was the uncertain tenure of the possession of reason; and the truth of this becomes more evident when we reflect, that when reason is gone there is no tragic act of which we may not become the authors; that every man capable of reflection must recognise as a possible event of his life that he may imbrue his hands in the blood of those whose life is dearer to him than his own.

The interest and importance of this question is greatly augmented by the nature and magnitude of the crimes which arise from insanity. Murders of the most fearful and unnatural kind, wholesale domestic slaughters, assassinations and attempts to destroy the kings and rulers of men, these are crimes which result more frequently from insanity than from any other cause. To what extent the wide-spread desolations which have resulted from the crimes of kings may justly be attributed to criminal insanity, we shall never know. The motives of powerful men, and especially of despotic rulers, are too obscure to be fathomed even by observers who are close to the event in time and place; and when we attempt to appreciate those which have become matter of history, we can only feel that—

"The times that are gone by
Are a mysterious book, sealed with seven seals."

The great ones of the earth have been peculiarly liable to insanity, from the time when Nebuchadnezzar presented the first instance of *zoanthropy*, to the days when Russia was overrun by the Swedish madman; even to the present time, when Europe is afflicted and affrighted by the gloomy and fanatical successors of him by whom the mad Swede was overthrown. The account has yet to be written of that criminal insanity in high places, whose overt acts have been wars and the desolation of nations. Our present inquiries take a humbler range.

We shall commence our analysis of these works with that of Dr. Mayo, an attention to which it is entitled, if not on account of the priority of its date of publication, at least and without dispute by reason of the antiquity of its opinions. M. Comte has shown that in all sciences, after the rude and barbarous period when men attributed all natural phenomena to the direct influence of deities,—when the winds were unchained, the storm aroused, or the thunderbolt darted, by the hands of Æolus, Neptune, or Jove, or by their representatives,—and when madness was attributed to the anger of the gods in general,—there is always a transitional period before men arrive at that of positive science, in which natural phenomena are attributed to metaphysical causes. This period, which M. Comte designates the metaphysical, is one of essences and immaterialities. It is in this period of pathological science in which at the present time we find Dr. Mayo.

"In respect to the actual changes which may be worked in the human being under insanity, it must be remembered by those who engage in this arduous inquiry, that they must be contemplated as concerning our immaterial phase of being in a light much more exclusive of that afforded by physiological considerations than we are accustomed to assume. That there should be a *disease of the mind in the abstract*, that such diseases should work changes in us, viewed in this light, analogous to the physical changes of our bodily organs, is neither unnatural nor inconceivable. A *parasitical growth*—if, for want of a proper term, I may borrow this epithet from physical speculation—may take place under such disease, itself possessing vital functions and energies, but having no other relation to matter than the obvious one on which the tenure of our present life is based, namely, that we have an immaterial and a material being indissolubly bound together for the duration of that life; while, for anything we know, the immaterial element may be just as subject to its proper affections as the material one is. The above remarks may, at least, have a wholesome tendency to keep before us in our speculations the immense fund of mental disease that may exist, inappreciable through any knowledge that we at present possess of phenomena so little capable of being made the subject of experiment, or even observation, as those I am supposing."

This theory is beyond the pale of scientific argument. At least, it is beyond our reach, and must enjoy the usual immunities of incomprehensible metaphysics:—Disease of the mind in the abstract; a parasitical growth of our immaterial existence, that which has neither length, breadth, nor substance; covered with lichens like an old apple-tree, or swarming with objectionable vermin like a dirty beggar! To what indignities may not the immaterial soul be subjected!

But granting to Dr. Mayo the undisturbed possession of his theory, that insanity is an affection of the "immaterial phase" of our being, and that it is a disease of the mind in the abstract, have we not a right to inquire in what manner he will distinguish this affection from that condition of the mind which produces crime? Granting the immaterial nature of the mind, and that insanity is a disease thereof, parasitical or otherwise; what is vice? what is wickedness? from what fount in the caverns of the soul does the dark stream of crime arise?

In Germany, men who are called physicians, but who have been content to live in the dreamland of speculation rather than in the active work-a-day world of reality and of phenomena, have entertained and maintained the same opinion of the nature of insanity as Dr. Mayo; and they have not hesitated to adopt the legitimate consequences of that opinion. The school of Heinroth maintains that insanity is identical with spiritual sin, and that of Ideler teaches that it is mainly owing to moral depravity; a verbal difference appearing to exist between the two.[*] Nor have these opinions been confined to the closet. They have found expression in the lamentable harshness with which lunatics are still treated in Germany. The reckless application of punishment to the insane, both in prisons and in asylums, can find its only justification in such detestable theories.

It is easy to trace throughout the whole of these lectures the effect of this opinion in removing the insane from that pity and full sympathy which Dr. Mayo would doubtless feel for their unhappy state did he believe that, like other diseases, it was referrible to organic affection only. For example, writing of the "mischief occasioned by the doctrines of moral insanity" on account of the "impunity it affords to crime," he states, "No such impunity is implied in the hypothesis which avowedly represents *the abnormal state as a mode of wickedness consisting in the non-development or absence of the moral sense.*"

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[*] The principles of the different psychological sects which now divide the medical profession in Germany, is well and concisely expressed in the following extract from the May number of Damerow's 'Zeitschrift für Psychiatrie:' "Den *Ursprung* der Seelenstörungen betreffend, so ist auch hier noch immer keine Uebereinstimmung erzielt worden und namentlich stehen zwei feindliche Lager sich noch immer sehr schroff gegenüber; ich möchte sie nach ihren Grundausschauungen die *Somatiker* und die *Moralisten* nennen. Während die meisten Irrenärzte die Ueberzeugung von dem körperlichen Ursprunge der Seelenstörungen festhalten, hält ein viel kleinerer Theil noch immer an der Ansicht fest, die Grundursache deselben liege in geistigen und moralischen Einflüssen. Die Hauptträger dieses Ideenganges, *Heinroth*, und *Ideler*, weichen nur darin von einander ab, dass Jener, Ursprung und Wesen der Insania in allgemeiner *Sündhaftigkeit* und *Lasterhaftigkeit*; Dieser, in einem übermässigen Vorhandensein von *Leidenschaften* sucht. Bei den Somatikern gehen die Meinungen nur darin auseinander, dass man nicht darüber einig werden konnte, ob das Gehirn, als das eigentliche Denkorgan, immer und ausschliesslich der körperliche Sitz und Ursprung der Krankheit sei, oder ob diese auch direct durch ein körperliches Leiden aller andern Organe erzeugt werden könne."

When Dr. Mayo sees the lengths to which he is bound to follow his opinion that insanity is a disease of mind in the abstract, we trust he will reconsider the grounds of his belief, even if such a step should lead him to desert the speculators, and to enlist himself in the ranks of those who are content to reason alone, upon matters which are capable of being observed.

Dr. Mayo would punish for crimes committed during a supposed lucid interval. Quoting from Rae, that we ought never to convict for a crime committed during that time, because there would be every probability that the individual was under that cerebral irritation which makes a man insane, he says—

> "That this law is not conformable with the dicta of the judges will not be disputed if my interpretation of those dicta is accepted. That it is not more conformable with the interests of society will also, I think, be admitted."

But it would be a thankless and unprofitable task to pick out all the expressions which bear the stamp of Dr. Mayo's opinion of the radical nature of insanity. They are such as would justly be deemed harsh, if they proceeded from a person who saw in a lunatic an afflicted but innocent sufferer from disease of the brain; but emanating from one who thinks insanity "a disease of the abstract mind," who confounds "abnormal states" of the mind with "modes of wickedness," they must be accepted as the imperfect results of a theory which a benevolent disposition alone can prevent the reason from pursuing to its logical and cruel last consequences.

Has Dr. Mayo ever reflected from whence he derives his right to express an opinion respecting the nature of insanity, or to undertake its treatment? When, a few weeks ago, he stood up in a court of justice and pronounced with authority his belief that Luigi Buranelli was not insane, did he understand from whence he derived his mission to do so? If the opinion was common in this country that insanity is a disease of the abstract mind, neither to Dr. Mayo nor to any other physician would it be permitted to claim any right to pronounce with authority an opinion thereupon. Insanity would be taken from the domain of medicine and given over to the mental philosophers; or if in this practical common-sense country a sufficient number of such dreamers was not to be found, one step further back, would consign the lunatic to his old guardian, the priest.

If Dr. Mayo's opinions are not unsound, the pathologist and the physician have no more to do with insanity than with the origin of evil or the nature of the beautiful. Let those who have the care of the insane dismiss Andral and Rokitansky to the dusty oblivion of the upper shelves, and make Aristotle and Plato, Kant and Hegel, their instructors and guides. But the practice common in this country contradicts these views, and places any man who may entertain them in complete disaccord with the prevailing tendency of thought.

Respecting the marks by which the presence of insanity may be known, Dr. Mayo says:

> "In looking for a term which may contain the essential elements of insanity, and therefore confer a criterion of its presence, I adopt delirium, as used by M. Pinel and Dr. Cullen. . . . . Delirium is generally contemplated as involving two conditions in this sense—that one of them must be present in every delirious person, both being generally present. The first of these conditions is a negative one. It is the absence of that control over the rise and succession of thoughts which exist in the sane mind, and co-existent with this, a state of ill-associated and incoherent thought. . . . . The second phase of delirium consists of the presence of certain delusions or false perceptions, of which there are two principal forms—namely, those of the special senses, which are called objective, and those having no reference to objects of sense, but turning on perceptions of the understanding alone."

Why Dr. Mayo prefers to call this latter variety by the quaint term of "notional delirium," we cannot tell. The converse of that which is objective is subjective; while idea supplies the converse of notion. But it would be vain to expect precision in the use of terms from an author who, on the supposed authority of an interpretation clause of an Act of Parliament, draws a wide distinction between insanity and unsoundness of mind—terms which are precise synonymes, notwithstanding that lawyers have more affected the use of the Latin derivative in criminal cases, and that of the Saxon in civil procedure.

But is *delirium* an "essential element of insanity" and "a criterion of its presence"? If so, we have to unlearn all that has been added to the science of mental pathology, from the time of Pinel and Cullen to the present. If we give up the *Manie sans Délire* of Georget, the *Monomanie Homicide* of Esquirol, the Moral and Instinctive Insanity of Pritchard, what shall we say of the numerous cases of Melancholia in which there is neither incoherent thought nor delusion? What shall we say of those fearful cases of suicidal impulse, in which the intellectual faculties are intact, while the instinct of self-preservation has undergone the most complete perversion? As well might Dr. Mayo assert that delirium is the essential element, the true criterion of the presence of fever.

We have devoted more space to the examination of Dr. Mayo's work, than we should have felt ourselves justified in doing, had not its contents received a certain stamp of authority by passing to the public through the lecture hall of the Royal College of Physicians. We trust that the next time the London College of Physicians submits to be lectured to on the subject of insanity, it will be more fortunate in the selection of its instructor. One more antiquated in his notions, more unprecise in his use of terms, and more illogical in his argumentation, it would be difficult to find than the Croonian Lecturer of 1853. Moreover, whatever practical acquaintance Dr. Mayo may have had with the phenomena of insanity must have been lost upon him, because he has viewed them as symbols of disease of the mind in the abstract, too unimportant to attract particular attention; for of the essential elements and the true criteria of cerebro-mental disease, these lectures scarcely contain a notice.

Dr. Forbes Winslow's lecture "On Medico-legal Evidence in Cases of Insanity," delivered before the Medical Society in London, is a lecture indeed. It occupies seventy-seven pages of closely-printed royal octavo, and although the lucidity of its argument, the elegance of its language, and the absorbing interest of its subject matter, must have rendered it impossible for the audience to have become inattentive or weary, still we fear that the learned author may have something to answer for, by having afforded the occasion of overwrought mind among not a few of his professional brethren.

We shall commence our examination of it by a quotation in which the author sets forth his views of the ultimate nature of insanity. Opinions on subjects of this abstruse nature have a more direct bearing upon medical and legal practice than would at first sight appear probable. For instance, Lord Brougham ignores all forms of partial insanity on account of his metaphysical views respecting the nature of the mind, which he believes to be one, indivisible, and without parts. The mind has neither an upside nor a downside, an inside nor an outside, a right side nor a wrong side. It has no limits or boundaries, and is therefore diffused through space. It has no parts, even of an atomic nature. It is imponderable and immensurable, and is therefore totally unlike any gaseous body; which latter is indeed as truly material as muscle or cerebral substance. There are no real distinctions in the operations of this spiritual and essential unity. Thought is like feeling, feeling like instinct, and instinct like thought. They are different modes of the same essence; as in the modes of fire, light is like heat, and heat is like smoke, and smoke is like light. Therefore no one can be partly insane and partly sane. Monomania is a delusion founded upon the supposed but unreal distinctions of mental function. According to this noble and learned metaphysician, there is, and there can be, no such thing as partial insanity—a conclusion which has succeeded in convincing us, not that our senses have been unfaithful in recording upon our memory a great number of such cases, but of the extreme danger and absurdity of applying the "*à priori*" mode of argumentation to matters of science. The intellectual function, or mode of mind, submits itself to examination much more easily than the instinctive or the emotional. If, upon examination, the intellectual mode was found to be unaffected by disease, the entire mind must, upon this theory of mind, be pronounced sound. For it must be remembered that assent to the proposition, founded upon the Unity of Mind theory, "Prove one function of the mind unsound, and the whole must be unsound," carries with it, as a logical sequence, assent to the converse proposition, "Prove one function of the mind sound, and the whole must be sound,"—a proposition which, if acted upon, would conduct many a madman to the scaffold.

The views of Dr. Forbes Winslow upon the ultimate nature of insanity are most explicitly set forth in the following passage:

> "We observe the principle of vitality manifested through different physical media; but whatever may be the character of the material tissue, or the special function of the organic structure through which life reflects its powers, we, as *Spiritual Physiologists*, maintain that these manifestations are only different modes or states of development of one and the same principle; that the life that manifests itself through the brain, lungs, stomach, or the heart, is identical and homogeneous in its nature and essence: the peculiarity of the physical organization affecting, as it undoubtedly does, its mode of being or action. Applying this metaphysical doctrine to the subject now under consideration, it must be evident that *in all the varied phenomena of insanity the same identical essence or principle*

> *is affected; that without any exceptions, the Mind—using this term in its liberal and philosophical acceptation—is in a state of disorder.* I would, however, protect myself from repudiating the great discovery of Gall, or of holding with the spiritualists, that the principle of thought is susceptible of actual disease, apart from any abnormal state of the cerebral tissue. In all cases of mental derangement, the *manifestations* of the mind, and not the mind itself, are implicated; or, to speak with a strict regard to the principles of cerebral pathology, the physical media, or the different portions of nervous matter through which the intellect is developed, are diseased, and as a necessary consequence, the principle of thought is disordered or deranged in its operations." (p. 119.)

Thus it appears that Dr. Winslow also considers insanity to be a disease of the mind in the abstract, but he differs from Dr. Mayo in adding thereto an abnormal state of the brain as a *tertium quid.* He thinks that in insanity, without any exceptions, the essence or principle of mind is affected; but he does not deny that in combination with this affection of the essence of mind, there must be an abnormal state of the cerebral tissue. We do not clearly understand what he means by "the manifestations of mind, and not the mind itself, being implicated in all cases of mental derangement," unless he refers to the mind itself as an ontological entity distinct from its qualities. Granting, for the sake of argument, the existence of an entity of this nature, we must observe that it appears impossible to prove, and most difficult even to conceive, any change taking place in the qualities or manifestations thereof, without change in the thing itself. In the above paragraph, Dr. Winslow avows himself to be a *spiritual physiologist, and also a materialist of the phrenological school.* With the mental chemistry which unites these apparently incompatible elements, we are unacquainted. To our understanding, it appears a mixture rather than a combination. But as Dr. Winslow maintains that insanity cannot occur from an affection of the principle of thought, apart from abnormal states of the cerebral tissue, and in fact, as he states in several other passages, that insanity is always accompanied by disease of the brain, we gladly accept the latter half of the author's opinions on this matter as the practical basis of his judgment in cases of criminal lunacy.

We have seen that the spiritualist doctrine, comprising that of mind being "an essence one and indivisible," (p. 118,) is inconsistent with the theory of partial insanity; and if Dr. Winslow adhered to the metaphysical opinions he has enunciated on this point, he would find it difficult to discover a logical route to the various forms of partial insanity which he recognises, or to the degrees of insanity and responsibility, which, we are happy to say, he not only admits, but on account of which he eloquently pleads for a *system of graduated and mitigated punishments.*

> "As the plea of insanity is one of the most important that can be urged in a court of justice in extenuation of crime, it should never be had recourse to except in clear and obvious cases, in which little or no doubt can be entertained, not only of the existence of mental derangement, but of derangement of such a *kind*, and to such a *degree*, as to justify the immediate admission of the fact, and the necessary and consequent acquittal of the prisoner." (p. 109.)
>
> "We should never forget in many criminal cases the alliance to insanity is close—the line of demarcation between the two conditions indistinct, vague, and shadowy, the boundary separating crime from insanity obscure—the one state

often, almost imperceptibly, blending with the other, and that the facts associated with the criminal act so analogous to the recognised phenomena of mental disease." (p. 155.)

"I maintain, and facts—an overwhelming mass of facts—clearly, irresistibly, and conclusively demonstrate my position, that there is a vast amount of crime committed by persons who, if not 'legally,' or 'medically,' insane, occupy a kind of *neutral ground between positive derangement and mental sanity.* I do not broach this idea with the view of supporting the absurd, unphilosophical, and dangerous opinion, that *all* crime is more or less referable to aberration of mind; but I do affirm, that in estimating the amount of punishment to be awarded, it is the solemn duty of the judge, not only to look at the *act itself,* but to consider the *physical* condition of the culprit—his education, moral advantages, prior social position, his early training, the temptations to which he has been exposed, and, above all, whether he has not sprung from intemperate, insane, idiotic, and criminal parents." (p. 156.)

On referring to Dr. Winslow's earlier works, we find that this philosophic and truly humane opinion has been long and consistently maintained by him. His work on the 'Plea of Insanity' concludes with the following sentence:

"I again repeat, that I am not prepared to give an unqualified assent to the dogma, that in every case of mental derangement, without any reference to its degree or character, ought the person to be screened from the penalty awarded by the laws for criminal offences. I am ready to admit, that if insanity be clearly established to exist, a *primâ facie* case is made out in favour of the prisoner; but that because a person may be proved to be strange and wayward in his character, to fancy himself a beggar when he may have the wealth of a Crœsus, or to be ill when he is in the buoyancy of health—to believe that such a person ought of necessity to be exonerated from all responsibility, is a doctrine as unphilosophical and untenable, as it is opposed to the safety and well-being of society." (*Plea of Insanity*, p. 78.)

Dr. Winslow himself observes on this passage:

"Our opportunities of extended experience have been great since the publication of this opinion. The more we have seen of insanity, particularly among criminals, the stronger are our convictions that this is the sound, the safe, the philosophic view of derangement of mind complicated with crime. If the extreme view of the subject is recognised and acted upon, society would not be safe; if every shade of disturbed mind, if every amount of eccentricity, even when evidently associated with unhealthy cerebral organization, or even incipient insanity, is to shield persons against the just punishment awarded for offences against life and property, we might be justified in closing the doors of our criminal courts, and superannuating the judges who are entrusted with the administration of the laws! If we carried out this principle to its full extent, many great criminals would elude the hands of justice, and escape the just punishment awarded for crime." (*Psychological Journal*, vol. iii. p. 448.)

He elsewhere laments the want for such cases "of establishments between a prison and a mad-house."

In his remarks upon the trial of Robert Pate,* Dr. Winslow observes—

"We propose to make it compulsory for the jury always to assign the motives of a recommendation to mercy. If such a rule obtained, what more valid motive could a jury adduce than that of a doubt respecting the perfect sanity of the prisoner? Cases are continually occurring in which the evidence, though insufficient to warrant an acquittal on the ground of insanity, is conclusive as to the existence of such an amount of mental derangement as should serve to extenuate the prisoner's guilt and mitigate his punishment.

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* Psychological Journal, vol. iii. p. 456.

"When the unsoundness of mind is found to amount to what the law terms total insanity, we would retain the present form of verdict; but in those cases of doubtful character, in which proof of some previous attack of insanity, or habitual eccentricity, or recent change of sentiment and conduct, not amounting to evidence of positive lunacy, and having no immediate connexion with the crime, yet sufficient to suggest the suspicion of so much mental unsoundness as would tend to impair the will, obscure the judgment, and preclude a full, clear, and just apprehension of the criminality of an offence; in such cases we would authorize the jury to return a verdict of guilty, with a recommendation to mercy on the ground of presumable insanity." (p. 123.)

In an article on the Plea of Insanity in the same journal (No. 17), he urges the same opinion, and suggests a remedy.

"In every criminal case where the question of responsibility arises in the course of judicial inquiry, if it be possible to establish any degree of positive insanity, it should always be received as a valid plea for a considerable *mitigation of punishment*, and as *primâ facie* evidence in favour of the prisoner; and in no case where insanity clearly exists (without regard to its nature and amount) ought the extreme penalty of the law to be inflicted.

"In its present state, the law permits no discrimination; for so far as punishment is concerned, it takes no account of the degree of insanity, and recognises no intermediate condition between perfect sanity and total insanity; but the change we have proposed would satisfy the necessities of the case, and render the law more consonant with the enlarged humanity and progressive enlightenment of the age."

We have been thus careful to place the opinions of this able psychological writer on this question fully before our readers, because it appears to us at the present time to be a most important one, urgently demanding a practical solution. We have ourselves on several occasions maintained, to the best of our ability, and with the earnestness of strong conviction, opinions exactly tallying with those so forcibly ennunciated in the passages we have quoted from Dr. Winslow. They have not been allowed to pass unchallenged; but we venture to affirm that they are incapable of refutation, unless the origin of insanity in disease of the brain can be proved to be an error. If insanity is an affection of the mind, as distinguished from the brain, and if the mind is an essence one and indivisible, we give up as untenable the doctrine of its partial disease. That which has no parts cannot be diseased in part. But if the cause of insanity is to be sought in bodily changes, we know, as physicians, that we have firm and trustworthy ground under foot, and with the force of experience derived from a vast mass of recorded instances of the special disease in question, and also from general principles of pathology, we know that the degrees and varieties of this disorder are and must be infinite. If this be true, degrees of insanity exist, for which exemption from punishment cannot be claimed, unless it be maintained that the very lowest amount of insanity ought to confer immunity from all responsibility, a proposition perfectly untenable. It is, however, such a proposition that the opponents of modified responsibility are compelled tacitly to adopt. They cry, Punish a madman! what monstrous cruelty! forgetting that modified punishment may be, and often is, an act of the greatest kindness and mercy to the individual punished. It is not, and it never has been, the law of this country, that all the pale and faint shades of mental disorder should shield the doer of a crime from the consequences of his act. From the times of Coke and Hale to the present day, judges have busied themselves in defining the kinds and degrees of mental disturbance which should be deemed sufficient to excuse the commission of crime. But the opponents of mitigated punishment ignore this fact, and argue upon the assumption, that any trifling amount of deviation from the type of healthy mind can reasonably and legally be urged as a complete bar to punishment. The fractious and peevish subject of gout,* in whose blood Dr. Garrod had found some lithic acid, would possess, according to these reasoners, an equal immunity for acts of violence with the man who was urged to acts of delirious fury by a more potent blood-poison—that of typhus, for example, perverting all the cerebral functions. It is easy to see the logical fallacy into which these writers have fallen. The immunities justly accorded to mental disorders of a certain kind and degree, they claim for all disorders of the mind whatsoever. It is the everlastingly recurring fallacy which is effected by changing the premises, *à dicto secundum quid, ad dictum simpliciter.* We have been astonished at the vehemence and pertinacity with which this flimsy argument has recently been urged. But for this and for the continuance of practices consistent with this false theory, but utterly opposed to the fundamental principles of justice, we should have scarcely deemed its refutation worthy of the ink we have shed upon it. If the necessity and justice of mitigated punishments founded upon the theory of responsibility being modified, but not entirely removed, by low degrees of mental disorder, had been recognised in the practice of our courts of law, we entertain the strongest conviction that one of the most appalling crimes on record—that of the adulteress-murderess, Mary Ann Brough—would not have been declared free from guilt; nor would the poor Italian, Buranelli (the evidence of whose insanity was infinitely stronger than that which existed in Mrs. Brough's case), have been made to expiate his deed of frantic revenge upon the scaffold. In these cases of diminished responsibility, the pendulum of judicial decision oscillates between the mischievous example of crime declared to be free from guilt, and the piteous spectacle of a wretched being subjected to the extreme penalty of the law; in spite of a reasonable belief that he was the subject of mental disease, however slight in degree, or unconnected with the act for which he suffers.

Dr. Winslow has not only pointed out with great clearness the true doctrine of modified responsibility and mitigated punishment in cases of partial insanity—he has suggested the reforms most obviously needful to carry these sound and just opinions into effect. He especially recommends that the jury should have the power to return a verdict in three forms—namely, the two to which they are now restricted—*guilty,* or *not guilty, on the ground of insanity,* and also an intermediate one, *guilty, but recommended to mercy on the ground of presumable insanity;* according to which three forms, he proposes that the judge should be compelled to regulate his sentence; thus assimilating the rule of procedure in England to that which obtains in France, in the use of the verdict of Guilty, with extenuating circumstances. Establishments "intermediate between a prison and a mad-house," the want of which Dr. Winslow so much laments, and which, we apprehend, would somewhat resemble the Asylum for Lunatics of Criminal Disposition which we have ourselves urgently recommended, would enable the judge to award a modified sentence in the manner most beneficial to the criminal.

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* See Psychological Inquiries, p. 30.

Should any other changes in legal procedure be found needful to carry into successful practice the great principle of punishments modified according to degrees of responsibility, we are assured that Dr. Winslow will urge their adoption, notwithstanding the opposition which such changes would meet with from those, who reverence the laws of England for their antiquity rather than on account of their justice and excellence. He is not one of those men who adhere with blind tenacity to that which is false and defective, merely because it has received the stamp of authority. The *liard*, with its coating of silver and its imperial N., is for him nothing more than a bit of brass; and he is the last man to appeal to the great charter in defence of the defective or bad customs of our courts, and to cry with indiscriminating zeal, "Nolumus leges Angliæ mutari." On the contrary, he thinks that the "first principles of law" have yet to be decided upon by our judges, and that, consequently, matters of procedure and custom founded upon such principles are unfixed, fluctuating, and reformable:

> "Before this can be recognised as a safe standard, it will be necessary for British jurists to lay down for their *own* guidance certain fixed and unalterable principles of jurisprudence. Is it not a notorious fact that, on apparently clear and well-recognised points, lawyers of eminence have arrived at the most opposite conclusions? One court reverses the judgment of an inferior tribunal, and one distinguished jurist overrules the decision of his predecessor. As long as able judges differ among themselves upon what may be termed *first principles of law*, it will be unreasonable to expect that *we* should prostrate ourselves before the legal test which I have been analyzing." (p. 108.)

We are inclined to differ from Dr. Winslow in thinking that difficulties of this question have arisen, not so much from any defect or uncertainty in the first principles of law, as from the clumsy and imperfect method of admitting medical evidence and conducting the formalities, which legal custom has sanctioned for the elucidation of truth. The manner in which scientific evidence is hunted up and marshalled by attorneys in criminal lunacy trials, is indeed lamentable. It is degrading to physicians holding a high position in the science of cerebral pathology to be calculated upon and made use of by lawyers, as trump cards in the trial of forensic skill on one side or on the other. Dr. A. thinks all men mad, therefore he is always subpœnaed for the defence. Dr. B., on the contrary, thinks all men are wicked whose habits of thought and conduct are strange: he, therefore, is certain to appear as evidence on the side of the Crown. Alas! for the honour and dignity of the profession, when physicians of high standing and of spotless integrity can in this manner be used by any astute or unscrupulous attorney to get the chesnut out of the fire. Let it not be forgotten, that if the medical witness, in consequence of the evidence adduced, changes the opinion he was supposed to entertain before the trial, he is not called into the witness-box—he is passed over, *sub silentio*, as an unprofitable servant; and this is done not only in the defence, but on the part of the prosecution. It is scarcely defensible in the former case, but in the latter it becomes an act of atrocious injustice, equivalent to the concealment of evidence which would prove the innocence of a man arraigned for a great crime. What would be thought of a prosecutor who, having brought a witness into court with the purpose of giving testimony that he had seen a murder committed by a person who was on trial for the deed, but who discovered that he had mistaken the identity of the prisoner whom he saw at the bar—what would be thought if, under such circumstances, the prosecutor withdrew that witness from the court, and withheld the testimony he was able to afford respecting the innocence of the prisoner?

The whole proceedings in our criminal courts for ascertaining the existence or non-existence of insanity are awkward, unscientific, and uncertain. They are such that it becomes in a great degree a matter of accident whether a guilty person escapes under the false plea of lunacy, or a lunatic is found guilty and hanged. They form a most painful and humiliating contrast to the scientific proceedings by which such questions are determined in France. We recommend such of our readers as may be desirous to see what medical evidence in cases of the plea of insanity ought to be, to turn to the pages of the 'Annales Médico-Psychologiques,' which excellent periodical contains numerous reports of the *experts* employed by the French judicature. These *experts* examine the supposed lunatics in the most painstaking manner, conversing with them day after day, observing them at all hours, confronting them with their friends or old associates, and even giving them chloroform to throw them off their guard. They make out the history of the whole life of the patient, and subject it to a strict analysis. Upon these data they make their report, being under oath; and we must say of these reports, that they are remarkable for sound reasoning, extensive psychopathic knowledge, and integrity of purpose. We greatly regret that their length prevents us from giving one of them as an example. It is an exceedingly rare occurrence for the court to differ from the conclusions arrived at by the *expert*. Compare this system with that which prevails in our courts, where an array of medical men are marshalled by the attorneys on each side, according to their preconceived opinions of the case. These medical witnesses may usually be divided into two classes—those who know something of the prisoner and nothing about insanity, and those who know something about insanity and nothing of the prisoner. They generally succeed in neutralizing each other's evidence, and in bringing the medical profession into contempt, at least among lawyers. One of the most experienced of medical witnesses (Professor Taylor) admits that the fate of the prisoner is, for the most part, a matter of chance—a proof that it is seldom determined by scientific evidence, for science and chance have no relation to each other. The French system, which is not an absurd plan of medical juries, but which places the scientific *expert* before the court in an independent and impartial position, and affords to him an ample opportunity to form a decided and trustworthy opinion, appears to be in every way worthy of imitation.

The medico-legal notes of Dr. Forbes Winslow on the case of Buranelli, afford ample proof of the inveterate tendency existing among psychologists to see only one side of these difficult questions. He states:

> "If a *primâ facie* case of mental derangement be established *in favour* of an accused person, the testimony of a scientific *expert*, although necessarily speculative, is legitimate and admissible. *His object is to save human life*, by affording the prisoner the benefit of any doubt that may have been raised as to his sanity and responsibility when the overt act of crime was committed. The witness may, with the best intentions, come to a *rash and unjustifiable conclusion*, and if such should be the case, *no serious injury* to society ensues if, as the result of his evidence, a fellow creature is rescued from the hands of the public executioner. On the other hand, if, in a criminal case, a medical witness incautiously or inadvertently gives a wrong opinion, a monstrous act of injustice and cruelty may be perpetrated, for which there can be no remedy. A scientific witness has no right, if called upon, to give such evidence, from the conviction that he cannot do so without recklessly trifling with human life. When we consider how suddenly symptoms of homicidal insanity develop themselves, how transient and evanescent these attacks are, that *a man may be wildly delirious and irresponsible in the morning, and sane, rational, and responsible in the afternoon*, how can a medical witness speak with satisfaction on the subject? If we were asked if Rush and the Mannings were of perfectly sane mind when they committed the brutal murders for which they justly suffered the extreme penalty of the law, we should certainly decline committing ourselves to an opinion, if the lives of these miserable criminals rested upon the answer we gave to the interrogatory." (p. 64.)

These opinions are highly creditable to the benevolence of Dr. Winslow's disposition, but we fear that they will not be received with much favour by the administrators of justice. Every member of society is liable to be called upon, and to be compelled, to aid in the administration of the laws of society, however painful that duty may be to his sensibilities; and we apprehend that a medical witness, whose regard for the life of criminals led him systematically to adopt and candidly to avow the line of conduct advocated by Dr. Winslow, would expose himself to the stern rebuke of the court, and the rude remarks of the gentlemen of the bar, if not to consequences still more unpleasant. If such reluctance to aid in the punishment of criminals as that avowed by Dr. Winslow should become general, how would the duties of the jury-box be discharged?

With regard to the *nature of insanity*, we must express our own decided conviction that it depends solely and entirely upon disease of the brain. Insanity we have ourselves defined to be "*a condition of the mind in which a false action of the conception or judgment, a defective power of the will, or an uncontrollable violence of the emotions and instincts, have separately or conjointly been produced by disease.*" This definition an able writer declares to be "superior for juridical purposes, as even a layman may pronounce, to those that have preceded it."* But supposing this to be the case, and the definition admitted as on the whole correct and satisfactory, how is any concrete instance to be recognised as falling within it? How are the conditions of mind which it indicates to be known as the product of disease, and therefore as insane conditions? In the classification of men into those who are sane and those who are insane, the only trustworthy mark is furnished by the absence or presence of that which is the *cause* of the difference — namely, *disease*. Causes are always preferable to any other mark in scientific classification, "both as being the surest and most direct of marks, and as being themselves the properties on which it is of most use that our attention should be strongly fixed."* It is the more needful in this case to take the cause as the mark of the class, inasmuch as no one of the effects is in the slightest degree trustworthy as a mark of the whole class. Disease, therefore, of the brain must be accepted as the sole mark whereby insanity can be predicated; and the demonstration of its existence is the rightful, though oftentimes difficult, task of the physician.

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* Crime and its Excuses: Oxford Essays. 1855.

In the accomplishment of this task one of two methods may be adopted. The phenomena of all forms and varieties of insanity, which have been ascertained to be such by accurate and faithful observation of their results, and by post-mortem examinations, may be recorded, and the varieties classified thereupon; so that when a new instance arises it may be referred by its phenomena to its proper place, in the same manner that a case of skin disease may be classified, even by a tyro, by reference to the descriptions and the atlases of Wilson or Rayer. This method, which is strictly experimental, has been recently adopted by the experienced Belgian physician whose work we have cited at the head of this article. Its application, however, to juridical purposes would appear to be difficult, if not impossible, for the phenomena recorded would never be admitted as a standard of comparison; and even were the admission made, the symptoms of insanity are so infinite in degree and character, that it would be far more perplexing to find, for a particular instance, its right place in this complicated classification, than to prove, deductively, its right to be so classified. Still, such systems have their use. They lead to the diligent and continuous observance of phenomena, without which, argumentation upon the basis of definitions is continually tending to become a war about words — a mere logomachy. "Words, however well-constructed originally, are always tending, like coins, to have their inscription worn off by passing from hand to hand; and the only possible mode of reviving it is to be ever stamping it afresh, by living in the habitual contemplation of the phenomena themselves, and not resting on our familiarity with the words which express them." †

A minute observance, and a painstaking classification of the phenomena of insanity, is therefore justly to be deemed a necessary foundation for the more difficult intellectual task of deciding whether the symptoms presented by a new case, of dubious nature, entitle it to be ranked in the category of insanity or not. This must be effected upon the broad principles of general pathology, and not by the aid of the minute distinctions of systematic nosology. This latter system—which may be called the deductive, in contradistinction to the experimental—is the one we have ourselves endeavoured to elucidate in the work quoted further on, and the principles of which may be said to depend upon a careful and philosophic *comparison of the individual with his former self*; and upon the relation of cause and effect in the influence of circumstances affecting the condition of the mind. Thus, if a circumstance which usually produces sorrow or chagrin is followed by extravagant spirits, and if such excitement, bidding defiance to advice and reason, gives way under the administration of pharmaceutical remedies and the influence of judicious control, there can be little or no doubt that the excitement has been caused by cerebral disease.

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* Stuart Mill.

† Ibid.

The etiology of disease must be allowed considerable weight in deciding upon difficult instances. Changes in the habits, or dispositions, or intellectual powers coming on after an injury to the head, or a fever, or intense mental excitement, or grief, will be more readily acknowledged as symptoms of mental disease than if they had been observed without the occurrence of such well known causes. The change which took place in the habits and character of Buranelli derived importance from the fact that they occurred shortly after, and apparently in consequence of, the death of a tenderly beloved wife.

The existence of any signs of disease of those portions of the brain which are not subservient to the mental functions, are of immense diagnostic value. It has occurred to us on more than one occasion to detect the early symptoms of general paralysis in persons accused of theft. The existence of epilepsy would, in our judgment, go far to prove that acts of violence were the result of cerebral disease. It is also well known that various forms of insanity often come on after attacks of cerebral hæmorrhage. But we must admit that it is quite necessary to be cautious in the admission of a past attack of cerebral disorder as a proof of insanity. Great stress was laid upon the puerperal convulsions from which Mary Anne Brough had suffered some years before the commission of her crimes. They had not left behind the slightest indication that the brain was permanently injured, and it would have been difficult to prove that they had a more intimate connexion with her crimes than might have been exerted by an attack of teething convulsions during the more remote period of infancy. To admit that proof of any previous affection of the sensory motor system afforded sufficient ground for inferring the existence of mental disease, would be extremely unphilosophical and unsafe.

The different forms of insanity dependent upon the particular function of the mind affected, are of importance to the medico-legal question, principally because lawyers have been in the habit of looking to one set of functions only as the subject of mental disease. We have on a former occasion fully expressed our opinions on the important question of moral insanity in the pages of this Journal (Nos. 24 and 25); and notwithstanding the metaphysical criteria of insanity which still maintain a place in the opinions of the least informed or most prejudiced of our legal authorities, we do not believe that proof of intellectual aberration will be deemed necessary to establish the existence of insanity in cases where an abnormal change in the affections and propensities can be demonstrated to have been the result of cerebral disease.

We cannot, however, omit all reference to a subject so important as that of delusion, a symptom which is still accepted in our courts as the most authentic mark of insanity, as the essence of cerebro-mental disease, if not as the disease itself. A clear appreciation of the nature of insane delusion is essential to the medical witness, since it is a topic on which barristers delight, by sophistical reasoning, to represent the opinions of medical men in an absurd and extravagant point of view, and in that manner to damage the value of their evidence.

What then, is, and what is not, a delusion? It cannot be doubted that this term has been long undergoing a transformation in its meaning. This new sense both medical men and lawyers would do well to accept with a good grace, and to agree to use the term only in its scientific medico-legal signification. "Logicians cannot make the meaning of any but *scientific* terms; that of all other words is made by the collective human race." The word delusion has become a scientific term, and it is highly necessary for those who have to make use of it to agree upon what it shall be held to connote. No better example can be given of this necessity than the different, and indeed contrary, meanings which great legal authorities have given to the term. Thus, Sir John Nicholl gave it as his opinion that "a delusion is a belief in facts which no rational reason would have believed." If rational reason means the reason of a sound mind, this definition is an authoritative restriction of the term to its pathological sense.

But Lord Brougham defines a delusion as "a belief of things as realities which exist only in the imagination of the patient;" and thus completely reverses the meaning of the term as fixed by Sir J. Nicholl.

The objections to Lord Brougham's definition of delusion are that it includes the erroneous opinions entertained by some people, such as mesmeric prophecy, clairvoyance, and spirit rapping, and that it also excludes many of the actual delusions of the insane which are founded upon false perceptions of the senses. Such false perceptions, arising as they do from actual changes either in the nerves of sense or in the *sensorium commune*, cannot be said to exist only in the *imagination* of the patient. The phantasmagoria of Nicolai were owing to material changes somewhere in the optic tract; had he believed in their reality he would undoubtedly have been the subject of delusion, but of delusion independent of the imagination.

The nature of delusion has hitherto been greatly misunderstood, and has been the source of abundant mystification in medico-legal trials. A clear recognition of its nature would be of immense importance, and the hope of arriving somewhat nearer thereto will, we trust, justify the following summary of our own opinions on the subject:

"*A delusion is a belief in the existence of things which have no existence in reality, or an erroneous perception of the nature of things, or of their relation to each other, occasioned by cerebro-mental disease.* It may be said that this also is begging the question, and leaves the difficulty as great as before. This is to some extent true, but unavoidable. For it must be admitted that there is no certain method of distinguishing between the erroneous intellectual operations of a diseased mind, and those of the sane but imperfect reasoners who abound in society. Insanity is a disease recognisable with sufficient certainty by many symptoms when grouped together, not one of which, however, can safely be trusted to by itself, or considered pathognomonic. Most of the symptoms are occasionally to be observed singly where insanity does not exist. In illustration from another disease: pain in the breast is a symptom of pneumonia, which, although it does not always occur in that affection, and does occur in some others, is not without value when taken in connexion with other symptoms and signs. In the same manner, an absurd opinion, which taken by itself would possess little value as an indication of insanity, when considered in a group with other symptoms may become of great diagnostic value. Thus, the belief of the unfortunate parricide, Mr. Dadd, in Osiris and the religion of ancient Egypt, was far more dignified, and scarcely more absurd, than the religion of the Mormons, the Lampeter Brethren, the followers of Johanna Southcott or Swedenborg. It could not, therefore, on its own merits or demerits, be pronounced to be an insane delusion; but when taken in connexion with sudden change of habits and disposition, loss of rest, and other indications of nervous disturbance, and followed by the homicide of a beloved parent, that strange opinion is at once recognised as the fantasy of a diseased brain.

"Not unfrequently, but by no means constantly, the delusions of the insane possess characteristic features, by which they may be distinguished from the absurd opinions of the foolish and ignorant. The following are some of the most prominent:

"1. The delusions of the insane are generally independent of the opinions of others; they isolate the person who entertains them from his kind, whereas the sane portion of mankind are gregarious in their absurdities; fools are to be considered sane, follow each other through a gap like a flock of sheep, oftentimes indeed, following some bell-wether who is more rogue than fool: they have neither the confidence nor the courage to walk alone. Mr. Dadd was probably the only person in England who believed in Osiris; had there been a few hundreds, or even a few scores of persons entertaining the same belief, his ideas on this subject would have been of infinitely less value as a symptom of insanity.

"2. The faith of the insane in the truth of their delusive opinions is stedfast and unflinching. It almost surpasses the religious faith of the Mohammedan or Hindoo, and renders pale by contrast the attenuated belief which sane men accord to the absurdities of the hour.

"The *dilettanti* philosopher, or religionist, concedes to clairvoyance, to rapping spirits, or to Mr. Prince, a certain amount of belief, which may give way under the assault of ridicule, or logic, or misfortune; but the lunatic believes in his delusion with all his soul; he may outlive it, or be cured of it, but he can never be driven from it by any influences, however potent; 'no one who has not been insane,' says a convalescent patient, 'can imagine how terribly real the delusions of lunatics are.'

"3. The delusions of the insane come on after some physical or moral shock, and often present strange contrasts to the previous habits of thought, or have no relation thereto. The absurdities of the foolish or the ignorant have no such starting point, and are generally consistent with their customary attempts at thinking.

"4. The delusions of the insane, in many instances, have relation to the patients alone, and are often of a kind which renders their nature apparent. The idea of loss of personal identity, in an infinite variety of ways, is a frequent source of delusion, and indicates so complete an overthrow of the normal action of the mind, that it must generally be considered the result of disease. Even where such delusion is to a certain extent endemic, and therefore loses its isolated character, as in the *loup garous* of France, it is found to be caused by disease. In doubtful and difficult cases the psycopathist can only form his opinion by a careful estimate of all concomitant circumstances. The true test of delusion as a symptom of insanity is its origin or mode of production. Its existence corroborates the testimony of various physical and rational symptoms of cerebro-mental disease, and its own nature is discovered by their existence: they mutually prove each other."*

The little work from which we have quoted is one among others which has been produced by the encouragement given to the study of these and similar questions, by a nobleman whose labours in advancing the scientific knowledge of insanity and improving the condition of the insane have been so great and so successful, that we shall be readily excused for concluding this article by a brief reference to them.

It is not common to find among lawyers of high rank a just appreciation of the medical profession, especially of that section of it which makes insanity its peculiar study. But many acts of enlightened liberality, directed to the advancement of psycho-pathological science, prove that the ablest and most profound lawyer of the age has never participated in those narrow prejudices which have too often found expression in sarcasm and rebuke, directed from the bench against medical witnesses, whose motives were at least honourable, if their opinions have not unfrequently been more unprecise and speculative than the purely practical and perhaps somewhat rude intellect of our judges could tolerate. It may be fairly presumed that Lord St. Leonards was by no means satisfied with the amount of knowledge respecting insanity which he found among medical men, but, instead of indulging in sarcasm and rebuke, he set himself manfully to work to remedy the defect. He not only established the Sugden prizes to be awarded by the Irish College of Physicians to the best essays on insanity subjects, but he urged upon Government the appointment of medical men to the superintendence of the public asylums in Ireland, and used the most strenuous efforts to establish a regular system of clinical instruction in those asylums. A complete account of his labours in the cause of the insane in Ireland would for ever associate his name with those of the greatest benefactors of this most pitiable section of the human race; with that of Pinel in France, and of Conolly in this country. When he first went to Ireland, he found both the law and the practice of lunacy in a deplorable condition. There was actually no law whatever for the regulation of private asylums, and the natural and inevitable consequence was, that the unhappy inmates were treated with great barbarity and neglect. Mechanical restraint of the worst and heaviest kind was constantly and habitually employed. The dietary was bad, the clothing worse; the attendance, after the old recipe of brute force, combined with stolid neglect.

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* Unsoundness of Mind in relation to Criminal Acts, by J. C. Bucknill, M.D., &c., 1854. (Sugden Prize Essay).

To remedy this sad state of affairs, Sir Edward Sugden introduced a bill framed upon the model of the English statute, but with additions and improvements suggested by his own large experience. This bill became law, in spite of the strenuous opposition of the owners of asylums, who wrote pamphlets against it, and used every effort to obtain its rejection. At first this law was carried into effect by the inspectors of prisons, but ultimately separate inspectors of asylums were appointed, and the constant and enlightened supervision which they have exercised has been productive of the utmost benefit in ameliorating the condition of the insane in Ireland. But Sir E. Sugden took a more active and personal part than that of a legislator in effecting these reforms. He frequently visited the asylums within reach of Dublin, examined the patients, and inspected the wards, the bedding, the food, the rules, the books, &c. As it was never known when these visits of the Chancellor were likely to occur, the keepers of all asylums within his reach were kept on the alert, and a manifest improvement immediately took place. He interposed actively, and successfully, to introduce the non-restraint system of treatment, and some of the most painful instances of barbarous restraint were discovered and remedied by his personal intercession. When it was found, that the owners of the asylums in which the patients were in a state of wretchedness, could not be compelled by the law to remedy the consequences of their neglect, with a speed sufficient to satisfy his ardent and earnest philanthropy, he himself supplied food, clothing and skilful attendance, to the pitiable objects of his solicitude, and paid for them out of his own purse.

When Sir Edward instituted inquiries respecting the lunatics under the charge of the Court over which he presided as Lord Chancellor of Ireland, he found that no one knew who they were, or where they were, or in what condition they were. By advertisements, orders, and other means, he at length was enabled to make a correct list of them: he then compelled the committee of every lunatic to make a report to him on a fixed day every year, accompanied by a certificate from a medical man not connected with any asylum in which the lunatic was placed, as to his mental and bodily condition. By these means he in a short time obtained an accurate account of all lunatics under the charge of the Court of Chancery, with full particulars respecting their property and their personal condition. Thus it happened that when an application was made to the Court respecting any lunatic, the Chancellor had only to refer to his book to possess himself of more ample and accurate information than either the counsel or the solicitor could usually obtain. In this manner he was able to correct many abuses arising from short allowances, improper treatment of the person, and unfair dealings with the property. As an example of the latter, we may refer to an instance in which the next brother had taken possession of considerable landed estates belonging to the lunatic, had built a house upon them, and had in his marriage deed settled them as the owner, whilst from year to year he reduced his brother's scanty allowance, and kept him at an inferior farm-house. When Sir E. Sugden left Ireland, all these abuses had been corrected; and the information he had collected and systematized, and the rules he had laid down, were such as effectually to prevent their recurrence.

When, as Lord St. Leonards, he became Chancellor of England, this great and wise man undertook the complete revision of the statutes relating to lunacy in this country, and it is well known that he bestowed great care, attention, and labour upon this work. The result has been the production of an excellent and comprehensive code of lunacy laws. In matters having such wide relations, it is not surprising that defects should still exist; but it may safely be affirmed that, viewed as a whole, the lunacy laws of England are not only more perfect than those of any other country, but that they are also greatly superior to the statute laws of this country on almost all other subjects. The great evils which still exist are attributable not so much to defects in the law, as to the inadequate ability of those to whom its administration is entrusted. These statutes were framed and carried by Lord St. Leonards, who had learned by personal observation, to what grievances and abuses the insane of different classes of society were most liable to be subjected, and in what direction therefore the power of the law was most needed for their protection—who had shown himself to be influenced by the warmest feelings of humanity and compassion towards the objects of this legislation—and whose unrivalled powers of legal insight enabled him to construct those wise and comprehensive statutes for which all persons interested in the welfare of the insane, owe to this nobleman a debt of deep and lasting gratitude.

John Charles Bucknill.

REVIEW VIII.

1. *The Principles and Practice of Ophthalmic Medicine and Surgery.* By T. WHARTON JONES, F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, London; Ophthalmic Surgeon to the Hospital, &c. Second Edition.—*London*, 1855. 12mo, pp. 549.
2. *A Guide to the Practical Study of Diseases of the Eye; with an Outline of their Medical and Operative Treatment.* By JAMES DIXON, Surgeon to the Royal London Ophthalmic Hospital, Moorfields; formerly Assistant-Surgeon to St. Thomas's Hospital.—*London*, 1855. Post 8vo, pp. 395.

THE appearance of a second edition of Mr. Wharton Jones's manual confirms, in the way most satisfactory to all parties, the favourable opinion given of the work in the 'British and Foreign Medical Review' for January, 1847. The result of much research, the manual is written with great care; while the materials, drawn necessarily from many sources, have been so thoroughly moulded in the author's own mind, that they are presented to the reader with much of the freshness of an original work.

Mr. Dixon's 'Guide' is neither so elaborate nor so systematic a work as that of Mr. Wharton Jones; but, as the title denotes, is intended chiefly to direct the attention of the student to the changes which the tissues of the eye present under various morbid conditions. In pursuing this object, the author has interspersed many original statements, evidently derived from his own experience, and worthy, therefore, of particular attention.

Both Mr. Wharton Jones and Mr. Dixon commence with an account of the methods of examining the eye, in order to a diagnosis, the only sure basis of successful treatment. The signs of eye-diseases, both *objective* or *anatomical*, and *subjective* or *physiological*, are described with some minuteness; and under the former head we find, in both authors, an interesting account of the methods of exploring the interior of the eye.

> "The light," says Mr. Wharton Jones, "concentrated by means of a convex lens, about two inches in diameter, and three or four inches focus, is sufficient for all practical purposes, in the case of the anterior segment of the eyeball, comprising the cornea, aqueous humour, iris, pupil, and crystalline body." (p. 14.)

He then proceeds to give a succinct account, illustrated by diagrams, of the different forms of the new instrument, called an *ophthalmoscope*, for throwing light by reflection on the bottom of the eye. In a coloured plate he also shows the appearance presented by the retina in a congested state, with its transparency somewhat impaired, as seen by the aid of the ophthalmoscope, in a case of incomplete amaurosis. The following extract contains his estimate of the utility and application of the instrument:

> "For all practical purposes, opacity of the lens can be sufficiently well ascertained by ordinary exploration with the pupil dilated. By means of the ophthalmoscope, adjusted for the purpose, however, otherwise undistinguishable opaque points can be seen. But it is of no advantage to push the diagnosis of opacity of the lens to such a minute degree of accuracy, seeing that we often meet with

cataractous opacity, quite evident to ordinary examination, notwithstanding which there is still pretty good sight; whilst, on the other hand, cases occur in which the impairment of sight complained of is not at all to be accounted for by the appreciable opacity of the lens.

"Whilst the morbid states of the anterior segment of the eyeball are sufficiently accessible to objective exploration, those of the posterior segment, comprising the vitreous body, retina, and choroid, could formerly, with some exceptions—(e.g., exuded matter in the vitreous body, scrofulous and encephaloid growths at the bottom of the eye, &c., which give rise to a yellow, shining appearance, sometimes traversed by bloodvessels)—be determined only from the attendant subjective phenomena. This defect in our means of diagnosis of the state of the posterior segment of the eyeball was, however, practically little felt. Having determined that the disease was not seated in the anterior segment, and thus, *per exclusionem*, and from the nature of the subjective symptoms (together with the objective symptoms presented by the anterior segment, and by the eye considered as a whole), referred it to some part of the posterior segment, we were in a position to conduct our treatment of the case, not with less efficacy at least than can be done now, when it is possible, in many instances, to discover, by means of the ophthalmoscope, opaque spots, shreds, &c., in the vitreous humour, and congestion, with extravasations, exudations, and pigment deposits in or behind the retina.

"Morbid states discoverable in the vitreous body, are opacities in the form of fixed spots, or of undulating membranous shreds and filaments, of the most different size and form, with sharp or indistinct outline.

"There is nothing more easy than to see the vessels of the retina in a cat's eye without an ophthalmoscope. Having previously dilated the pupil by atropia solution, drop some water into the eye while the eyelids are held apart, and cover the cornea with a thin plate of glass. The vessels of the retina can then be seen, slightly magnified. It has been proposed to explore the bottom of the human eye in a similar manner, and instruments have been contrived for the purpose, but the ophthalmoscope . . . . . is of more ready and convenient application.

"In man, the red colour which the bottom of the eye presents varies in tint; being brighter in fair, more of a yellowish-brown in dark, individuals. The retinal vessels are seen branching on the uniform red field formed by the more vascular choroid. At the entrance of the optic nerve, which appears whitish-yellow and well defined, the retinal vessels are seen emerging. The retina in the situation of the yellow spot is little or not at all vascular, and sometimes presents a greenish-grey aspect. A streak of pigment deposit may be seen at some part or all round the border of the optic papilla.

"The principal morbid appearances in the retina which have been observed, are congestions, spots of extravasated blood, pigment deposits, opacities of various aspect, the retina itself bulged forward by fluid accumulated between it and the choroid, and tremulous in the dissolved vitreous body." (Jones, pp. 28—30.)

In Mr. Dixon's 'Guide' the use of the ophthalmoscope is referred to on many occasions. The reader is probably aware that the form of instrument now generally employed is that of a single concave mirror, of four inches focus, perforated in the centre, and fixed in a handle; the mirror being used either alone, or with the addition of a convex or concave lens, according to the kind of illumination required, or the visual focus of the observer. This sort of ophthalmoscope goes by the name of 'Anagnostakis', after a young Greek physician. "I am informed, however," says Mr. Dixon, "that an instrument precisely similar to that introduced by Anagnostakis as his own invention, had been used by Graefe, of Berlin, in 1853, and shown by him to Anagnostakis during the same year."

The following extracts contain some directions and cautions respecting the use of the ophthalmoscope, so clear and important, that we cannot refrain from presenting them to our readers:

"The examination is made in a darkened room in the following manner:

"The patient sits sideways at the edge of a table on which a lamp is placed, the flame being close to, and on a level with, the suspected eye, but far enough back to prevent any light falling directly on the cornea: or he may sit with his back to the table, the lamp being close behind him, so that the rays of light just clear the top of his head, and fall perpendicularly, instead of obliquely, on the surface of the mirror.

"A chimney faintly tinged with blue, by decomposing the red rays, whitens the light, and imparts to the tissues of the eye a more natural appearance.

"Unless the pupil have become dilated by disease, it must be fully brought under the influence of atropine before the examination is begun.

"The surgeon, sitting close to and facing the patient, holds the mirror at such a distance in front of the eye to be examined, that the rays of the lamp may be brought to a focus on the patient's retina. As this cannot instantly be found by a beginner, it is better for him to direct the rays at first upon the patient's lower lid, and as soon as a well-defined inverted image of the flame has been formed there, a slight upward and forward movement of the mirror will bring the retina itself within the exact focus of the instrument.

"If it is desired to magnify the parts observed on the surface of the retina, or to increase their illumination, the surgeon, holding the ophthalmoscope steady in one hand, must take in the other a convex lens of about two and a half inches focus, and place it at a suitable distance in front of the patient's cornea.

"So much has lately been written about the value of the ophthalmoscope as a means of detecting incipient disease of the retina, that the student must be warned against the irreparable mischief he may inflict upon an eye, in which vision is only slightly impaired, by subjecting it to an intense glare of concentrated light.

"His first trials should be made on one of the lower animals,—a kitten, for example; and when he has acquired readiness in using the instrument, he may next proceed to examine patients who have long been hopelessly blind, but in whom the media of the eye remain transparent.

"One very important fact should never be lost sight of by those who employ the ophthalmoscope—namely, that the mere concentration of powerful light on the retina, if continued for more than a few seconds, does of itself place the part in an unnatural condition. In exploring the internal ear, by means of artificial light, we may, indeed, concentrate the rays upon the tympanic cavity, or its membrane, to any amount, without injury to the parts illuminated; but the retina, so far from being a merely passive object of examination, is just the one tissue in the body which appreciates the intensity of the rays which fall upon it; and it must be borne in mind, that an eye may be irritable and intolerant of light to an extreme degree, even although there may be a considerable diminution in its power of perceiving objects.

"Incipient amaurosis is a term in itself so vague, and often so indiscriminately employed to designate slight failure of sight from assumed loss of function in the retina, that I would most earnestly impress upon the student the danger of too hastily subjecting persons complaining of slight defects of vision to the searching glare of the ophthalmoscope. First, let the cornea be most rigidly examined, to ascertain whether some loss of transparency may not exist in the axis of vision. Then, after the pupil has been dilated, let the lens undergo a similar scrutiny; and until the observer has thoroughly satisfied himself that both these structures are transparent, let him not subject the retina to the possibly irritating effects of concentrated light.

"The chief value of the ophthalmoscope seems to consist in enabling the surgeon to set aside, as positively hopeless, a large number of cases formerly termed *amaurotic*, or *nervous*, which were assumed to be still curable, because their real nature could not be demonstrated.

"We now know that total disintegration of the vitreous body, detachment of the retina from its connexion with the choroid, and other equally hopeless conditions of structures essential to vision, may exist, without any alterations being produced in the outward appearance of the eye. In enabling us, therefore, to appreciate these conditions, the ophthalmoscope has proved of immense value; but the more delicate changes occurring in the early stages of deep-seated disease, whilst they elude detection by this instrument, are almost certain to be aggravated by its application." (Dixon, pp. 7—10.)

"We frequently meet with patients who have gradually lost sight in one eye or in both, and yet have neither suffered pain nor observed any unnatural appearances—such as spots, sparks, or flashes—on their field of vision. There is nothing faulty in the structure or functions of the iris; in fact, the eye presents every appearance of perfect health; and yet even the brightest light fails to make any impression on the retina.

"Such cases have been commonly classed as *amaurosis;* the absence of inflammatory action in the superficial tissues of the globe making the surgeon discard all thought of the choroid being the seat of the mischief. Yet the ophthalmoscope may at once reveal extensive changes both in the choroid and retina. These two structures may be widely separated from each other by effused fluid. The retina may be partially or wholly overspread with opaque, whitish deposit, or extravasated blood. The papilla of the optic nerve may be atrophied, so as to be barely traceable; the branches of the central artery or vein being fewer in number and diminished in size.

"In short, the whole fundus of the vitreous chamber may afford evidences of extensive disorganization, while the iris and superficial textures appear perfectly healthy. . . . .

"It must needs be a difficult task to suggest any rational plan for treating a disease which, in many instances, begins by inducing mere dimness of sight, without any other noticeable symptom, either local or general. Chronic inflammation of the choroid, with effusion of fluid between it and the retina, sudden extravasation of blood in the same situation, and other changes, equally serious in their nature, are, as I have just stated, revealed by the ophthalmoscope in patients who have not been aware of any impairment of health, and who have suffered little, if any, local pain in the affected eye. In such cases the surgeon is commonly consulted after all the mischief is done, and when treatment can be of no avail. He will at least abstain from attempting violent measures, such as extensive bleeding and the action of mercury, which, while they are powerless to renew a disorganized structure, may seriously impair the general health of the patient." (pp. 174—6.)

"The invention of the ophthalmoscope has been hailed as the beginning of a new era in the study of eye-diseases, and as a means of clearing up all the obscurity which has hitherto surrounded the pathology of the choroid and retina; and doubtless the discovery of extensive organic changes in eyes which had never exhibited any of the ordinary signs of inflammation, has shaken old-established opinions as to the frequency of *functional amaurosis;* that is to say, loss of visual power in the retina without traceable change in its structure.

"Nevertheless, there are many limits to the powers of the ophthalmoscope.

"1. It can afford us a clear view of the retina only when the lens is transparent; and we know how commonly this body becomes opaque where disease has for a long time existed in the deeper tissues.

"2. Unless the pupil be of good size, or dilatable by means of atropine, only a very small extent of the retina can be brought into view; and chronic iritis—so frequent an accompaniment of changes in the choroid and retina—never exists without giving rise to such adhesions between the iris and capsule of the lens as must limit both the size and dilatability of the pupil.

"3. Although various alterations in the retina—loosening of its attachment to the choroid, thickening, extravasation of blood, &c.—are so readily seen by the aid of the instrument, the earlier stages of disease (which alone are curable) produce changes so slight and delicate as to elude observation.

"A marked increase of redness in the colour of the retina sometimes takes place after it has been exposed for only a short time to the rays of the lamp; and the observer, who is not aware of this, may very likely attribute to a settled morbid state of the eye what is in truth only the temporary result of his own examination.

"When the rays reflected from the mirror are first allowed to fall upon the fundus of the eye, a generally diffused, reddish glare is usually perceived; but as soon as the proper focus has been attained—which is known by ramifying blood-vessels coming clearly into view—this red colour changes into an orange-red, an orange-yellow, or, in some cases, even a buff tint. The surface of the retina is now exposed, and as this is not quite a transparent tissue, but only translucent, it appears like a delicate whitish film, overspreading the red network of closely-packed choroidal vessels. The latter are not recognisable as separate trunks, but they produce the effect of an indistinctly mottled layer behind the retina.

"If the patient turns the eye inwards, the papilla of the optic nerve comes into view—a circular patch of white, faintly tinged with pink. From the centre of the papilla emerge the central artery and vein of the retina; the more frequent arrangement being that an artery and vein pass upwards, and a similar pair downwards; both sets of vessels then dividing into several branches, which run towards the periphery of the retina." (pp. 179—181.)

"There are certain forms of blindness which, from their partial extent or transient duration, we cannot suppose to arise from any such alterations in the retina as could be appreciated by the ophthalmoscope; and a search with that instrument, in these cases, would not only be fruitless, but, unless prosecuted with great care, would almost certainly produce mischievous results." (p. 187.)

"One of the most important results of the ophthalmoscope will probably be a great restriction in the administration of mercury. Patients who, for many months, have lost the perception of objects—perhaps even of light itself—will no longer be encouraged to submit to a lengthened mercurial course by the vague assurance that, 'as their case is one of amaurosis, a full course of mercury may give them a chance.' A view of the fundus of an eye overspread with *old* coagula; of a retina detached from the choroid by effusion of serum, and undulating with each movement of the globe; of an atrophied optic papilla; of a vitreous humour filled with opaque filaments and corpuscles; these, and other palpable signs of disorganization, will force the most devoted believer in the omnipotence of mercury to dethrone his idol." (p. 189.)

"The ophthalmoscope now shows us that morbid changes in the vitreous humour are by no means so uncommon as had been supposed. It is, however, far from easy to determine the precise nature of those changes; for not only is it difficult to bring into the proper focus the variously-shaped bodies, which appear as flakes, rounded granules, filaments, &c., but this difficulty is greatly increased by the rapidity with which some of the bodies are whisked about in the vitreous humour by each involuntary movement of the patient's eye. They are probably, for the most part, coagula of effused blood, mixed in some cases with granules of pigment, or the products of inflammation. Some of the larger, more membranous flakes suggest the notion of their being portions of broken-up hyaloid tissue." (p. 192.)*

These extracts from the works of Mr. Wharton Jones and Mr. Dixon, while they may serve as specimens of the style of writing of their respective authors, are valuable as exhibiting the amount of benefit, as well as the difficulties attendant on the new mode of exploring the deep-seated textures of the eye, as met with by men well qualified for observation, and attached to institutions where eye-diseases are extensively treated.

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* For further information regarding the employment of the ophthalmoscope, we may refer the reader to an article by Mr. Wharton Jones, in this Review (Oct. 1854, p. 459), and another by Dr. Bader and Mr. Bransby Roberts, conjointly (April, 1855, p. 501).

Two slight inaccuracies occur in Mr. Dixon's directions for the use of the ophthalmoscope.

At p. 7, he says the patient "may sit with his back to the table, the lamp being close behind him, so that the rays of light just clear the top of his head, and fall perpendicularly, instead of obliquely, on the surface of the mirror." If the mirror were held perpendicularly to the rays, their focus, formed by its reflection, would not enter the patient's eye, but would return towards the luminous object. The mirror must always be placed more or less obliquely in reference to the lamp, if the focus is to fall on the patient's eye.

He says further, that "A chimney faintly tinged with blue, by decomposing the red rays, whitens the light." The blue glass does not decompose the red rays. In the light of a lamp, the red and yellow rays are in excess, so that objects seen by it appear differently from what they do in daylight, which contains a large proportion of blue rays. The blue glass chimney, by adding blue rays, will improve the light of the lamp, by making it approach to the white colour of daylight.

Having occupied so much of the space allotted to this article with the topic of ophthalmoscopic exploration, we must now satisfy ourselves with a rapid review of some of the most interesting parts of Mr. Dixon's 'Guide,' leaving Mr. Wharton Jones' 'Manual' as a work whose character for accuracy, minuteness, and condensation is fully established.

Mr. Dixon arranges the remaining subjects of his work under the following heads:—Conjunctiva; Abnormal States of Sub-conjunctival Areolar Tissue; Cornea; Sclerotic; Iris; Inflammation of Iris and Cornea together; Choroid and Retina; Retina; Vitreous Body; Lens and Capsule; Diseases which Involve all the Tissues of the Eyeball; Diseases of Uncertain Seat; Lachrymal Apparatus; Eyelids; Orbit; Operations for Cataract; Artificial Pupil; Operations for Staphyloma, Strabismus, &c. In an appendix, a number of interesting cases are related.

Pterygium.—Under this head, Mr. Dixon having stated that it is the encroachment on the cornea that usually first alarms the patient, who applies to the surgeon under the apprehension of "a skin growing over the sight," adds, "there is, however, no real danger of this taking place, for, according to my own experience, the apex never extends so far over the cornea as to obstruct the area of the pupil." (p. 13.)

Purulent Ophthalmia.—In purulent ophthalmia, Mr. Dixon reprobates the depletory plan of treatment:

> "If the treatment," says he, "of purulent ophthalmia by excessive depletion be judged by its results—the only sure test—we shall, I think, be forced to confess that there was ample cause for trying some less violent means of cure. It has been suggested that the more temperate habits of the mass of the people at the present day, as compared with what existed fifty years ago, may exert a considerable influence over the inflammatory manifestations of certain diseases, and that those surgeons who describe purulent ophthalmia as they saw it at the commencement of the present century, had really sometimes to contend with a greater fulness and force of circulation in their patients than we are in the habit of witnessing, especially among the overworked and crowded population of our great towns.

Certain it is that, as far as my own experience at a large metropolitan hospital enables me to form an opinion as to the general condition of patients suffering under purulent ophthalmia, I should say that they are uniformly more or less depressed, with a pulse more feeble than natural, and in a state which in every way contradicts general bleeding, and calls for the administration of tonics. There is usually a coated tongue, with loss of appetite, and a brisk purgative is needed at the very outset of the treatment. Afterwards, either bark and ammonia, or quinine, should be given, and hyoscyamus if the patient be restless. Pure air—to many the best of all tonics—must, if possible, be obtained; and all unnecessary confinement to bed, or to one room, avoided. Meat may be allowed once a day, and a moderate quantity of beer or wine; but on this head no arbitrary rule can be laid down. The surgeon's judgment must guide him as to the cases in which he ought to forbid stimulants, recommend them in moderation, or even insist upon an extra quantity being taken.

"Meantime, the local treatment should be commenced at once. I always employ either a solution of alum (eight or ten grains to the ounce of distilled water), to be injected under the lids every quarter of an hour, or nitrate of silver (three or four grains to the ounce), to be applied three times a day. It is useful, after employing the nitrate of silver for a few days, to change it for the alum, or *vice versâ*. The application of the solid nitrate of silver to the whole surface of the inflamed conjunctiva is preferred by some surgeons, but I have not found it superior to the solution above mentioned; it may be used to the cornea in those cases (chiefly occurring, however, in *gonorrhœal* ophthalmia) where rapid ulceration is beginning at its margin.

"The student ought constantly to bear in mind that, although the disease termed purulent ophthalmia has received its name from that symptom which most readily attracts notice—namely, the profuse conjunctival discharge, the real source of danger lies in the *cornea;* and that, even if it were possible so to drain the patient of blood as materially to lessen, or even wholly arrest, the discharge, we might still fail to save the eye. It is not the flow of pus or mucus, however abundant, that should make us anxious, but the uncertainty as to whether the vitality of the cornea be sufficient to resist the changes which threaten its transparency.

"These changes are twofold—*rapid ulceration* and *sloughing*. Now, has any sound surgeon, I would ask, ever recommended excessive general bleeding and salivation as a means of averting these morbid changes from any other part of the body except the eye? And, if not, why are all the principles which guide our treatment of disease in other organs to be thrown aside as soon as it attacks the organ of vision?

"Do what we may, it must sometimes happen that, in the more acute cases of purulent ophthalmia, our best endeavours are in vain, and the cornea becomes irreparably damaged; still I feel convinced that, if we are unremittingly watchful to observe the changes which take place in the eye itself or in the general health of the patient, and to modify our treatment accordingly, a stimulating plan such as I have sketched will do all that our present knowledge of the disease can enable us to accomplish." (pp. 32—34.)

Granular Conjunctiva.—The following is the advice of Mr. Dixon in one of the most desperate maladies of the eye—granular conjunctiva:

"I believe that in most cases of granular lid our chief dependence must be placed in improving the patient's general health, by giving him iron and quinine, singly or in combination, regulating his diet, and, if possible, placing him in a pure and bracing air. An issue in the skin of the temple, kept open with a single pea, and occasionally stimulated if the discharge becomes scanty, with some caustic or other irritant, is a slow, but often very serviceable adjunct. Tincture of iodine painted on the skin of the lids is also useful.

"I have at various times tried all the most approved lotions and drops, but have never satisfied myself that any of them were of much benefit. The acetate of lead in fine powder, dusted over the everted lid, produces considerable pain at the time of its application, but afterwards gives decided relief, apparently by mechanically filling up the interstices of the "granulations," and so producing a smoother surface for the eyeball to move upon. As the salt slowly dissolves, it probably exerts also an astringent effect upon the vessels supplying the enlarged follicles and papillæ, and so diminishes the size of these excrescences." (p. 36.)

Gonorrhœal Ophthalmia.—"I do not remember," says Mr. Dixon, "to have seen a well-marked case of gonorrhœal ophthalmia in which both eyes were not affected, although it is certainly true that, very commonly, an interval of two or three days elapses before the second eye is attacked; and even then the disease is often much milder in one eye than in the other." (p. 38.)

Our own experience would lead to the conclusion, that gonorrhœal ophthalmia, a disease distinctly produced by the contact of the urethral discharge, seldom, if ever, affects both eyes. Such a course as is described by Mr. Dixon would lead us to suspect merely an accidental coincidence of non-specific purulent conjunctivitis with gonorrhœa.

Scrofulous Inflammations of the Eye.—In the treatment of this very numerous and distressing class of diseases, Mr. Dixon directs, that great attention be paid to the state of the general health. Thus, speaking of scrofulous conjunctivitis, at p. 49, he warns us, to—

> "Take care not to confound the disease with mere conjunctival inflammation, or to suppose that medicines of specific action against scrofula (if any such there be) can afford a substitute for that general dietetic and other treatment which has for its aim the strengthening and soothing of an enfeebled and irritable system."

Scrofulous Iritis.—A case of scrofulous iritis, related at p. 157, confirms in a very striking manner the effect produced by proper attention to diet. Good beef-tea and other nourishing food were chiefly relied on, under which treatment the whole aspect of the eye was changed; effused blood and flocculent deposit were taken up from the pupil and the anterior chamber, and a solid nodulated mass, of a yellow colour, which had been thrown out on the surface of the iris, disappeared. The case is important, both as illustrative of the admirable effects of dietetical treatment, and confirmative of the occasional existence of a rare form of iritis.

Scrofulous Conjunctivitis.—The treatment recommended for this very troublesome affection is on the whole judicious, with the exception of a lotion of acetate of lead, which our author says may be employed. We had hoped this piece of barbarism had been wholly abandoned, from the fact being so perfectly established of the opaque and indelible deposits which form on ulcers of the cornea, when solution of acetate of lead is allowed to touch the eye.

A serious omission in the treatment is, that no notice is taken of the use, neither internal nor external, of belladonna, or atropine. Mention, no doubt, is made at page 52, of another medicine belonging to the class of mydriatics—namely, hyoscyamus, a pill of the extract being directed at night, or night and morning, when there is restlessness at night, or extreme intolerance of light during the day. The external medication from which by far the most remarkable effect in relieving the intolerance of light in scrofulous ophthalmia is obtained, is belladonna, or atropine, employed under the form of collyrium or drop; while small doses of belladonna leaf internally act also so beneficially, that we incline to place them next to quinine, and before the preparations of iron.

Conical Cornea.—The notion of some, that conical cornea is always preceded by a central ulcer, Mr. Dixon regards as disproved by the fact which he has repeatedly verified, that in some corneæ, exhibiting the deformity in the most marked degree, not the slightest opacity has been found, either at the apex of the cone or elsewhere. That ulcer sometimes is a precursor of conical cornea, he confirms by the case of a child, aged four years, who was first brought to him with an opacity at the centre of the left cornea, the result of a small ulcer which had cicatrized. The cornea was very slightly conical, and, except at the centre, quite transparent. In twenty-two months after, the child was brought again, the sight of the eye having become very defective. The small cicatrix was in the same state as before, but the cornea had assumed a completely conical form. (p. 68.)

> "As regards my own experience of conical cornea," observes our author, "I have never found astringent or stimulating applications of the slightest use; nor have I felt warranted in performing any of the operations which have hitherto been recommended." (p. 70.)

Abrasion of Epithelium of Cornea.—Mr. Dixon thus describes the effects of a slight scratch, scraping off a portion of the corneal epithelium:

> "It would be impossible for any one who had not actually witnessed the effects of this seemingly trifling injury, to believe it could give rise to such acute suffering as it sometimes produces, especially in persons of an irritable nervous system. I have seen many almost fainting in consequence of the pain resulting from the edge of a sheet of paper, the cuff of a coat, or an infant's finger-nail, coming in contact with the cornea, even although the abrasion of epithelium was so minute that it required the most careful examination to detect it." (p. 94.)

Mr. Dixon states the most soothing application, and one which produces instantaneous relief, to be a drop of perfectly fresh castor or olive oil upon the surface of the cornea. Three days are generally required to effect a regeneration of the abraded surface, during which rest of the eyes should be observed. We have never thought of any application to the cornea itself, having always found the pain to yield speedily under the influence of extract of belladonna painted on the lids, and a pledget, wrung out of cold water, laid over the eye.

Corneitis, or Keratitis.—Those practised in the observation of eye diseases will readily acknowledge the accuracy and clearness of Mr. Dixon's description of corneitis. With respect to the treatment, he expresses his "conviction that, in the vast majority of such cases, if not in all, both bleeding and mercurialization are most injurious." (p. 75.) He also disapproves, and certainly with justice, of the local application of nitrate of silver. (p. 76.)

We are not disposed to coincide altogether in the rejection of mercury. To debilitate the patient by a full course of mercury, or to salivate, would certainly be wrong; but either by itself, or combined with quinine, mercury in alterative doses generally exercises so beneficial an effect in corneitis, that we must regard it as most valuable in a disease so apt to baffle the best directed treatment. Mr. Dixon says nothing of another excellent remedy in this very troublesome affection—namely, tartrate of antimony; and of the powers of belladonna administered internally he gives no hint.

Suppuration and Ulceration of the Cornea.—Mr. Dixon's statement

(p. 79), that inflammation of the cornea, attended with the formation of pus among its fibres, or loss of its substance by ulceration, is invariably followed by a permanently opaque cicatrix, cannot be admitted. Such a result is frequent, perhaps general, but not invariable. Onyx, or abscess of the cornea, is often discussed, so as to leave no opacity; and a transparent cicatrix, at first depressed but ultimately on a level with the rest of the cornea, and leaving no trace of its existence, is not at all uncommon.

Neither can be admitted as consonant with fact, our author's account of the progress of an abscess of the cornea, when he says (p. 80), that pus continuing to be infiltrated more and more among the fibres of the cornea, "commonly the posterior lamina is the first to give way," so that "the pus slowly oozes into the anterior chamber, and sinks to the bottom of that cavity." On the contrary, we believe that, to use the language of Hunter, and adopt the truth of a principle which he established, there is a "susceptibility which the parts lying between an extraneous body and the skin have to ulcerate, while all the other sides of the abscess are not irritated to ulceration,"* a fact most remarkably illustrated by the uniform progress of pus through the anterior, and not through the posterior, laminæ of the cornea. We believe that although in some very rare cases of abscess of the cornea a coincident exudation of lymph or of pus takes place into the anterior chamber, the bursting of the cornea is never into that cavity in the first instance, as Mr. Dixon describes it to be, but that the front of the cornea first gives way in every case, to be followed, often, but not always, by the laminæ posterior to the abscess also giving way by ulceration, so that the whole thickness of the membrane is disorganized.

In speaking of the treatment (pp. 82—99) of suppuration of the cornea, Mr. Dixon omits all mention of one of the most effectual remedies—namely, evacuation of the aqueous humour. He notices the fact, that as soon as that fluid escapes through a rupture of the cornea, the pain subsides or ceases; yet overlooks the inference, that a small puncture will remove the tension of the inflamed textures, relieve the pain, and permit a healing action to set in—an inference, the truth of which was practically demonstrated by Wardrop.

Prolapsus Iridis.—Speaking of prolapsus of the iris through a perforating ulcer of the cornea, Mr. Dixon says (p. 85), that "in some books on ophthalmic surgery, the reader is directed carefully to push back again with a probe such protruded portions of iris." We dislike such vague phrases as "some books." If there is any book containing so absurd an advice, why not mention it expressly?

When prolapsus iridis occurs in incised wounds of the cornea, Mr. Dixon advises (p. 98) an attempt to be made to replace it with the small spatula. No doubt there is a difference between this case and that of prolapsus through an ulcer. In general, however, the attempt at reduction in the manner here advised will prove as fruitless in the one instance as in the other. The only method which has succeeded in our hands, for replacing a piece of iris prolapsed through an accidental wound of the cornea, has consisted in these three consecutive steps—

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* Hunter on the Blood, &c., part ii. chap. vi. § 2.

first, bringing the iris under the influence of atropine; secondly, placing the patient in a state of insensibility by means of chloroform; and, thirdly, pressing the bit of iris back with a small probe.

Removal of Foreign Bodies from the Eye.—Under this head the reader will find a number of useful practical hints. It is remarkable, however, that nothing is said of the advantage of bringing the patient under the influence of chloroform, before attempting such operations as the extraction of splinters of metal buried deep in the cornea, lying in the anterior chamber, or fixed in the iris.

Iritis.—In all the varieties of this disease there is apt to occur a change in the colour of the iris. If blue, for instance, it becomes green. The change of colour is generally attributed to the presence of stagnant blood, or a deposit of lymph, in the substance of the inflamed membrane; but Mr. Dixon (pp. 127, 135) thinks it depends chiefly upon yellowness of the aqueous humour, or effusion of yellow lymph into the anterior chamber.

The mottled appearance of the internal surface of the cornea, which is often seen in syphilitic iritis, Mr. Dixon describes as a symptom chiefly of the rheumatic variety. He notices that, if of long standing, it is very difficult to remove. While yet recent, he recommends (p. 133) a blister to the temple, to be kept open with savine cerate.

Abscesses of the iris, such as so frequently occur in syphilitic iritis, Mr. Dixon describes (p. 136) as "masses of fibrin." He notices that they are of the most varied colour, shape, and size: in some cases less than pins' heads; in others, occupying a considerable portion of the anterior chamber; and yellow, reddish, or reddish-brown, according as they are recent, or of some duration, or in proportion to the number of vessels traversing them. The masses in question are generally described as *tubercles;* they are, in fact, abscesses, not mere masses of fibrin. Not unfrequently they burst, and give rise to small hypopya.

Mr. Dixon appears to be of opinion that the mydriatic power of belladonna is of no use in the treatment of the internal ophthalmiæ:

> "Indispensable as atropine is," says he, "in our examination of many morbid states of the eye, I do not regard it as of any service in iritis; for, as I stated at the commencement of this section, an inflamed iris loses its power of motion. Atropine, therefore, must be *useless* during the active stage of inflammation. At a later period, when the iris is beginning to recover its motory function, it may, I think, even *do harm*, and in the following way:—The hinder surface of the iris, termed 'uvea,' is covered with a layer of pigment-cells. When fibrin is poured out behind the iris (which no doubt happens in all cases of acute inflammation), these pigment-cells become, for a time, firmly united to the capsule of the lens; and if, when the iris is regaining its motory function, a forced dilatation of the pupil be effected by the influence of atropine, some of the pigment may be detached from the posterior surface of the iris, and left adhering to the capsule, forming those brown patches so familiar to us in patients who have suffered from iritis. Only get quit of the fibrin which is glueing the pigment-cells to the capsule of the lens, and the iris is at once effectually liberated.
>
> "People sometimes talk and write as if occlusion of the pupil in iritis were the result of spasm of some sphincter muscle, the contractions of which could be paralyzed by atropine, and the pupil thus kept permanently dilated. But the real cause of closure is totally different from this. Fibrin is poured out from the surface of the iris and edge of the pupil, upon the front part of the capsule of the lens, overspreading the latter where it corresponds to the area of the pupil.

Now, if this effusion is not quickly removed by absorption, it becomes organized, and forms a membrane stretching across and blocking up the pupillary opening. Gradually this membrane contracts, and, in doing so, draws together the edges of the pupil, until that aperture is reduced, in some cases, to the size of a pin-hole." (pp. 138, 139.)

It would certainly save a great deal of trouble, both to the practitioner and the patient, if the use of belladonna could be safely dispensed with in the treatment of iritis; but we believe this cannot be done, and that Mr. Dixon's reasoning on the subject is fallacious. On a due employment of these three means of cure—depletion, mercurialization, and artificial mydriasis—the successful treatment of iritis depends.

With regard to what is stated by Mr. Dixon, we would observe, in the *first* place, that an inflamed iris does not lose its power of motion from the very commencement of the attack. The pupil still continues for a time to close during sleep, and expand when the patient is awake. It also dilates, and remains widely dilated, by belladonna, in the early stage of the disease. Belladonna being applied, and the patient bled immediately, the pupil will often be seen to become considerably expanded before the arm is tied up.

In the *second* place, the tearing away of the uvea from the iris, in consequence of belladonna, is an exceedingly rare occurrence, and, as far as we have seen, never occurs where bleeding and the use of mercury are had recourse to with promptitude.

In the *third* place, the little brown patches which are seen in many cases projecting from the edge of the pupil, do not arise from forced dilatation of the pupil, but are most frequently met with in neglected cases, where no belladonna has been employed.

Mr. Dixon's abandonment of belladonna may be regarded as equally extravagant and unjust as the notion of the late Dr. James Hamilton, jun., who supposed calomel to be superfluous in the treatment of idiopathic iritis, and actually attributed the successful cure of the disease to the belladonna:

> "Admitting in its full extent," says he, "the signal success of the late ingenious Mr. Saunders, and of his very intelligent editor Dr. Farre, in cases of inflammation of the iris, which the author most willingly does, he ventures, with much deference, to suggest, that calomel had no other influence in the cure than in subduing the syphilitic virus. He is inclined to attribute the success chiefly to the external application of the extract of belladonna, which was so happily employed by Mr. Saunders; for it cannot be doubted, that if the radiated fibres of the iris be kept in a constant state of contraction, so as to dilate the pupil to the utmost extent, the minute vessels of that delicate part must undergo mechanically such a change as shall probably prevent effusion from their extremities." *

On the subject of *Syphilitic Iritis in Infants*, which has been too much neglected by authors on eye-diseases, the reader will find some valuable observations in Mr. Dixon's work, illustrated by cases.

Aquo-capsulitis.—Mr. Dixon quarrels with this convenient name for a well-marked disease, and proposes to substitute the lumbering appellation of "Inflammation of the Iris and Cornea together." Aquo-capsulitis means inflammation of the free surface of the aqueous cell, and although the parietes of the cell are formed by a variety of structures, the facts that the free surface is continually moistened by one and the same fluid—that the cell itself is closed at every point, so that its secretion, if morbidly increased, is retained within it, and can be removed only by absorption, or by an artificial opening—and that when inflammation affects the cavity, adhesions are apt to form between portions of its parietes, in consequence of effusion of lymph—are sufficient to vindicate the comparison which is made of the aqueous cell to the other shut sacs, and to excuse the name *Aquo-capsulitis.*

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* Observations on the Use and Abuse of Mercurial Medicines, p. 217. Edinburgh, 1819.

Mr. Dixon's description of the disease is short, but differs from what is usually given, in the statement (p. 168), that in the more acute cases hypopyon commonly occurs. This event we have never witnessed.

Cataract.—Mr. Dixon correctly states (p. 197), that the ancients supposed a considerable empty space to exist between the iris and lens, and that a certain humour, dropping into this space, coagulated there into a membrane in front of the lens. He goes on to say:

> "The latter body they supposed to be quite unaffected by the disease, and this opinion was so firmly rooted in their minds that, even after using a needle to depress a cataract, they believed they had only removed an opaque skin out of the axis of vision, and had left the lens untouched, and in all its natural transparency. One sees, therefore, how readily they could trace an analogy between a membranous screen descending in front of the lens, and a *portcullis* let down before the gate of a fortress. Now, the original meaning of 'cataracta' is *portcullis.*" (pp. 197, 198.)

It happens, however, in the first place, that the ancients never applied the term *cataracta* to the disease in question; and that, in the second place, *cataracta* did not originally signify a *portcullis.*

The word καταῤῥάκτης, from ῥάσσω, *I dash down*, originally signified a steep place whence water rushed down; by and by it was applied to a bar or flood-gate, such as might, by partially shutting up a stream, cause an artificial dashing down of water; ultimately, the word came to be occasionally used for what might shut up a door-way—i.e., a portcullis; but it was never employed by the Greeks to signify a disease.

The Greeks styled the disease which we call cataract, ὑπόχυμα ὑγροῦ, which means *the flowing down of a humour.* This name the Arabians translated into their own language by words which the mediæval translators out of Arabic into Latin rendered literally by *aquæ descensus;* whence, by way of synonym, or Latino-barbarous quibble, arose *cataracta,* as applied to the disease in question.

The catoptrical test Mr. Dixon regards as of no real value in deciding on the presence or absence of incipient cataract.

> "Any opacity in the lens," says he, "sufficient to interfere with the reflection of light from its two surfaces, must be readily appreciable, on simple inspection, by an observer endowed with powers of adjustment of vision for most minute objects, without which no one can profitably study the diseases of the eye." (p. 195.)

We cannot agree with our author in considering the catoptrical test as valueless in distinguishing cases of suspected cataract. Even the eyes of observers perfectly capable of distinguishing minute objects, may be deceived by apparent want of transparency in the lens, when recourse to the catoptrical test would instantly demonstrate the real state of matters. A melancholy illustration of the truth of this occurred some time ago to us in a gentleman, who called upon us to inquire whether we should consider the cataract, which he said we would see in his right eye, ready for operation, the left lens having already been removed, but unsuccessfully, for that disease, in London. The left eye bore the marks of a hapless extraction. The right lens appeared to us free from opacity, but had been pronounced by the operator on the left eye to be affected with incipient cataract. To neither eye had the catoptrical test been applied. In the right eye, the three images were perfectly normal; the case was one of amaurosis. The patient had come to London from a distant colony, under the impression that the failure of his sight was owing to cataract; he had paid a large sum of money for the removal of a sound lens; he had suffered pain, confinement, and deterioration of health, in consequence of an unnecessary operation; he had lost the only proper season for being successfully treated for the amaurotic affection under which he laboured; and all this under circumstances the real nature of which would have been revealed in a few seconds, had a lighted candle been passed before his eyes by any one even moderately acquainted with the catoptrical test. We consider it just to mention, that Mr. Dixon was not the operator in this deplorable case.

The point in question—namely, the utility or inutility of the catoptrical test in doubtful cases, being a very important one, we shall make no apology for presenting the reader with the opinion upon it of a gentleman well qualified to judge—namely, Dr. Hays, of Philadelphia.

"In the diagnosis of cataract," says he, "the catoptric examination of the eye affords the most unerring test.

"In the early stages of lenticular cataract, the brilliancy and distinctness of the inverted image are diminished; it has no longer a sharp and well-defined margin, but its outline appears shaded off. This image gradually fades with the increase of the opacity, and long before the cataract is mature, the inverted image is obliterated. The deep erect image is also indistinct in the advanced stage, the anterior surface of the capsule giving only a general reflection.

"In capsulo-lenticular cataract, the inverted image fades much earlier than in mere lenticular cataract; a very slight degree of opacity of the capsule sufficing to destroy its function of reflection.

"Among the numerous cases we have seen, in which we have derived great assistance in our diagnosis from the aid of the catoptric test, we shall relate two, which will serve to show its utility, and to justify the confidence we repose in it.

"In September, 1839, I was invited by my friend, Dr. G. W. Norris, to examine a mulatto man, named Peter, in the Pennsylvania Hospital, who was supposed to be affected with glaucoma. The pupils had been dilated by the application of belladonna. There was opacity in both eyes, which was denser in some parts than in others. This opacity seemed more deep-seated than is usual in cataract, and its colour was of a greenish grey. Vision was, however, quite as good, perhaps better, than might have been supposed from the degree of opacity.

"On holding a lighted candle before the eyes, the three images were visible. The anterior upright image was natural in all respects. The deep-seated upright and inverted images were dull, their margins indistinct, and of an unusual reddish tint. The inverted image in one eye disappeared as the candle was moved opposite to the more nebulous portion of the lens; and when the observer looked at the eye of the patient obliquely, the second inverted image seemed to have a double point, like the letter W. I did not hesitate, from these phenomena, to pronounce it to be a case of cataract.

"A few days afterwards this man died suddenly, and we were afforded an opportunity of examining his eyes.

"The transparency of both capsules was impaired. A narrow streak at the posterior part of one lens, extending from near the margin to the centre, was quite opaque, and on applying a needle to this portion we found it quite soft, so as to be readily removed, leaving a depression. On carefully washing both lenses, so as to remove their superficial layers, which were softened as well as partially opaque, the remaining portion of each was found perfectly transparent and of a beautiful amber colour. This colour was the same whether the lens was viewed by transmitted or reflected light.

"The second case was that of a man named Christian Minster, forty years of age, admitted into Wills' Hospital, October 7th, 1841. This man stated that he had recently come from the country; that he had been admitted in one of our public institutions, where he was pronounced to have cataract, and it was determined to operate upon him; but that, being unwilling to submit to this, he had eloped. In a letter which he showed from his physician in the country, his disease was said to be cataract.

"The opacity behind the pupil had certainly great resemblance to that of cataract. The degree of vision he enjoyed corresponded to the degree of opacity; the independent and associate motions of the iris were tolerably active, and the patient saw best by twilight.

"On applying the catoptric test, however, it was at once found to be a case of amaurosis, and not of cataract. The three images were visible, and of their natural appearance. The history of the case led me to ascribe the amaurotic affection to congestion of some portion of the nervous apparatus, and a course of treatment corresponding to this view was directed, consisting of counter-irritation to the back of the neck, purging, stimulating pediluvia, &c. Under this course he improved so rapidly, that in a week he could read with one eye a diamond-print Bible. He subsequently had a relapse; but by the application of cups to the head, pustulation with tartar-emetic on the back of the neck, and afterwards ptyalism, he was completely restored. He was discharged cured in January following."*

If the catoptrical test be capable of assisting, even in a moderate degree, in assuring an accurate diagnosis between incipient cataract and incipient amaurosis, it is surely worthy of cultivation by practical men. If it affords the smallest chance of avoiding such deplorable results as occurred in the case which we have related, or as were likely to happen in Dr. Hays' second case, in place of being scouted as "of no real value," it deserves the utmost possible attention.

Hard Cataract.—With respect to the formation of hard cataract, Mr. Dixon has adopted views similar to those of M. Malgaigne, and believes the ordinary progress of the disease in elderly persons to be in *striæ*, proceeding from the circumference of the lens towards its poles.

"From puberty to the age of forty," says he, "opacity of the lens is rare, except as a result of injury. When it does occur spontaneously, it is *cortical* in its origin; the margin of the lens first exhibiting opaque striæ, which gradually converge as they extend themselves along its anterior and posterior faces. Between forty and fifty, cataract may still be said to be rather a rare disease; but from fifty to sixty, and still more after the latter period, it is the defect we naturally expect to find when a patient consults us for failing sight." (pp. 212, 213.)

Mr. Dixon had already explained that when the *nucleus* of the lens

"Becomes cataractous in an old person, the change seems to consist in a process of drying and atrophy of its fibres, although its amber tint, and a certain degree of translucency, are still retained." (p. 199.)

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* Hays' Edition of Lawrence on the Diseases of the Eye, p. 675. Philadelphia, 1854.

He goes on to state, that it is impossible to fix the exact time of life at which the peculiar morbid changes, now referred to, take place in the nucleus:

"Perhaps," says he, "sixty might be named as the age after which it may be expected to occur; although I am perfectly convinced, from repeated and careful examination of patients, that even to extreme old age it is much more common to find marginal opacity beginning while the nucleus is still clear, than to find nuclear opacity beginning while the periphery of the lens remains transparent." (p. 213.)

"The following is the ordinary progress of cataract in elderly persons:—At first, opaque streaks appear at the extreme edge of the lens; most commonly I have observed the lower edge to be first affected; the streaks gradually coalesce into patches, and spread themselves over the posterior face, a few only extending a short distance along the anterior face. The cataract may perhaps remain in this state for a year or more; then the whole body of the lens becomes slightly hazy, but not so much so as to prevent the posterior radiated opacity from being recognised. Gradually the opaque streaks advance farther and farther along the anterior face of the lens, until they appear within the area of the undilated pupil. By this time the general haziness has increased so much that the posterior face of the lens is hidden from view, even when concentrated light is thrown into the eye; and the opacity—both the linear and the diffused—becomes denser and denser, until little more than the anterior surface of the lens can be seen. Cataract is then complete, and vision commonly restricted to mere perception of light.

"As years go on, the surface of the lens usually becomes more and more opaque and white, in consequence of the increased deposit of earthy and fatty matter. The latter sometimes assumes the form of cholesterin, in such large crystals as to give the surface of the cataract a sparkling appearance." (pp. 215, 216.)

While Mr. Dixon acknowledges the priority of M. Malgaigne in the doctrine, that the opacity of cataract commences in the superficial laminæ, and generally towards the edge of the lens, he seems rather dogmatically wedded to it, and fond of representing the belief of preceding authors to be that the disease begins in the nucleus, and gradually works its way towards the surface. That the opacity commences in the very kernel, and gradually invades the lens from this point towards the surface, till the whole is opaque, does not appear to be the opinion of any ophthalmologist. When Mr. Middlemore says, "Lenticular cataract generally commences with a dulness of the centre of the lens;"* or when Mr. Morgan tells us that "Hard cataract usually begins to form in the centre of the lens;"† these authors probably mean by centre merely that portion of the lens which is seen through the undilated pupil, and where the opacity is, in general, first discerned; the opaque streaks which sometimes fringe the margin of the lens in the incipient stage of the disease not being visible unless the pupil is widely expanded by belladonna.

The fact is, as Mr. Dixon himself acknowledges, there is a twofold change observable in any fully developed senile cataract. The laminæ next the surface are soft, and present a whitish opacity; while the internal half of the lens is firm, amber-coloured, and sufficiently turbid to prevent any object from being seen through it. The changes which produce these appearances do not in all cases go on, *pari passu*. The nuclear change in many cases advances before the superficial, and *vice versâ*. The nuclear change has also attracted more the attention of some observers, and the superficial more that of others. This explanation will go far, we think, to reconcile the descriptions of Tyrrell, Wharton Jones, and others, with the statements of Mr. Dixon.

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* Middlemore on Diseases of the Eye, vol. ii. p. 84. London, 1835.

† Morgan's Lectures on Diseases of the Eye, second edition, p. 175. London, 1848.

Fluid Cataract.—Under this name, Mr. Dixon describes what is oftener called Morgagnian cataract, or *cataracta fluido-dura.*

> "In lenses," says he, "which have become opaque, either from disease or injury, the softening process will sometimes slowly go on until nearly the whole mass is converted into a fluid state.
>
> "The most uniform characteristic of fluid cataract is a total absence of those radiating streaks so evident in ordinary cataract, where the fibres of the lens, although deprived of their transparency, still retain their natural arrangement." (pp. 217, 218.)

He goes on to describe the colours and other appearances of the kind of cataract in question; but the most remarkable of them all has escaped his notice—namely, the change which takes place in the colour of the cataract according as the patient sits erect or lies on his back. In the former position, with the head gently bent forwards, the cataract presents a brownish colour, owing to the nucleus of the lens gravitating forward towards the pupil; but the instant the patient lies down on his back, the cataract assumes a white colour, from the nucleus falling back towards the vitreous humour.

Our limits prevent us from entering minutely on Mr. Dixon's account of the operations performed on the eye for the cure of cataract, or for other diseases.

Spring Speculum.—He frequently recommends the employment of a spring speculum for holding the eyelids apart during operations on the eye. Even in children, about to be operated on for congenital cataract, he appears (p. 317) to use this contrivance; also, in the operation suggested by Mr. Bowman, of tearing open an opaque capsule with two needles, introduced, one in each hand, through the two sides of the cornea (p. 321). In operating for strabismus (p. 366), the spring speculum is again recommended; as well as in forming an artificial pupil (p. 356), an operation in which it is generally so important to prevent the premature escape of the aqueous humour, that we doubt much the propriety of dispensing with the manageable pressure of the fingers of an assistant for the action of a mechanical contrivance which cannot be controlled.

Maunoir's Operation for Artificial Pupil.—Mr. Dixon has fallen into an error regarding this subject, when he says—

> "Maunoir's method of cutting out a piece of the iris with scissors, in addition to other difficulties, has the defect of requiring a large corneal wound to be made before the scissors can be introduced; and, however simple it may look in a diagram to snip out little triangular bits of the iris, any one who has tried to do so on the dead subject will have found that even the finest and sharpest scissors cannot divide, with precision, such a flabby and yielding tissue as the iris becomes the moment the aqueous humour has escaped." (p. 355.)

Maunoir made no such attempt to cut out a piece of the iris as is here attributed to him. His operation consisted merely in dividing the iris by two incisions divaricating from each other, leaving the intervening membrane to shrink.

Chronic Dacryocystitis.—Mr. Dixon's treatment of this very common and annoying complaint appears singularly inefficient. After speaking of the injection of various fluids, either through the puncta or up the nasal duct; of astringent solutions dropped into the corner of the eye, or used as lotions; and of various other means, as abandoned or worthless—

> "The remedy," he says, "I have found most serviceable is a little oval blister, about half an inch long, placed directly over the sac. This may be repeated every fortnight or so, according to the state of the skin; and the plan, to be efficacious, must be persevered in for several months. Astringent drops may be used at the same time." (p. 268.)
>
> "The chief objection to the style (apart from its unsightly appearance) is that it requires management and care such as very few patients indeed can, or will, bestow upon it. They neglect to remove it and cleanse it regularly, and when it sets up some irritation (as it occasionally will do) they probably remove it altogether, and are unable to replace it. These and other reasons have induced me almost to abandon its use, and I do not think I have introduced ten styles within as many years." (p. 269.)

Stricture of the nasal duct is generally at the bottom of chronic dacryocystitis. The question then comes to be, how is the stricture to be removed with least trouble to the patient? We have no hesitation in answering, by opening the sac and passing a probe, of about one-twentieth inch thick, down the nasal duct. The incision into the sac ought to be pretty free, so that the probe may enter easily into that cavity; the probe is then to be directed nearly perpendicularly downwards, and pressed firmly through the stricture. It is rarely the case that the freeing of the stricture in this way does not require a very considerable pressure; under this the stricture yields, and the end of the probe is felt to strike against the floor of the nostril. The probe may be kept in for a few minutes only, or for an hour or two, as is convenient. It sometimes happens that a single passage of the probe, in this way, effects a complete and permanent cure; and with patients coming from the country, and eager to return home, this plan may be tried. The wound requires no further attention. It is better, however, to repeat the passage of the probe daily, as long as the wound keeps sufficiently open to permit that to be done without making any new incision, which is generally the case for fourteen days, or longer. A series of probes, gradually increasing in thickness, may be employed; and during their use attention is to be directed to the general health of the patient, which is often deranged; and such local means employed as are suitable for reducing the inflamed state of the skin over the sac, and removing any catarrhal or tarsal ophthalmia which may be present. From long experience of this simple treatment, we can recommend it as greatly superior to the employment of the style, or to any other plan which has hitherto been proposed.

Entropion and Ectropion.—The operations for the cure of these affections, falling so much under the care of the ophthalmic surgeon, Mr. Dixon omits to consider, for a reason which strikes us as altogether unsatisfactory.

> "The operations," says he, "of entropion and ectropion, involving as they do merely such tissues as are met with in other parts of the body, need not therefore here be specially described." (p. 369.)

Nævus.—Mr. Dixon (p. 286) recommends subcutaneous ligatures, or the introduction of needles coated with fused nitrate of silver, as preferable to the including of any considerable portions of skin within the ligature, or the extensive use of escharotics. He states that he had seen injections of alum cause sloughing of the lids and great subsequent deformity.

Epithelial Cancer, affecting the skin over the lachrymal sac, Mr. Dixon (p. 289) treats with chloride of zinc, applied as a paste over the entire surface of the sore. This application very effectually destroys the growth, and may be repeated as often as any part of the border appears inclined to extend itself.

Strabismus.—Under this head our author takes occasion to mention, that division of the external rectus is very rarely required, but may be resorted to for the purpose of remedying the unsightly abduction which sometimes occurs when the section of the adductor muscle has been accompanied with too extensive separation of its connexions.

> "I lately saw," he adds, "a patient whose appearance had been remarkably improved in this way by the skill of my colleague, Mr. Critchett. The man came to him with the left eye extremely abducted, and quite fixed; and, as the semilunar fold had also retracted, his expression was most disagreeable. Mr. Critchett first divided the tendon of the external rectus, and afterwards removed the cicatrix of the old operation, loosening the conjunctiva very thoroughly from the inner side of the sclerotic. He then brought together, with fine stitches, the gap he had made in the conjunctiva, so as in a certain degree to draw out again the plica semilunaris. The operation was tedious and difficult, but the object was perfectly attained. All deformity was removed; and when I saw the case, some months afterwards, there was no trace of the unsightly leer which the poor man had formerly exhibited." (p. 368.)

Amaurosis.—There is probably no subject in ophthalmology more likely to receive additions and corrections, in proportion as it is investigated on sound pathological principles, than amaurosis. It is universally acknowledged that the methods hitherto pursued in treating this subject have been very insufficient for the purposes of the practitioner.

> "I would wish," says Mr. Dixon, "to impress strongly upon the student, that although, for convenience sake, we may still continue to use the word *amaurosis*, we do not thereby express any opinion whatever as to the real pathology of dim sight or blindness. Amaurosis implies no definite and ascertained disease; it is only a word expressive of our own ignorance as to the cause of our patient's blindness. . . . .
>
> "The descriptions of the older ophthalmic writers have so *popularized* amaurosis and its symptoms, that most students, before commencing the practical study of eye diseases, form to themselves some ideal type of an amaurotic patient, with his 'vacant stare,' and 'widely dilated pupils;' and have commonly a vague belief in the exclusive power of *mercury* to effect a cure.
>
> "Nothing can be more unfounded than such sweeping generalizations." (pp. 182, 183.)

The following are judicious cautions:

> "If a patient comes before us, complaining of dimness of sight, attended with the constant or occasional appearance in the field of vision of moving clouds, dark disks, spots, sparks—in short, of any of the manifold subjective symptoms of disordered retina, we are not hastily to call the case *amaurosis*, and forthwith proceed to treat it according to any fixed routine.
>
> "Our first business should be to ascertain *for ourselves* what sight the patient really possesses; testing his power for near things with type of different sizes, and, for those more distant, by directing his attention to various objects about the room, or across the street. Some patients involuntarily exaggerate their defects of sight to such an extraordinary degree, that their own accounts are absolutely valueless." (p. 184.)

A remark which our author makes (p. 344) respecting the occasional failure of the operation of extraction, leads us to reflect that the same "degeneration of tissue in the bloodvessels supplying the globe, and a weak condition of the heart itself," to which he attributes the loss of the eye in that case, may also, in many instances, have to do with the production of amaurosis. It seems to be necessary, in investigating cases of amaurosis, to pay a much more extended and more minute attention to the state of the constitution and to the antecedent diseases of the patient, than is commonly done. It is rarely the case that amaurosis is a merely local affection, unconnected with general bodily disorder. The inquirer should have in his mind, if not in his hand, while examining an amaurotic patient, a catalogue of the chief affections, general and local, likely to produce or to complicate the disease—a catalogue somewhat like that lately given by Dr. Lauder Lindsay,[*] of the complications of insanity. Such a plan of proceeding might prevent the mistake so frequently fallen into, of singling out some one antecedent of amaurosis as the cause of the disease, while the rest are passed over as merely subordinate conditions. The catalogue might run somewhat as follows:

I. Cachexies and General Systemic Affections:
    1. Plethora; 2. Anæmia; 3. Scorbutus; 4. Scrofula; 5. Syphilis; 6. Fevers.
II. Diseases of the Skin and Cellular Tissue:
    1. Cutaneous Eruptions; 2. Carbuncles and Boils; 3. Erythema and Erysipelas; 4. Ulcers and Abscesses.
III. Diseases of the Digestive System and Alimentary Canal:
    1. Dyspepsia; 2. Chronic Vomiting; 3. Constipation; 4. Diarrhœa; 5. Dysentery.
IV. Diseases of the Respiratory System:
    1. Bronchitis; 2. Asthma; 3. Influenza.
V. Diseases of the Circulating System:
    1. Heart Disease; 2. Aneurism; 3. Varicose Veins; 4. Hæmorrhoids.
VI. Diseases of the Liver and Kidney:
    1. Hepatitis; 2. Diabetes; 3. Albuminuria; 4. Oxaluria; 5. Dropsy.
VII. Diseases of the Brain and Nervous System:
    1. Fractures of Cranium; 2. Tumours of Dura Mater; 3. Morbid Formations in Brain; 4. Wounds of Fifth Nerve; 5. Epilepsy; 6. Paralysis; 7. Hysteria; 8. Mania.
VIII. Diseases of the Genito-Urinary System:
    1. Gonorrhœa and Gleet; 2. Spermatorrhœa.
IX. Diseases of the Uterine System:
    1. Menorrhagia; 2. Leucorrhœa; 3. Amenorrhœa.
X. Blood Diseases, affecting the Bones and Joints:
    1. Rheumatism; 2. Gout.
XI. Chronic Poisoning:
    1. By Alcohol; 2. By Tobacco; 3. By Lead; 4. By Mercury.
XII. Diseases of the Organs of Special Sense:
    1. Ophthalmia; 2. Otorrhœa; 3. Deafness; 4. Anæsthesia; 5. Neuralgia.

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[*] Journal of Psychological Medicine, p. 70. January, 1855.

In perusing Mr. Dixon's work, we had noted many other passages besides those which we have quoted in this analysis, sufficiently interesting for remark. We also purposed directing our readers' attention to several valuable details of cases—such as, one of prolonged sojourn of a foreign body in the anterior chamber (p. 162); one of accidental transfixment of the lens from side to side with a pin (p. 220); two of acute choroiditis (p. 231); one of melanosis within the vitreous chamber (p. 239); one of scrofulous deposition at the bottom of the eye (p. 243), similar to a case given by Panizza; one of total detachment of the iris (p. 372), and several others; but as this article has already gone far beyond the limits we contemplated, we shall extend it no further than cordially to recommend the work to the study of all who feel interested in the treatment of eye-diseases, and who are anxious to know something of the experience of one who has had ample opportunities, and possesses excellent talents, for observation.

W. Mackenzie.

Review IX.

1. *The Identity or Non-identity, or the Specific Cause, of Typhoid, Typhus, and Relapsing Fevers.* By William Jenner, M.D., Professor of Pathological Anatomy in University College, London. ('Medico-Chirurgical Transactions of London,' vol. xxxiii.) 1850.
2. *Clinical Reports on Continued Fever, based on Analyses of One Hundred and Sixty-four Cases; with Remarks on the Identity of Typhus and Typhoid Fevers, &c. &c.* By Austin Flint, M.D., Professor of Principles and Practice of Medicine and of Clinical Medicine in the University of Buffalo.—*Buffalo,* 1852. 8vo. pp. 390.
3. *The Gulstonian Lectures on the Acute Specific Diseases.* By William Jenner, M.D., &c. &c.—*London,* 1853. 12mo. pp. 55.

The subject of Fever, in all its varied aspects, has been at different times so thoroughly discussed in the pages of this Review, as to lead us at once to apprise our readers of the limited, though not on that account less interesting, nature of our present inquiry. We propose to examine the evidence which has up to this time been adduced in regard to the question of the separate and distinct nature of certain forms of continued fever —in other words, to inquire if the existing state of our knowledge permits the conclusion that typhus and the so-called typhoid fever are separate and distinct diseases, and if relapsing fever is likewise entitled to be so regarded. As respects fever, this question of the identity or non-identity of certain of its forms is emphatically the question of the day. For, while at the outset we are prepared to acknowledge that the facts which have been accumulated and brought forward during late years have been sufficient to enable us to make up our own mind as to the non-identity of the three diseases now named, we are also aware of the different notion which is still entertained by many whose opinions are justly entitled to the fullest consideration, and shall therefore feel it our duty in stating our own view, to do so with becoming deference. As the subject of the morphological changes occurring in fever, to which the recent most important investigations of Virchow and Dr. Parkes have added a new interest, will fall to be discussed in another article, our present inquiry chiefly aims at the nosological classification of fevers by the contemplation of their origin, symptoms, and course.

That the distinction of certain forms of continued fever, and their recognition as separate diseases, should have remained to be achieved by the physicians of our own day, need create no surprise, when we reflect that for many years small-pox, measles, and scarlet fever, now universally acknowledged as distinct, were believed to be the same disease, and were confounded under one name. Such being the case, as little surprise need be occasioned by the fact of many enlightened physicians still regarding the three forms of fever already named as varieties of one disease.

The admirable description of typhoid fever contained in the illustrious work of Louis,[*] "by affording a standard of comparison," as has been well remarked by Dr. Jenner, "materially lightened the labour of separating from that disease those which had previously been grouped with it." Louis' work was originally published in 1831; previous to that time, several French physicians—among others Prost,[†] Petit,[‡] and Serres, Bretonneau[§] and Trousseau[||]—had written fully on this form of disease; while by Bretonneau in particular it had been accurately described under the name of *Dothinentérie*, or *Dothinentérite*. An English physician—by name W. Stark—is mentioned by M. Andral as having given, in 1788, a very fair account of the alteration of Peyer's glands. Stark subsequently died of the disease he had himself described.[¶] The revolution of prevalent views in regard to fever was, however, mainly accomplished by Louis when he described his fièvre typhoïde. During the interval which elapsed before the publication of a second edition of his work, which, strange to say, did not appear (and then in a considerably enlarged form) till 1841, the distinguished author had not only the satisfaction of finding his observations amply confirmed by his own countrymen—Lombard (who studied the typhus fever of this country), Chomel, and others—but by many foreign physicians, particularly in America. Of the latter, M. Louis, in the preface to his second edition, cites the names of about a dozen; among others, those of Dr. Gerhard[**] and Professor Jackson,[††] to whose contributions of sterling value we shall have occasion to refer in the course of this article. In our own country, the excellent papers of Mr. Kennedy, of Dublin, and Dr. A. P. Stewart,[‡‡] now of the Middlesex Hospital, were the earliest to give a faithful description of the symptoms and pathological appearances in cases of typhoid fever.* In the conclusion of his paper, Dr. Stewart puts the question—"Are typhus and typhoid fevers identical, or are they not?" While hesitating to give a direct reply, it is very evident how Dr. Stewart's opinion tends; while, after perusal of the whole article, and of Mr. Kennedy's, more especially in connexion with the papers of the American physicians already named, we have been impressed with the feeling of a strong probability, though not actual proof, of the two diseases being separate and distinct, having been made out. The actual demonstration, however, of the non-identity of typhus and typhoid fevers has remained for Dr. Jenner to accomplish, and we are very ready to acknowledge that by him the greatest amount of light has been thrown upon this difficult but most important and interesting subject.† This belief we have indeed very great pleasure in avowing, because we are aware of no other papers on any scientific medical question which in themselves have more completely evinced the great amount of trouble in research, and devotion, with which the whole inquiry has been conducted by their author.

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[*] Recherches Anatomiques, Pathologiques et Thérapeutiques sur la Maladie connue sous le nom de Fièvre Typhoïde, &c. Par P. C. A. Louis. Deuxième édition. Paris, 1841.

[†] Médecine éclairée par l'Observation et l'Ouverture des Corps. Paris, 1604.

[‡] Traité de la Fièvre Entéro-Mésentérique. Paris, 1813.

[§] Archives Générales de Médecine. Première série, tom. xxi.

[||] De la Maladie à laquelle M. Bretonneau a donné le nom de Dothinentérie ou Dothinentérite, par M. Trousseau: Archives Générales de Médecine, 1826.

[¶] Pathologie Interne, vol. iii. p. 665; Stark's Works, 4to, London, 1788.

[**] The American Journal of the Medical Sciences for 1834 and 1837.

[††] A Report founded on the Cases of Typhoid Fever, or the Common Continued Fever of New England, which occurred in the Massachusetts General Hospital, &c. &c. By James Jackson, M.D. Boston, 1838.

[‡‡] Some Considerations on the Nature and Pathology of Typhus and Typhoid Fevers, applied to the Solution of the Question of the Identity or Non-identity of the two Diseases. By Alexander P. Stewart, M.D.: Edinburgh Medical and Surgical Journal, 1840.

Aided chiefly by Dr. Jenner's observations, and those of Dr. Flint, we now proceed to exhibit, in as succinct, but at the same time as clear, a manner as possible, the present state of our knowledge on the subject of the difference between typhoid fever and typhus. We shall commence with the consideration of the course, symptoms, and lesions of the two diseases, and conclude with the view which Dr. Jenner has offered, of the difference in their specific cause.

The origin of typhus is certainly more distinctly marked than that of typhoid fever, the occurrence of crises and critical days in the former are more easily recognised, and the duration of the disease is more strictly limited and altogether shorter than in typhoid. "The mean duration of typhus," writes Dr. Stewart, "is about one-half that of typhoid fever;" and the same author, after speaking of the occurrence in typhus of a perceptible crisis. adds,

> "In reply to the question, whether this is the case in typhoid fever, I can only adduce my own experience when I state, that neither in the numerous cases I saw in Scotland, nor in those I watched in Paris (about a dozen of them very carefully), have I ever seen anything approaching, in the remotest degree, to what I have noticed so frequently in typhus."

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* Peculiarities in the types of prevailing fever, and more particularly as respects the morbid changes visible in the course of the intestinal canal, had about the same time arrested the attention of various other observers in this country. See, for example, the interesting account given by Mr. Goodsir (now the distinguished professor of anatomy in the University of Edinburgh) to the late Dr. John Reid, of the fever prevalent at Anstruther, in Fifeshire. Edinburgh Medical and Surgical Journal, p. 459. 1839.

† Dr. Wood, of Philadelphia, in his admirable work on the 'Practice of Medicine' (vol. i. p. 360), says:—"It was in the results obtained by the careful post-mortem examinations made in the Philadelphia Hospital by that distinguished pathologist (Dr. Gerhard), in conjunction with Dr. Pennock, and their no less careful investigation of the symptoms during life, that we first obtained positive proof of an essential distinction between the two diseases, which Louis himself had previously been disposed to consider as identical." Interesting and most valuable as the observations of Dr. Gerhard were, Dr. Wood here claims too much both for him and them. The production of "positive proof," if such an expression be permitted, is undoubtedly due to Dr. Jenner, since the tracing of the contagion of the two diseases to separate and distinct habitats is the real proof of their non-identity. With the highest appreciation of the correctness and value of the observations of Mr. Henry Kennedy, of Dublin (Dublin Medical Journal, 1838), we cannot allow his claim to distinction, as some writers of the sister island appear to do, to be considered as superior to that of Dr. Gerhard, Dr. Stewart, and several other physicians.

In regard to the question of duration of typhus, the following, among other facts, are given by Dr. Jenner:[*]

"Seven cases were received into the hospital, respectively on the fourth, fifth, sixth, eighth, tenth, twelfth, and sixteenth days of the disease, the average day of admission being the 8·7th."

Again, of thirteen patients, the exact duration of whose disease before admission could not be ascertained, the average day was about the 14·5th. We have mentioned these facts in order to render as clear as possible those relating to the duration of illness after admission, which chiefly concern our present inquiry. Now, of the seven cases first mentioned, three survived till after the fever had run its course. The others died respectively on the twelfth, seventeenth, twentieth, and twenty-seventh days of the disease, the average day of death being the nineteenth. This, Dr. Jenner observes, being probably below what a larger number of cases would give, because one proved fatal on the twelfth day from severe pneumonia. Then, of the thirteen cases, six lived some time after the termination of the typhoid fever, and the remaining seven died severally on the sixteenth, seventeenth, twenty-third, twenty-fifth, twenty-seventh, twenty-eighth, and thirtieth days of the disease. The average day of death for these seven cases being the 23·7th.

Entering into as minute details regarding certain cases of typhus, Dr. Jenner exhibits the 14·4th as the average day on which the disease proved fatal in seven cases, and then contrasts the typhus cases, of which thirty-six per cent only proved fatal after the fifteenth day of the disease, and not one after the twentieth day, with the cases of typhoid, of which 90·9 per cent. proved fatal after the fifteenth day, and nearly one-half after the twentieth day. In presenting a "summary of symptoms[†] distinctive of typhoid and typhus fevers," Dr. Flint observes that the duration of disease prior to admission into hospital is in a marked degree shorter in typhus than in typhoid; while in another part of his work (p. 121) he accounts, and quite correctly, for this circumstance, from the symptoms in typhus sooner assuming a degree of gravity, and so leading the patient or friends to seek hospital relief. Mild as the symptoms have been in some cases, M. Louis expressly states that he has never known the disease to continue during a less period than fourteen days.[‡] The average duration of typhoid fever in severe cases, writes M. Andral, is from twenty-eight to thirty-two days, and sometimes it may continue for two months or two months and a half.[§] The duration of typhoid fever according to M. Valleix,[||] is very different in different cases; in the milder cases it varies from fourteen to twenty or twenty-five days; in severe cases it may extend to thirty-five or forty days, and even to a much longer period. Dr. Jackson (p. 32), in his most interesting report, previously referred to, says, "The difference as to the day of convalescence in different years was very great, the extremes being nearly eighteen in one year, and nearly twenty-six in another." Speaking of the mode of access of typhoid fever, Dr. Bartlett observes, "There is no other acute disease, perhaps, in which the attack is more frequently slow and gradual than in this."* (p. 10.) The point we have now considered is not the one upon which the proof of the non-identity of typhus and typhoid fevers is to rest; but still, without drawing upon the reader's indulgence by quotations relative to the course of typhus, with which we shall consider him familiar, we think that in the opinions of the different authors referred to, more especially as regards the duration of the disease described, there is afforded abundant evidence of a very material difference between it and typhus.

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[*] Monthly Journal, p. 673. 1849.

[†] We would here take the liberty of reminding Dr. Flint, that "Age," "Nativity," "Season," and "Duration of disease," ought not to be called "Symptoms"—the application of this term is not only confusing, but erroneous.

[‡] Louis, vol. ii. p. 510.

[§] Pathologie Interne, vol. iii. p. 682.

[||] Guide du Médecin Practicien, vol. v. p. 464. M. Valleix has long been known as an earnest student of the subject under discussion; in the cause of which he at an early period did good service by his able analysis of the cases collected by Dr. Shattuck, of Boston.

The symptoms of typhoid fever, which we shall now shortly consider, as affording evidence of a marked difference or distinction in this form of disease from typhus, are, 1st, the appearance presented by the skin, in other words, the different character of the eruptions; and 2nd, the condition of the abdomen, the state of the bowels, and the appearances presented by the dejections. These afford the indications of greatest difference in what may be called the best marked cases, and while there exist many other, and some sufficiently striking, differential characteristics in the symptoms of the two diseases—as, for example, in the pulse, and in the condition of the chest, the aspect of the patients, and the state of the cerebral functions, the characters presented by the urine, &c.,—we shall not, for the reason already mentioned, direct particular attention to them, though it is proper here to allude to their existence. The reader, if anxious to compare the observations of different physicians on these points, is referred to Dr. Jenner's paper in the ninth volume of the 'Monthly Journal,' and to Dr. Flint's work (at page 237).

1st. *The Eruption in Typhoid Fever.*—The following is the description of the separate rose-coloured spots (the *taches roses lenticulaires* of Louis) as given by Dr. Jenner:

> "The separate spots are circular, and of a bright rose colour; this hue passes insensibly at their bases into that of the surrounding cuticle. Their usual diameter is about two lines. They are somewhat elevated; but, although perceptible to the finger pressed lightly over the surface, they possess none of the seed-like hardness of the first day's eruption of small-pox, nor are they so prominent and perceptible to the touch as the papulæ of lichen. Their surface is rounded, lens shaped, never acuminated. No trace of vesication can be detected on their apices. If tolerably firm pressure be made on these spots, they entirely disappear; but they resume their distinctive colour and elevation as the finger is being withdrawn."†

The exact period when these peculiar spots appear varies considerably in different cases. Louis‡ mentions that, in three individuals who came under his observation on the fourth and fifth days of the disease, the spots did not appear before the sixth and seventh; further, that they were detected in six cases, who were examined for the first time on the eighth day, and that they were generally found on admission in the cases of patients who were brought to the hospital after the continuance of the disease for ten days. In all these the disease proved fatal. According to Dr. Jenner, "the eruption in typhoid fever appears from the seventh to the twelfth day of the disease, very rarely later, and still more rarely at an earlier period." Dr. Jenner has frequently noticed the occurrence of the characteristic spots, preceded by a very delicate scarlet tint of the whole skin, resembling the colour of the skin in a person who has recently left a hot bath.

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* The History, Diagnosis, and Treatment of Typhoid and of Typhus Fevers, &c. By Elisha Bartlett, M.D., Professor of the Theory and Practice of Medicine in Transylvania University. Philadelphia, 1842.

† Medico-Chirurgical Transactions, vol. xxxiii. p. 26.

‡ Vol. ii. p. 96.

> "The ordinary duration of each spot," says Dr. Jenner, "is about two days, but it varies from two to six days. Fresh spots appear every day or two from the outset of the eruption, till from the twenty-first to the twenty-eighth day of disease. This successive daily eruption of a few small, very slightly elevated, rose-coloured spots, disappearing on pressure, each spot continuing visible for three or four days only, is, so far as I know, peculiar to, and absolutely diagnostic of, typhoid fever."

The trunk of the body, both anteriorly and on its posterior surface, is the ordinary site of the typhoid eruption, in comparatively few cases are the spots found on the limbs. It would appear from Louis' observations, that the abundance or rarity of the lenticular spots do not hold a fixed relation to the gravity or mildness of the disease. It is probable, from what Louis and others have found, that in a few cases of typhoid fever the distinctive eruption now described may be wanting; but it is certain that in almost all cases it will be found to exist, although its duration may frequently prove very transient, and the spots themselves be both few and indistinct.[*] We are sorry that we cannot congratulate Dr. Flint upon the clearness of his description of the typhoid and typhus eruptions. The spots in the former are certainly not oval; they are, on the contrary, distinctly circular. After the accurate manner in which the observations on this subject are given by Louis and others, it would be disappointing to the reader were we to dwell on the somewhat loose statements of Dr. Flint; we shall, however, indicate some of those which we think objectionable, and which, from the vagueness of statement and the slight amount of information they convey, had better have been altogether omitted.[†]

> "In *many* of the cases the disease had commenced *several* days before they came under observation, and the eruption was apparent from the first. . . . The size of the spots is not given in the history of any case. It is stated, however, in some of the cases, that the spots were *of different sizes*." (pp. 88, 89.)

Absence of the eruption is mentioned as having been noted in three cases (p. 89), but no indication is given of the period when the hospital cases (two of the three) came under treatment; reference to a most important element is therefore entirely overlooked. How disappointingly vague is the following, in regard to sudamina:[‡] "It is very probable that they may have been present in other cases in which they were not noticed." (p. 89.) A *petechial* eruption, which, not unlikely, for no further account of it is given, was caused by flea-bites, is stated to have been present in one case where the characteristic spots were absent (p. 89). Another case, equally remarkable, presented "a *combination of the eruptions*" of typhoid and typhus. We might comment on several other descriptions, such, for example, as a case in which "some of the spots were vesicular, the contents of the vesicles being subsequently absorbed;" but in the treatment of a work in which there is so much to commend, and by which its author has proved himself a most earnest and successful student, we shrink from the appearance of hypercriticism.

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[*] It should be held in remembrance that the first accurate description of the eruption in typhoid fever is due to Louis, just as the first accurate and minute description of the typhus rash is due to the Irish physicians.

[†] In the following quotations from Dr. Flint's work the italics are our own.

[‡] The eruption of sudamina, it may be observed, is more frequent in typhoid than in typhus fever. "The elimination of lactic acid from the skin is more permanent and distinct in enteric fever than in typhus. The skin also acquires a very peculiar smooth, satiny feel, and *sudamina* are more profuse, and desquamation more copious and remarkable, in this disease than in the

We need not do more than direct attention to the differences presented by the eruption of typhus, as compared with the description now given of that in typhoid. Dr. Jenner has not unaptly designated the eruption of typhus, "the mulberry rash;" and has further described it as consisting of "very slightly elevated spots, of a dusky-pink colour." The general distribution of the typhus eruption, very frequently covering the chest and abdomen, and often being distinctly visible on the face, the legs and feet, the arms and hands, the toes and fingers, forms a striking contrast with the usually very limited distribution of the rose-spots in typhoid fever.*

According to Dr. Jenner, in forty-three cases of typhus analysed by him, the eruption underwent certain changes in the course of the disease.

> "In one, two, or three days, the spots were no longer elevated above the surrounding cuticle; their hue was darker and more dingy than on their first appearance; their margins rather more, but still imperfectly defined; and now (instead of disappearing completely on pressure, as at first) they only faded on pressure."

Dr. Jenner has observed a further change, which he calls the third stage, in it: "The centres of the spots became dark purple, and remained unaltered by pressure, although their circumferences still faded; or the entire spots, the circumferences as well as the centres, changed into true petechiæ." Now the importance of these distinctions, the fruit of Dr. Jenner's most careful observations, is at once seen, when we consider that by them is exhibited the real difference between the typhoid and typhus, no such changes as the spots in the latter undergo, occurring to those of the former. Dr. Jenner truly says, that on the first day of the appearance of typhus spots, the most tutored eye might be in some doubt as to which order they belonged; but when, after a day or two, some or all of the spots passed into the second stage, the disease of which they were characteristic was proclaimed. So much for the distinctive appearances presented by the eruptions of the two diseases. We are aware of no extended observations which have been published corroborative of Dr. Jenner's; but, on the other hand, we know none which oppose them, while our own limited experience is entirely in accordance with his views.

We have now to consider, secondly, the condition of the abdomen, the

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other."—Dr. Charles Ritchie, of Glasgow, in the Monthly Journal for 1846–47, p. 254, whose careful researches, published in that periodical and in the Glasgow Journal of Medicine for 1854, have materially added to our knowledge of the generic distinctions of typhus and typhoid.

* Monthly Journal, vol. ix. p. 677.

state of the bowels, and the character of the dejections, in typhoid fever, as affording marked distinction between that disease and typhus. So far as symptoms go, it is here that we naturally look for the most complete evidence of that distinction. The names of abdominal and enteric fevers express in a word the character of the disease, which has been regarded as the most expressive. Tenderness of the abdomen, if not a constant symptom in typhoid fever, is at all events a very frequent one. Dr. Jenner found it present in three-fourths of the cases in which accurate examination was made.* In a great number of cases, the tenderness, if not limited to a particular part of the abdomen, the right iliac fossa, is most marked there, and is generally associated with a peculiar gurgling sound when pressure somewhat suddenly is made. In regard to abdominal tenderness and gurgling, Dr Flint writes:

> "Of the 18 hospital cases, more or less tenderness was present in 11. The tenderness was either moderate or slight in all but 3 cases. It is noted as especially marked in the right iliac region in 2 cases, and in both iliac regions in 4 cases.
>
> "*Gurgling.*—Of the cases of typhoid, this symptom was present in 12, and its absence was noted in 2 cases."† (p. 85.)

Again, in his summary of symptoms distinctive of typhoid and typhus fevers, Dr. Flint, in speaking of tenderness on pressure over the abdomen, says, this is "an almost constant symptom in typhoid, and less frequently present in typhus. In the latter, usually slight; and in the former, more apt to be marked, or considerable in degree." (p. 237.)

Dr. Jenner has drawn attention to the peculiar tub-shaped form presented by the belly, which he ascribes to the flatus, which is often in very considerable amount, occupying the ascending, descending, and transverse colon.

In regard to the state of the bowels, Dr. Flint thus sums up the evidence he has collected:

> "Diarrhœa present in one-half of the cases of typhoid, and in one-third of the cases of typhus; in the latter type always mild or slight, but in the former sometimes prominent as a symptom. Hæmorrhage from the bowels, characteristic of typhoid." (p. 237.)

The tendency to looseness of the bowels, whether arising spontaneously or induced by very slight doses of laxative medicine, is a symptom of typhoid fever upon which it is unnecessary here to enlarge. The character of the stools is thus alluded to by Dr. Jenner:

> "The consistence of the stools was watery in twelve cases at some period of the disease, and soft, pultaceous, or almost fluid, in four others. In eight cases only were stools of natural consistence passed, after the patients came under observation, and in five of these the stools were watery during some period of the disease. Their colour varied from pale brown to almost black; when watery they were usually pale, yellowish brown,‡ with a sediment composed of small, solid, yellowish particles."

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* In fifteen out of twenty: in one of the fifteen, however, it did not arise till peritonitis, the consequence of perforation, had taken place. Monthly Journal, vol. ix. p. 820.

† In what proportion of cases this was, cannot be gathered, as Dr. Flint adds, "in the remainder, nothing is said on this point."

‡ A colour owing to which the name of "pea-soup stools" has been not inappropriately applied.

In regard to the occurrence of bloody stools, Dr. Jenner further observes, that seven out of twenty-one patients, particulars respecting whose stools were recorded, passed blood from the bowels; whereas "hæmorrhage from the bowels did not occur in a single case" of typhus.* The really important point connected with the occurrence of these abdominal symptoms during life, is the appearance presented by the intestines, more particularly the glands of Peyer and the mesenteric glands, and not unfrequently associated with this, the deposit of a peculiar exudation in the spleen, and more rarely in other organs. Into the examination of the morphological changes undergone in these organs we do not here propose to enter, for the reason previously assigned. But from the data afforded by our two authors we shall draw attention to the frequency of the intestinal disease in typhoid fever as contrasted with the usually unaffected condition of the bowels in typhus.† In twenty-three fatal cases of fever which Dr. Jenner had an opportunity of examining, the agminated glands, or Peyer's patches, were ulcerated in all. In forty-three fatal cases of fever (typhus), recent disease of Peyer's patches was absent in every one. In regard to the affection of the mesenteric glands, in the same cases of typhoid fever, twenty-three in number, these organs were more or less extensively diseased in all. In the cases of typhus, with two exceptions in which they were the seat of tubercular deposition, the mesenteric glands presented no deviation from a healthy structure. Dr. Flint's remarks on the affection of the bowels are few in number, and are not, we regret to say, capable of conveying a very correct idea of what he did observe.

"Of the fatal cases," writes Dr. Flint, "the bodies were examined with particular reference to the lesions deemed to be characteristic of *typhoid fever* in *twelve.* Of these cases, *nine* had been classed under the head of *typhoid,* judged by their symptoms, and *three* under the head of *typhus.* On reference to the description of the morbid appearances in these cases severally, it will be seen that in each of the *nine* typhoid cases there existed notable changes in the intestinal follicles, accompanied by corresponding degrees of enlargement of the mesenteric glands."

Dr. Flint then proceeds to describe more minutely the follicular lesions; and having done so, makes the following remarks on the appearances presented in the *three typhus* cases:

"In each of the three *typhus cases,* on the other hand, there existed changes in the follicular patches, accompanied in *two* cases by very slight enlargement of the mesenteric glands. The follicular lesions, however, were insignificant in comparison with those observed in the *typhoid* cases. The patches were simply developed so as to be visible. They were slightly hypertrophied, not projecting several lines above, or depressed below the level of the mucous surface, as in the *typhoid* cases, and in no instance presenting an ulcerated appearance. The contrast, indeed, is scarcely less striking than if, in the typhus cases, the follicles had remained invisible. The united number of observations, thus, which are contained in the preceding reports, sustain the existence of characteristic lesions of the intestinal follicles and mesenteric glands in typhoid fever; but they also show that these parts do not always continue wholly unaffected in *typhus*." (pp. 240, 41.)

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* The condition of the urine in typhoid fever has been made a particular subject of study by Dr. G. W. Edwards, and to his interesting paper in the Monthly Journal for September, 1853, we beg to refer the reader. See also, on this subject, a short paper by Mr. Trotter, in the Lancet, for 1854. In 1852, the writer of this article expressed the opinion, founded on personal observation, that albuminuria would be found a frequent concomitant of typhoid fever, and specially occurring in the advanced stages of the disease. See Monthly Journal for 1852.

† It must be observed (and all who are familiar with Dr. Jenner's original papers will remember), that the affection of the agminated glands was the feature by which he distinguished the typhoid from the typhus cases, and therefore the facts he brought forward, and which are mentioned above, are not to be looked upon as affording any new ground of distinction, seeing that the existence of the intestinal lesion was observed by Dr. Jenner before the cases of typhoid fever were classed as such.

If they do not, and if Dr. Flint's three post-mortem examinations reveal that they did not, we must confess that the proof he has brought forward to establish this fact is altogether inadequate. Dr. Flint must have forgotten that Peyer's patches are not such invisible things as he here appears to regard them. It is by no means uncommon to see them clearly marked in persons who have died from diseases altogether unconnected with the intestines, and as a general rule in young subjects (and the mean age of Dr. Flint's typhus cases is stated at 26¼) the agminated glands are distinctly recognisable. We cannot then agree with Dr. Flint, but, on the contrary, think that the facts he has adduced fail in showing "that these parts do not always continue wholly unaffected in typhus."

Having thus briefly adverted to the chief differences in the course, symptoms, and lesions of typhoid and typhus, it remains for us to direct attention to the view which has been offered by Dr. Jenner, from inquiries as admirably planned as they have been efficiently carried out, of the essential difference in the specific cause of typhoid, typhus, and relapsing fevers.

We have not thought it necessary to offer any detailed account of the course and symptoms of relapsing fever. It is thus sketched by Dr. Jenner:

> "Sudden rigors, headache, skin hot and dry, tongue white, urine high-coloured, bowels regular, occasional or frequent vomiting, loss of appetite, absence of abnormal physical abdominal signs. In severe cases, jaundice, profuse sweating on about the seventh day, followed by apparent restoration to health; on, from the fifth to the eighth day, reckoning from the apparent convalescence, repetition of the original symptoms, with greater or less severity, again terminating in sweating, and then permanent convalescence."

To this may be added the frequent occurrence of epistaxis at the period of crisis.

In Edinburgh a second relapse was not unfrequent, and even a third has been observed. This form of fever has been twice epidemic in Edinburgh during recent years—in 1843 44, and 1846-47.* The probable existence of this form of fever during upwards of a century is shown by Rutty in his 'History of the Diseases of Dublin,' who describes a non-malignant fever of six or seven days' duration, terminating in a critical sweat, but followed by one, two, three, in some cases even four, relapses—yet recovering. In 1800 and 1801 there was an epidemic of a similar fever in Ireland. Barker and Cheyne, in their Reports, and Dr. Welch, in his work 'On Bloodletting,' prove the existence of a similar fever in 1816, 1817, 1818, 1819, and 1820; while Dr. Christison shows the probable identity of the fever he so carefully studied in 1826 with the fever described by Welch.*

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* See Dr. Cormack's Account of the First Epidemic. London. 1843. Also Dr. Halliday Douglas in Northern Journal of Medicine, for 1845; and for account of the latter see Dr. William Robertson, in Monthly Journal, for 1848; and Dr. Robert Paterson, in the Edinburgh Medical and Surgical Journal for the same year.

The following are the facts, summarily expressed, of Dr. Jenner's induction, which we shall presently notice:—In the year 1848,† one-fourth of the cases admitted into the Fever Hospital had typhoid fever; while from 34 foci of typhus fever, yielding 101 cases, there was brought to the hospital once only a case of typhus fever and a case of typhoid fever from the same house; and during the same time, among five localities, affording 9 cases of typhoid fever, one locality only—viz., the house from which a father and son were brought—yielded a case of typhoid and typhus fever. In 1849, although eighteen foci of typhus fever yielded 51 cases, and four foci of typhoid fever afforded 10 cases, not a single example of the two diseases being received into the hospital from one house occurred. The facts of the exceptional case, which happened in 1848, were the following: —A man, aged 46, was admitted October 10th, 1848, with well-marked typhus fever, while his son, aged 16, who had been received into the hospital on September 19th, laboured under equally well-marked typhoid fever. The mother of the boy, however, had visited him in the hospital, and therefore might have carried the contagion of typhus fever to her husband. The father, moreover, had been little exposed to the contagion emanating from the son, because the latter, a vagabond, at variance with him, was from home when the father was taken sick. We feel more disposed to agree with Dr. Jenner in the following observation in regard to such exceptional cases as the one detailed, than to attach much weight to the circumstances either of the mother visiting the boy in the hospital where there were typhus cases, or, though we regard it as of more importance, the father being little in the company of his son. Dr. Jenner holds that exceptional cases "must be met with more frequently than similar exceptional cases are met with in diseases having a specific cause, universally acknowledged to be different," before they can be of any value in proving the identity of typhus and typhoid fevers. It has occurred to Dr. Jenner, within three years, to see a case of typhus brought to the hospital from a house where all the children were suffering from measles. Another case of typhus, brought from a house in which the children had scarlet fever—a girl admitted with scarlet fever who had been on terms of intimacy with another girl, admitted shortly before with typhoid fever.

In April, 1849, a girl suffering from relapsing fever was brought from a house in Fulham; in a few days her brother and her sisters were admitted into the hospital. At this time typhus was the prevailing disease, and typhoid was much more widely spread than relapsing fever, but all had the same fever.

Judging, then, from such facts, Dr. Jenner arrives at the conclusion— and we confess that, with the confidence we have in the accuracy of his observations, we feel committed to the same—that the specific causes of typhus, typhoid, and relapsing fevers are absolutely different from one another:

"If," says Dr. Jenner, "small-pox be separated from measles, and both from scarlet fever, because their course, symptoms, lesions, and specific causes are different, so must, for like reasons, typhoid fever, typhus fever, and relapsing fever be separated from each other, and regarded as absolutely distinct diseases, not merely varieties of each other, as scarlatina anginosa and scarlatina sine eruptione, but distinct species of disease, as are scarlatina, rubeola, and variola."

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* See Dr. Jenner's Gulstonian Lectures, p. 43; the British and Foreign Review for 1851; and Dr. Wood's Lectures, vol. ii. 361.

† Medico-Chirurgical Transactions, vol. xxxiii. p. 40.

The historical bearings of the subject we have considered, deeply interesting as they are, being altogether too immense for our present consideration, we beg to refer the reader specially to Dr. Stewart's admirable paper for a key to its vast literature; and to the very able articles on fever in this Review for 1841 and 1851, as well as to the last chapter in Dr. Jenner's 'Gulstonian Lectures.' It must not be forgotten that the authors of three countries—of France and America, besides our own—have added imperishable monuments to their talents and labours in the cultivation of this most interesting subject. Of the researches of Dr. Flint, the latest American observer, as embodied in the work we have had under our notice, we are very glad to express a high opinion. Not free from confusion in some particulars, from causes its author indicates in his preface, Dr. Flint's Report contains a body of most useful facts, and for the most part very accurate deductions from those facts, which are equally honourable to Dr. Flint and to the medical school of which he is so distinguished a teacher.

J. Warburton Begbie.

Review X.

The Obstetric Memoirs and Contributions of James Y. Simpson, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, &c. Edited by W. O. Priestley, M.D., Edinburgh; and Horatio R. Storrer, M.D., Boston, U.S. Vol. I.—*Edinburgh*, 1855. pp. 857.

The republication of the obstetric writings of Dr. Simpson in a complete and accessible form was an undertaking no less due to the reputation of their author than to the interests of obstetric science. Few men have laboured more assiduously in this department of medicine, and few have contributed more largely to its scientific advancement by the publication of various original papers and practical observations. It was right that the results of such labours should be rescued from the perishable record of the various serial publications in which they originally appeared, and be presented in an authorized and collective form to the notice of the profession. The present volume supplies, to a certain extent, this desideratum; and without assuming that all the views which are contained in it will receive general assent, believing rather that many are both unsound in doctrine and unsafe in practice, we are yet convinced that it will be received as a valuable boon by the profession, and consulted by all who are interested in the progress and advancement of obstetric science.

The subjects treated of in the present series are arranged under three heads: the first comprising forty-three papers On the Special Pathology of the Unimpregnated Female; the second, five upon the Physiology and Pathology of Pregnancy; and the third, thirty-three On Natural and Morbid Parturition. It should be observed that many consist of a few curt and extempore observations only, whilst others are extremely elaborate. As it would be impossible to bring all the topics thus treated of before our readers, within the necessary limits of this article, we propose to confine ourselves to an examination of some of the more important papers, selecting those points which especially distinguish the views and practice of their author.

In the first, the subject of the general diagnosis of uterine disease is very fully considered, and this is well worthy of attentive perusal, not only because all the elements of such diagnosis are very lucidly set forward, but also because it furnishes a clue to many of those views respecting uterine pathology upon which so much of the author's peculiarity of practice is founded. He treats of the subject under two heads—the first comprising the *symptoms*, and the second the *signs* of uterine disease; the former are referred to the five following sources or varieties of information:

1. *Derangements in the Functions and Vital Condition of the Uterus itself*, as indicated by the quantity, character, periodicity, &c., of the menstrual and mucous secretions of the organ; by the occurrence of morbid uterine or vaginal discharges—as blood, serous fluid, pus, &c.; by the existence of morbid sensations in the region of the uterus—such as different modifications of pain, intermittent or continuous, feelings of heat, weight, tension, bearing down, &c.; and if the patient be married, by the reproductive powers being affected, as shown by sterility, the recurrence of abortions, premature labour, and the like.

2. *Dynamic Symptoms in neighbouring Pelvic Organs*—more especially affections of the rectum and bladder, and of branches of the vessels and nerves passing through the pelvis. In this division are included pains about the bladder or rectum, in the groins, along the crest of the ilium, and along the course of the crural and sciatic nerves; intermittent pains in the lower part of the abdomen; derangement of the functions of the rectum or bladder, producing either constipation, or difficult or painful defæcation on the one hand, or too frequent micturition, dysuria, retention or incontinence of urine, on the other.

3. *Sympathetic Pains in different and distant Parts of the Body*—including pain in the mammæ, along the lower extremities; in the loins, and at points along the course of the spinal column; in the parietes of the thorax or abdomen; on one or other side, and especially under the left breast; along the course of the colon; under the margin of the ribs; increased by any causes which tend to an increased action of the uterus, such as the erect position, menstruation, &c.

4. *Derangements of Functions in Distinct Organs*, including affections of the kidneys, intestinal canal, liver, lungs, nervous system, and skin.

5. *States of General Constitutional Derangement.*—These comprise more particularly febrile, cachectic, and anæmic conditions.

The above may be regarded as the principal symptoms, local, sympathetic, and constitutional, of uterine disease; and the whole as comprising its chief symptomatology in the widest acceptation of the word. But there is this evident error or omission in the review of these morbid phenomena, that no attempt is made to determine their absolute relations to the diseases in question—whether, in reality, they are specifically symptomatic of primary disease of the uterus, or of abnormal conditions affecting distant parts, or the economy at large, of which the uterine affection is but a secondary and subordinate feature. That the symptoms in question are very commonly associated with uterine disease we do not doubt; but that they are specifically its consequences in all cases, is a point we are prepared to contest. We have devoted some time to the determination of the relations which subsist between uterine ailments and various remote and constitutional affections, and the result has convinced us that the supposed symptoms and consequences of uterine disease are in many cases to be regarded rather as their causes or antecedents. We would instance in particular morbid states of the nervous system, the blood, and the digestive organs; and we are fully persuaded that the category might be extended. The analysis we have therefore given of uterine symptomatology, founded upon Dr. Simpson's views and experience, must be understood as comprising associated rather than contingent derangements, and a wide range of morbid actions not necessarily dependent upon the uterine ailment.

The signs of uterine disease, or rather the means by which we are enabled to discriminate or detect them, are given in the following summary of the author's:

1. The external or abdominal examination of the patient by sight, touch, auscultation, and percussion.
2. The tactile examination of the uterus, ovaries, &c., by the vagina or by the rectum.
3. That most important mode of diagnosis, viz., the simultaneous combination of the external and internal modes of tactile examination.
4. The use of the speculum.
5. The use of the uterine sound.
6. The use of sponge-tents, with the view of dilating the os uteri, so that the finger can be introduced into the cavity of the cervix or cavity of the body of the organ.
7. The microscopic and chemical examination of the discharges from the uterus and vagina.
8. The employment of the exploring needle in cases of fluid collections, in order to ascertain the contents of such collections; and,
9. The adoption of anæsthetic agents to relax the abdominal parietes, and enable us to practise the different modes of examination, in cases of excessive or neuralgic tenderness of the abdominal surface or vagina, &c.

We believe, as stated by the editors, that the profession is indebted to Dr. Simpson for the introduction of four of these means of diagnosis, viz., the sound, sponge-tents, the exploring needle, and the production of anæsthesia to facilitate examination; and we advert to the circumstance as showing the extreme, and as it seems to us undue, importance attached by him to this mode of investigation—a circumstance which is indeed attested by the general tenor of the paper before us. We are, for instance, repeatedly reminded of the uncertainty and insufficiency of rational symptoms in the discrimination of uterine disease. We are told that by these means we may indeed determine its existence, but not its nature; that by physical exploration alone this object can be attained; that improved physical means of diagnosis are still so much required; and that it is only through these that our knowledge of uterine pathology can be really advanced. It is not difficult to perceive that an undue preference for physical means of diagnosis should lead to a similar preference for physical methods of cure; and thus we may deduce the origin and introduction of those peculiar mechanical principles of treatment which constitute the leading characteristics of the school of which Dr. Simpson is the acknowledged head, and in regard to which professional opinion has of late been so much divided.

But whilst we concede the importance of carefully investigating the physical changes of an organ affected by disease, or, in other words, the value of physical diagnosis, and would neglect no means by which this object can be safely and efficiently accomplished, we would yet observe, that there are some limits to its application and utility. It is quite true that by physical means we may become readily acquainted with the physical changes which have taken place in an organ, but what we desire to attain to for the purposes of accurate treatment, is not so much a mere knowledge of the lesions of structure which have been effected, as a knowledge of the conditions under which they arise, their course, tendency, and relations to coincident phenomena. When, for instance, with the aid of the speculum or uterine sound, we have discovered the existence of an excoriated cervix or a retroflexed fundus uteri, we have in reality but commenced our inquiry, and without carrying our investigations further, very erroneous views will result both in regard to doctrine and practice. Thus, from the frequent coincidence of redness with abrasion of the cervix, it was assumed that the latter lesion was necessarily of an inflammatory character—was spoken of as inflammatory ulceration, and said to require the usual treatment for inflammation; whereas it can be shown that the lesion in question not only occurs without any redness, or any other sign of inflammation, but would be aggravated by a resort to treatment adapted for inflammatory conditions. So also it was assumed, from the frequent coincidence of this lesion with various functional derangements of the uterus and disordered states of the general health, that it stood in the relation of cause to these affections; whereas it may be shown that such lesions do very commonly exist, not only without any remarkable derangement of the functions of the uterus, but also without any unfavourable reaction upon the general health. Again, with reference to the discovery of a retroflexed condition of the uterus, it by no means follows that we are thence enabled to deduce its real nature and pathology; and accordingly, under erroneous impressions, a practice has sometimes been adopted altogether at variance with sound physiological and pathological doctrines. We could extend the number of examples to show that there is a limit to the value of mere physical diagnosis, and that the aid of dynamic or rational symptoms must be brought to our assistance before we can attain to a right knowledge or apprehension of the nature of the lesions thus revealed.

The tendency of modern researches in uterine pathology, based almost entirely upon physical evidence, is forcibly and accurately depicted by Dr. Simpson himself; and his remarks on this subject form a singular corollary or contrast to his oft-repeated maxim, that "The medical science of the present day owes its superiority over that of an earlier date, to no circumstance more than to the increased attention that has for a considerable time past been directed to the study and improvement of physical diagnosis," and that it is from this that so much is to be expected.

> "Since the diseases of the uterus and its appendages have of late years attracted so much more the attention of the profession than they formerly did, one grievous error," he observes, "has been committed in their study and investigation. The error I allude to is the error of exclusiveness. Formerly many practitioners seemed to look upon all diseases of the uterus as diseases indicative of debility; and they treated almost every one of them with muriate of iron and other chalybeate preparations, sometimes adding, where there was any discharge, the local employment of astringent or tonic injections. I fear that even yet you will find some old practitioners treating their uterine cases upon this sole principle. Then a second set of pathologists would, if we may judge from their writings, seem to suppose that all diseases of the uterus are marked by, if they do not consist of, congestion and engorgement of blood; and that they are to be remedied by the remedies applicable to their state. A third set, again, look upon the general run of uterine cases as almost invariably inflammatory in their nature, and imagine that we are sure, or almost sure, to find in every case inflammation, or some of its results, as ulceration, purulent discharges, &c. A fourth set would seem to fancy all uterine ailments to be produced by some mechanical displacement or dislocation of the uterus, and to consist of prolapsus, versions, and flexions of this organ upon itself and upon the neighbouring parts. Again, there are some practitioners—one in particular, in immense practice in America—who would appear to believe that the affections of the uterus are fundamentally nervous or neuralgic disorders, and that they are always to be treated by the local inunction upon the cervix uteri of morphia, aconite, and similar sedatives. Another section of pathologists imagine the so-called uterine diseases are, after all, not uterine, but ovarian; and that ovarian irritation and inflammation is actually the source and origin of much of the suffering that is imagined to be uterine in its seat. Lastly, for it is needless to extend this tedious enumeration, you will find another set enjoying the belief, that these supposed uterine or ovarian diseases are not at all uterine or ovarian in their origin, but in reality diseases of the general system; they may not deny that local affections do occur in the uterus and ovaries, but these local affections are, in their opinion, results and effects of some more general or constitutional disease of the nervous system or of the economy at large." (pp. 3, 4.)

Let it be added, that these remarks apply to doctrines which have become rife since the physical exploration of the uterine organs has been so much resorted to by medical men, and if true, they are calculated to shake our confidence in the sufficiency of the means to the end proposed; to show that its value may be over-rated and its revelations misinterpreted; and that the mere recognition of facts, without a just perception of their practical relations, can add little but doubt and confusion to our existing knowledge of uterine disease.

From the very elaborate memoir on the uterine sound, we extract the following summary of the chief purposes which, in the opinion of our author, the instrument is calculated to fulfil:

1. The sound increases to a great degree our power of making a perfect and precise tactile examination of the fundus, body, and cervix of the uterus, by enabling us to bring these portions of the organ successively into the most convenient position for external or internal examination or manipulation;

2. The previous introduction of the instrument facilitates and simplifies the subsequent visual examination of the cervix uteri with the speculum

by lessening the difficulty which sometimes arises of catching the os and cervix uteri in the upper or internal extremity of the instrument;

3. By its use we may, in many instances of pelvic and hypogastric or abdominal tumours, ascertain the connexion or non-connexion of these tumours with the uterus. Thus, when the tumour is uterine, the instrument, when passed into the uterine cavity, enters, as it were, more or less into the very structure of the morbid mass, and the tumour and instrument afterwards reciprocally move in exact correspondence with each other. When, however, the tumour is not uterine—(1,) the uterus may be retained in its situation with the sound, and then, by means of the hand above the pubis, or by some fingers in the vagina, the tumour, if unattached to the uterine tissues, may be moved away from the fixed organ; or (2,) the tumour being left in its situation, it may be possible to move away the uterus from it to such an extent as to show them to be unconnected; or (3,) instead of keeping the uterus fixed and moving the tumour, or fixing the tumour and moving the uterus, both may be moved simultaneously, the uterus by the bougie, and the tumour by the hand or fingers, to opposite sides of the pelvis, to such an extent as to give still more conclusive evidence of the fact;

4. The uterine bougie is capable of affording valuable diagnostic information, by enabling us to measure the length of the uterine cavity, as in the following cases, in which it is abnormally increased: (1.) Morbid permanence of the state of puerperal hypertrophy; (2.) Normal elongation of the puerperal uterus as a sign of delivery; (3.) Increased length in metritic and congestive hypertrophy of the body of the uterus; (4.) Longitudinal hypertrophy of the uterus, and especially of the cervix; (5.) Enlargement of the uterus and uterine cavity from the growth of fibrous tumours in the parietes of the organ; (6.) Enlargement and distension of the uterus from polypi; (7.) Elongation of the uterus in hernia of the organ. The uterine bougie affords information in the following cases, in which the uterine cavity is abnormally diminished: (1.) Preternatural shortness of the uterus from original malformation; (2.) Shortening of the uterine canal from stricture or partial obliteration; (3.) Diminution of the depth of the uterine cavity from inversion of the organ.

We have endeavoured to condense into the preceding paragraphs the more important uses of this instrument, as set forward in the text; but we are bound to add that so far as our experience of it has extended, it has not altogether realized the expectations we had formed of its value. We do not, however, participate in the opinions of those who regard it as a useless and a dangerous instrument, and would discard it altogether from practice. We have no doubt that judiciously employed, it is useful in the diagnosis of many obscure forms of uterine disease; but at the same time we are equally persuaded that its employment requires much care, and that the range of its utility is more limited than that claimed for it by the author. In our own practice we have experienced the following difficulties in its employment:

1. Although great experience may give facility in the introduction of the instrument, it is yet an operation by no means so easy of performance as is commonly represented. In cases in which the cervix is high up, as also in those in which the os uteri is small, or the vagina constricted, we have found great difficulty in its introduction by merely manual pro-

ceedings. In the first of these cases we have found it difficult to insert its point into the os uteri when guided by a single finger, on account of the mobility of the organ; and when two fingers have been passed into the vagina, for the purpose of steadying it, we lose the guide which is afforded by the application of a single finger to the os uteri. Under these circumstances we have found it more convenient to bring the cervix into view by means of the speculum, and to introduce the instrument by the aid of vision, rather than of touch. In this way there is seldom any difficulty in introducing it; and by varying the axis and direction of the speculum, so as to follow the necessary movements of the instrument, we have been enabled to employ it with considerable advantage;

2. We have occasionally experienced some difficulty in passing it into the body of the uterus after it has entered the cervix, either from some constriction at its distal extremity, or from its becoming entangled in the lacunæ or rugæ of the lining membrane of the cervix. We apprehend that in such case it must be difficult to determine the exact cause of the arrest, whether, for instance, from obstruction, constriction, or inflexion. In any case, however, it must be obvious that violence would be unjustifiable, and hence we prefer to forego the advantages of the instrument, and to be satisfied with negative evidence, rather than incur the risk of injuring or wounding the interior of the uterus;

3. We have met with cases in which the most careful use of the sound occasioned extreme pain and suffering, and was followed by persistent hæmorrhage. Other dangers, such as contusion, laceration, and injury of the uterine parietes, have been known to follow its incautious or forcible employment; and therefore, without any wish to depreciate its value, we feel it necessary to qualify our recommendation of it with the warning that great care and judgment are necessary in its employment.

Passing over several minor papers upon the Use of the Exploring Needle in the Diagnosis of Doubtful Forms of Pelvic and other Tumours—Anæsthesia as a means of Diagnosis—Inflammatory Eruptions upon the Mucous Membrane of the Cervix Uteri—Medicated Pessaries—Chloride of Zinc in Ulceration of the Cervix Uteri—Potassa Fusa in inflammatory Induration of the same organ (which we regard as very questionable practice)—Morbid Deficiency and Morbid Excess in the Involution of the Uterus after Delivery; and two papers upon Fibroid Tumours of the Uterus, we come to three interesting communications upon Polypi of the Uterus, of which the principal points mooted are the following:

1. The employment of the uterine sound as a means of diagnosis in the case of polypi growing from the lips of the cervix uteri. It being contended that by this we can best determine the position of the os, and the direction of the uterine cavity to the abnormal growth; and although it is admitted that the use of the instrument is difficult, and requires considerable care in these cases, yet it is believed to be capable of furnishing information which would amply repay the overcoming of any difficulties which might be met with in its employment.

2. The employment of sponge tents for the mechanical dilatation of the cervix uteri in cases of intra-uterine polypi to such an extent as to admit of the introduction of a finger into the uterine cavity, for the purpose of exploration and diagnosis, a practice which is very fully described and illustrated; and,

3. The advantages of the excision of large pedunculated uterine polypi over deligation; which the author regards as being, in many respects, a safer operation, one which upon the whole is more easily performed, the cure by it being infinitely quicker, and admitting of being accomplished with far less restraint and annoyance to the patient, with less risk of local irritation, and with less ultimate chance of actual peril to health and life.

With regard to the general questions raised in these very practical papers, we would venture to express our opinion, that the suggestion ot the employment of sponge tents for the purpose of exploring the interior of the cervix and body of the uterus in certain cases, is one of great value, the operation being comparatively harmless, and for the most part unattended with much pain or suffering. In giving the symptoms of intra-uterine polypi, the author enumerates menorrhagia, recurrent, almost constant, and periodical; leucorrhœa, of a mucous, purulent, or serous character; increased size of the cervix and body of the uterus, from the presence and distension of the polypus, and sympathetic irritations in the bladder, rectum, and distant organs; but he omits to mention what we believe is often an important symptom of the disease—viz., periodical pains or contractions of the uterus, not dissimilar to those that occur in abortion, and which are evidently occasioned by the efforts of the uterus to get rid of the abnormal body in its interior. He also omits to make any reference to the employment of galvanism, either for the purpose of stimulating the uterus to expel the polypus, or of moderating any attendant hæmorrhage. We are aware that Dr. Simpson's trials of galvanism, for the purpose of exciting uterine action during labour, were not such as to lead him to think favourably of its powers. This, however, is a question to which we shall hereafter revert, and in the meantime we would remark, that we have strong grounds for believing that it may be usefully employed in the treatment of polypus uteri for the fufilment of both of these indications.

On the subject of the excision of large polypi, we would observe, that whilst concurring with the author in a full appreciation of the dangers to be apprehended from sloughing and suppuration consequent upon deligation, we should yet feel equally apprehensive in regard to the dangers of hæmorrhage as a probable consequence of excision. Dr. Simpson very justly draws attention to the liability of phlebitis and surgical fever supervening upon the use of the ligature, as a consequence of the absorption of the ensuing purulent discharges. But it is to be remembered that an empty or exsanguine state of the bloodvessels powerfully predisposes to the same results; and, therefore, in taking a comparative estimate of the merits and demerits of these respective operations, the fact must not be lost sight of, that the conditions in question very commonly occur in an inverse ratio in the two, and that the greater risk of hæmorrhage after excision forms, in some degree, a set-off to the increased suppuration and sloughing after deligation; whilst the dangers of the latter may be somewhat obviated by the use of antiseptic injections. We are certain that the liability to phlebitis, puerperal fever, and various secondary affections, is infinitely greater in those cases in which profuse hæmorrhage has followed upon mechanical injury, or difficulty during labour, than in those in which, without it, the same amount of injury had been received, and accordingly we have felt it necessary to watch cases with greater vigilance whenever this circumstance has occurred. Believing, then, that the operation of excision, as ordinarily practised, is fraught with great danger, from the risk of consecutive hæmorrhage, but otherwise is far safer and better than that of deligation, we would wish to consider whether it might not be practised in a manner that would obviate this consequence. We have no practical facts to offer on the subject, but have been led to think that some modification of the electric cautery might be substituted for the knife, or that the object might be attained by the torsion of the polypus previously to its division. It is quite certain that the risk of hæmorrhage is the great objection to excision, and that any plan of proceeding by which this might be obviated or lessened, would add very considerably to the safety of its performance; until this desideratum however has been attained, we fear that the advantages of the excision over the deligation of large uterine polypi will be found to be less considerable than that claimed for it by Dr. Simpson.

Two papers follow upon Amputation of the Cervix Uteri. In the first, the history of a case of cauliflower excrescence from the os uteri is given, in which the operation was successfully performed, and to it are appended some remarks upon the pathological nature of the affection. In the second, three cases are reported, in which excision of the cervix uteri was performed, and these are followed by some general remarks upon the operation in carcinomatous disease. The subject of malignant disease of the uterus is further illustrated by two additional papers—the one Upon the Occasional Latency of the Symptoms in Advanced Carcinoma Uteri, and the other On Carcinomatous Disease of the Cavity, Body, and Fundus Uteri. We will commence the consideration of this very important series by an examination of Dr. Simpson's opinions respecting the pathological nature of the cauliflower excrescence.

> "The pathological nature of this variety of morbid growth," he observes, "has given rise to a considerable difference of opinion among physicians. Drs. Gooch, Hooper, Davis, and Lee regard it as truly cancerous in its character; others—as Drs. Clarke, Burns, and Waller—consider it as a morbid tissue, not necessarily of a malignant or carcinomatous nature. A number of circumstances appear to me to show that, in reference to at least the first stage of cauliflower excrescence, the opinion of these latter authors is probably correct. The occurrence of the disease in some cases as early as the twentieth year of life; its occasional shrinking and almost total disappearance upon the application of a ligature, or after death; the frequent slowness of its general progress during life; the apparent absence of diseased deposits in the neighbouring tissues and parts upon the dead body; and, above all, the alleged restriction, and even complete removal, of the tumour, in one or two instances, by the use of astringent applications and other simple means, form so many circumstances strongly pointing to the opinion that, in the earlier part of its progress, the tumour cannot be regarded as of a carcinomatous character." (p. 168.)

The view thus expressed of the non-malignant nature of the disease is, however, somewhat at variance with the following remarks, which occur almost immediately after the preceding:

> "But whatever view we may take of the primary nature of the cauliflower excrescence of the cervix uteri, we havé sufficient evidence for believing either that

this disease has been often confounded with carcinomatous or medullary fungus from the cervix uteri, from the want of adequate diagnostic marks to distinguish them; or that, though non-malignant in its commencement, the cauliflower excrescence may, like some other local benign growths, become the seat of carcinomatous deposit and malignant action during its progress. Thus it has been found by Gooch and Madame Boivin to return again in a malignant form, after its imperfect removal by the ligature or knife. In an instance mentioned by Dr. Davis, its removal was followed, after the lapse of a considerable period, by its reproduction, and ultimately by carcinomatous ulceration; and in two cases that occurred to Professors D'Outrepont and Siebold, in which large tumours having a cauliflower form were found affixed to the cervix uteri during parturition, the neighbouring uterine tissues, as well as the contiguous structures of the bladder and uterus, were found in a carcinomatous state upon the post-mortem dissection. In another case, in which Michaelis excised what he terms a *fungus medullaris* with a cauliflower appearance, from the anterior lip of the uterus during labour, the posterior lip of the organ afterwards degenerated, and cancer of the stomach ultimately supervened."

The opinions expressed in the two preceding paragraphs respecting the pathological nature of the cauliflower excrescence are, we venture to think, somewhat incongruous, and as such are scarcely tenable in the present state of pathological science. We believe that Dr. Simpson is in error in regarding the disease in any of its stages as non-malignant, and are of opinion that all its varieties are essentially referable to two forms of cancer—the epithelial and the medullary. Probably it would be more correct to restrict the term to the former variety alone, to which in its *genuine* forms its anatomical affinities are closest. By doing so, the discrepancy in the views of Dr. Simpson would be reconciled, and the malignant and non-malignant forms or stages of the disease described by him would be found to be strictly accordant with the ordinary developments of this form of cancer. We may observe that Virchow has described three kinds of papillary tumours of the os uteri—the simple, the cancroid, and the medullary; but the two first—to which the term cauliflower excrescence should, we think, be restricted—have been shown by Mr. Paget to be but the different stages of the same disease—viz., epithelial cancer.

He remarks, with reference to the opinions of Virchow, that—

"It is evident from his description that the cauliflower excrescence, in the two conditions distinguished by him, illustrates the usual history of the most exuberant epithelial cancers; it might," he adds, "be taken as the principal example of the group. That which he calls the '*simple papillary tumour*,' is an excessive papillary outgrowth of epithelial cancer; the later stage of the same, when it passes into '*cancroid*,' is the usual extension of such a cancer into deeper parts—a continuous growth of the same thing in a new direction; for the papillary structures—composed, as Virchow says, of epithelial cells with bloodvessels and a very little connective tissue—are the essential characters of the epithelial cancerous outgrowths; and I believe that the same composition has never been seen in any papillary or warty growths that did not, if time were allowed, proceed to the formation of epithelial structures in the deeper parts, and thence through the usual progress of malignant disease."*

We believe that these views are essentially correct: that the cauliflower excrescence is but an epithelial cancer of the cervix uteri, and as such, malignant in its nature throughout, although the more evident indications of such malignancy are only manifested in the later stages of its progress. By bearing in mind this fact, and the ordinary course of epithelial cancers, we are enabled to reconcile the very incongruous views respecting the nature of the disease which have been put forward by different writers. In its earliest stage it appears as a simple papillary tumour, without any sign of malignancy, and as such has suggested the opinion that it was of a simple, benign nature. In its further progress, however, those characters become superadded which leave no doubt of its malignancy, and thus, according as the disease is studied or observed in either of these stages, opinions have varied as to its pathological nature.

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* Lectures on Surgical Pathology, pp. 451, 452.

In discussing excision of the cervix uteri, Dr. Simpson states that the following forms of disease furnish cases for the operation:

1. Great morbid hypertrophy, by elongation of the vaginal portion of the cervix uteri.
2. Corroding ulcer, when limited to the lips of the cervix, and pathologically identical with the form of lupus or malignant ulcer so well known on the face; and,
3. Circumscribed and local forms of carcinomatous disease, or excrescence of the lips and lower segment of the cervix uteri.

We think this series is somewhat too extended. Cases of simple hypertrophy of the cervix would, in our opinion, very rarely require so severe an operation; and with regard to corroding ulcer of the uterus, it appears to us that its generally insidious and irregular progress would render it very seldom amenable to operative treatment. At the present time we have a case under our care, in which, before the patient was aware of having any serious uterine disease, the posterior segment of the cervix was found to be entirely eaten away, as well as a portion of the corresponding part of the body, whilst, at the same time, the anterior part of the cervix was still partially intact. We think, therefore, that the question of the excision of the cervix uteri is practically limited to cases of cancer, or rather, we would say, to one form of cancer—viz., the epithelial or cauliflower excrescence, and for the following reasons.

Regarding the three principal forms of cancer which are met with in the uterus—viz., the scirrhous, the epithelial, and the medullary—we apprehend that there are very cogent reasons, founded upon their pathological history, against the performance of the operation in cases of either the first or the last; and there can be no doubt that many of the unsuccessful cases, in which it is said to have been adopted for the removal of cauliflower excrescence, were really cases of medullary cancer. Restricting the term cauliflower excrescence, however, to epithelial cancer of the cervix, we would observe that there are many circumstances deducible from the general history of this form of cancer, which we think would justify the performance of the operation at an early stage of the disease. It is the peculiarity of this variety to remain long in a local form, and to be attended with the fewest symptoms of constitutional contamination. In the words of Mr. Paget—

> "Among all the cancers, the epithelial present the general or constitutional features of malignant disease in the least intense form. They commence at the

latest average period of life; they appear to be most dependent upon local conditions; they are least prone to multiplication in internal organs, and they are associated with the least evident diathesis or cachexia."*

We would submit that these circumstances are sufficient to justify our resorting to the operation in certain cases, and the success of Dr. Simpson in the one he reports, is calculated to give us additional encouragement.

A re-perusal of the very remarkable memoir On Retroversion of the Unimpregnated Uterus, has only served to convince us that, whether regarded with reference to its literary merits or practical tendency, it is one of the most specious and least satisfactory of all Dr. Simpson's obstetric contributions. The leading doctrines and practical deductions put forward in it may be summed up in the following propositions:

1. That the disease had generally been considered to be rare, merely in consequence of the want of a proper and easy means of detecting it; but that since the author had made this discovery, he had found it to be one of the most common and frequent displacements and affections of the unimpregnated uterus;

2. That the functional symptoms of this lesion are of a very varied and uncertain character, sometimes being either few or altogether absent, and sometimes extremely severe and distressing. That when present, they are more or less perfect imitations of the secondary phenomena of pregnancy, such as dyspeptic and hysterical symptoms, neuralgic pains in the mammæ, in portions of the vertebral column, or in the parietes of the chest or abdomen. Mechanical irritations and symptoms in neighbouring organs, more especially of the rectum and bladder, giving rise to constipation or impeded defæcation on the one hand, or to dysuria, retention, or incontinence on the other. Weight, tension, and bearing down in the regions of the uterus and rectum, with dragging at the loins and in the regions of the uterine ligaments. Pains stretching down one or both lower extremities. Inability to bear carriage exercise, whilst walking and standing speedily produce fatigue. In some cases menstruation is not morbidly altered, but in others it is either suppressed, painful, or excessive. Leucorrhœa is sometimes absent, and at other times present; and when pregnancy occurs, abortion is apt to take place; but in some cases the uterus is spontaneously rectified, and utero-gestation proceeds to the full time. Usually it interferes with the function of conception, and is often a cause of sterility;

3. That with regard to its physical signs, the employment of sight by means of the speculum in no respect assists our diagnosis; but that, on tactile examination, a solid tumour is to be felt behind the cervix uteri, between the uterus and rectum, and the same firm mass may be felt through the anterior wall of the bowel, in making an anal examination; and that the most simple and certain method of diagnosis consists in the introduction of the uterine sound, which, by following the curvature or reflexion of the uterus, can be made to enter and traverse the centre of the supposed tumour;

4. That the most frequent abnormal conditions with which it is apt to be confounded are—a. Pregnancy. b. Fibrous and other tumours in the

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* Lectures on Surgical Pathology, vol. ii. p. 457.

posterior wall of the uterus. c. Ovarian tumours in their earlier stages. d. Pelvic cellulitis. e. Extra-uterine conceptions lodged between the uterus and rectum. f. Organic disease in the anterior wall of the rectum. g. Stricture of the rectum. It is scarcely necessary to add, that the principal means relied upon for diagnosis is the introduction of the uterine sound.

5. That the treatment should comprise the three following indications: *a.* The removal, if necessary and possible, of any morbid action in the uterine structures that may exist along with the displacement. *b.* The restoration of the uterus to its normal situation. *c.* The use of means to retain it in its replaced and natural position. The first indication points to the removal of congestion, inflammation, and hypertrophy of the uterus—constriction of the cervix uteri; and painful, congestive, and inflammatory conditions of one or both ovaries. The second to the restoration of the uterus to its normal situation, which is best effected by introducing the uterine sound into the cavity of the organ, and using it as a lever. The retention of the replaced uterus in its normal situation is proposed to be accomplished by means of an intra-uterine pessary, which is to be worn for a period varying from one or two weeks to many months.

In reviewing the general character of this paper we are, in the first place, struck with the want of logical concatenation between the premises stated and the conclusions arrived at. We fail to see any necessary connexion established between the lesion in question and the many abnormities with which it is said to be associated: or, in other words, there is a want of proof that it, *per se*, is the cause specifically of the several consequences which are imputed to it. We believe, in fact, that there is some misapprehension in the mind of our author as to the pathological importance and significance of this lesion; and are led to conclude, from clinical observations, that the uterus is altogether a passive agent, both in its production and the causation of the many morbid phenomena with which it is associated. We would observe that this organ has no fixed position in the pelvis, but is liable to be altered both in its position and axis by a variety of extraneous agencies—by the state of the urinary bladder in front, of the rectum behind, the intestines above, and that of the colon on either side; and consequently, from a variety of morbid states incidental to these organs, it is liable to be deposed from its natural position, and to have its axis altered from its normal state. If we mistake not, it will very commonly be found that the same causes which give rise to abnormal states of the chylopoietic or abdominal viscera, simultaneously disturb the functions of the uterine and pelvic. Now, if it be true that the uterine organs suffer in these cases in a secondary rather than in a primary manner—if it be true that the retroversion and coincident disturbance of the uterine functions follow upon and are the consequences of the more general derangement referred to, rather than its cause—then it must be obvious that any treatment, to be successful, must be directed to the original cause rather than to the secondary effect; or, in other words, to the various antecedent conditions out of which the uterine malady, together with the others, may be said to arise. In support of the correctness of these views we would observe that cases of retroversion have come under our care in which there was considerable uterine and constitutional derangement, and in which the greatest benefit was derived from treatment exclusively addressed to the cure of the latter, notwithstanding that the retroversion remained. We have also met with others in which the displacement was discovered by means of the uterine sound, in which little or no uterine or constitutional disturbance was present. Now these circumstances are calculated to make us hesitate in accepting the theory which would impute to this lesion so many of the most distressing sufferings and ailments to which the female sex is obnoxious, as set forward in the text.

But if the pathological nature and relations of a lesion are doubtful, it becomes still more necessary to be cautious in the application of our therapeutical or curative measures; and here we cannot help thinking that the means suggested for its cure are scarcely warranted by our knowledge of the nature of the disease. It is proposed by Dr. Simpson, first, to replace the organ by means of the uterine sound, and secondly, to fix and retain it in position by means of an intra-uterine pessary—that is to say, by means of an inflexible and unyielding instrument, which can permit of little or no uterine movement. Now, apart from the difficulty and danger which are said to have attended this proceeding, we are prepared to oppose the practice upon purely physiological grounds. We have already adverted to the fact that the uterus was never intended to hold a fixed and undeviating position, but to accommodate itself somewhat to the varying states of adjacent organs. In the words of Dr. Simpson, its position is capable of being changed to a very considerable extent without incommodity or injury, by such exterior influences as may naturally or accidentally act upon it. Its position is so far constantly changed by the varying states of distension of the bladder and rectum. Under voluntary efforts of straining it can in general be readily pushed down half an inch or an inch into the cavity of the vagina, and it may be drawn down by instruments till the cervix reaches the external parts themselves, or even protrudes beyond them. (pp. 50, 51.) Now we contend that to fix an organ which nature never intended to be fixed, and to do away with all those necessary movements by which the uterus is enabled to accommodate itself to the varying conditions and necessities of neighbouring organs, is not only to act in opposition to all sound physiological principles, but to adopt a practice which, apart from its dangers, must be productive of very doubtful advantages.

Four papers are contributed upon the subject of Ovarian Dropsy. In the first the object of the author is to recommend the horizontal, in preference to the erect, position of the patient for the operation of paracentesis, the advantages being that the necessity of a bandage is thus done away with, and the tendency to fainting and syncope, for the prevention of which it is especially recommended, is obviated; whilst at the same time it is remarked that the contents of the dropsical cyst or cysts are more easily and more completely evacuated than when the operation is performed while the patient is in the upright position.

In the second, Upon Inflammatory and Non-inflammatory Ruptures of Ovarian Dropsical Cysts, the following conclusions are arrived at:

"1. The cysts forming an ovarian dropsy occasionally rupture, first, from inflammatory effusion into, and distension of, their cavities; or secondly, the contents of the cysts being only the common bland secretion of such cysts, and unmixed with any inflammatory matter, they may rupture from mere over-dilatation and gradual attenuation of their coats, or under sudden mechanical pressure and injury.

"2. When a cyst ruptures from the effects of inflammation, or contains within it, at the time of rupture, inflammatory secretions and materials, the escaping fluid, if effused into the cavity of the peritoneum, is always liable to be followed by dangerous, and generally fatal, peritonitis.

"3. If, however, a cyst burst into the peritoneum under mechanical injury, or in consequence of simple laceration from over-distension of its cavity, and the fluid effused into the sac of the peritoneum is consequently not commixed with inflammatory secretion, there is little or no great tendency to peritonitis.

"4. Sometimes, indeed, when a non-inflamed ovarian cyst thus ruptures into the cavity of the peritoneum, the life of the patient is preserved, or at least prolonged, by this accident.

"5. When an ovarian cyst ruptures into a mucous canal, or upon the cutaneous surface, the safety or danger attendant on the laceration is not regulated by the inflamed or non-inflamed character of the effused fluid.

"6. In cases in which the fluid of an ovarian cyst obtains an outlet by a mucous canal, or by the skin, a temporary or more permanent reduction of the tumour, and comparative cure of it, may be the consequence.

"Lastly, let me add that, as in many cases and points the surgery of art is an imitation of the surgery of nature, possibly the artificial repetition and establishment of the above modes of relief, if they could be imitated safely and certainly, may yet be found capable of temporarily arresting, if not curing, ovarian dropsies in some appropriate cases, and more particularly in instances in which the great bulk of the tumour is formed by one original large preponderating cyst, or by several cysts broken up and conjoined into one common cavity or cell." (pp. 258, 259.)

In the third paper, On the Treatment of Ovarian Dropsy by Injections of Iodine into the Cysts, it is stated that the author had so treated some ten or twelve cases; that the disease had recurred in a few of these cases, but that in others the cure had as yet been permanent; that the operation was unattended with any bad results; that little or no uneasiness had been expressed by the patients during its performance; and that the common Edinburgh tincture of iodine had been employed, usually to the extent of two or three ounces.

In the fourth—On Ovariotomy—the question is discussed—Is it, or is it not, an operation justifiable upon the common principles of surgery? and the conclusion arrived at may be thus briefly stated: that it is an operation of a most serious and dangerous character, and quite unjustifiable in many of the cases in which it has been resorted to; that when the health and life of the patient are not immediately threatened by the stage and progress of the malady, when the tumour is a source of inconvenience and deformity rather than a source of danger, and when the evils of the disease are as yet prospective rather than real, it ought not to be attempted: but that if the health of the patient were becoming rapidly undermined by the disease; if the progress of the affection showed that, ere long, it would inevitably prove fatal; if the question were reduced to one of certain and not distant death, from the course of the malady, or possibly an entire escape from the affection, with prolonged life and health from the operation; and if, in addition, it were ascertained that the tumour was free from adhesions; and other circumstances were such as to present no counter indication—then, and then only, should it be undertaken.

In the general tenour of these remarks we fully concur; and we can only hope that the admitted danger and fatality of the operation, even by those who are not altogether opposed to its performance, may direct professional attention to the discovery of other means of treating this affection than purely operative proceedings; for it is scarcely too much to affirm that, for the last fifteen or twenty years, the treatment of the disease has mainly consisted of a succession of surgical experiments, each in turn lauded as successful, and each in turn abandoned for some other. We may enumerate, for instance, besides tapping—pressure; tapping and pressure; tapping and injection of iodine into the cyst; artificial openings in the cyst made to communicate with either the surface, the vagina, or the rectum; excision of a portion, large or small, of the cyst; incision; extirpation and ligature; and yet it is not too much to assert that, after all that has been written and done, our practice is not more successful, our knowledge of the pathology of the disease is not more advanced, nor the tenure of life in the case of persons suffering from it more secure, than in the days of our more empirical forefathers. Nor can any one, probably, claim a greater success in the treatment of the disease at the present time than that claimed by the late Dr. Hamilton, who stated that, after sixteen years' trial, he had succeeded, in a number of cases, in curing or retarding it by the simple means of firm compression of the abdomen, percussion, the use of the warm bath, and a protracted course of the muriate of lime, together with the ordinary means for promoting general health.

What we require as a first step to successful practice is a knowledge of the circumstances under which these cystic growths arise; for that their origin is determined by the operation of some abnormal stimulus acting upon the uterine system is a proposition the truth of which is too evident to be doubted. Being immediately dependent upon the abnormal evolution of one or more Graafian vesicles, we can at once trace their connexion with some form of menstrual irregularity; and from a careful study of the various causes of menstrual disorders, we may ultimately hope to educe the principles upon which their development may be arrested or prevented. We have reason to believe that such an inquiry will prove more promising and successful than might at first sight appear, but it is one necessarily of a very comprehensive character. It should comprise an inquiry into all the various causes, local and constitutional, by which the uterine organs may be unfavourably impressed, including direct and secondary causes of irritation or excitement, the study of various constitutional taints and diatheses, hereditary influences, and that of various abnormal conditions of the blood and nervous system; for we are persuaded that the causes of the disease vary greatly in different cases, and comprise an extensive range, from mere simple local irritation or excitement, to that grave form of constitutional derangement out of which the colloid and semi-cancerous forms of the disease arise. It must thus be evident, that the clue to successful practice lies in a right apprehension of the particular nature of the cause in each case; and we are assured that, by investigating and correcting this in individual instances, we have been enabled to arrest their progress in some, and to effect their resolution in others. We know that, under varying circumstances affecting the constitution, these growths have entirely disappeared—the fact is certain, although it is at present inexplicable, but it serves to show that their origin and increase have a fixed and specific relation to certain morbid or peculiar states or actions of the economy, and that, if these could be accurately discerned, we might successfully co-operate with the natural efforts in effecting their spontaneous cure. We have briefly indicated the preceding views in connexion with the subject before us, but are compelled, from a regard to space, to forego entering more fully into them for the present.

We conclude our notice of the papers in the first part of the volume by a brief reference to two, On the Pathology and Treatment of Dysmenorrhœa. In the first, the author combats the prevalent opinion, that the membranous structure occasionally expelled in some forms of the disease is a mere exudation of coagulable lymph, occasioned by inflammation; and contends, and we think successfully, that it consists, in reality, of the superficial layer of the mucous membrane of the uterus itself, hypertrophied and separated. He enters at length into a consideration of the several grounds upon which this opinion is founded; but we are unable, from want of room, to give them in detail. In the second, the operation of division of the cervix is recommended for the cure of obstructive dysmenorrhœa, and the nature of the proceeding, as well as the instrument employed by him for the purpose, is described at some length. We are not in a position to judge of the merits of the operation, having neither performed it ourselves, nor witnessed the effects of its performance by others; but we are led to believe that the necessity for the practice might be obviated, at least in some cases, by the free exhibition of alkalies before and during each menstrual period. We have known such practice useful in cases in which there was an unusual constriction of the cervical orifice and canal, and in which the menstrual suffering appeared to be referable to this cause. We believe, moreover, that this practice is supported by various physiological considerations. The administration of alkalies tends remarkably to increase the fluidity of the blood, and this fact was strikingly brought under our notice some years ago, in a case in which they had been largely administered, with the view of testing their value in the treatment of consumption, given for the purpose of promoting the absorption of tubercular deposits. Now, as in some forms of dysmenorrhœa there appears to be a want of relation between the tenuity of the uterine secretion and the passage by which it has to escape, as this in many cases is clearly traceable to a rheumatic diathesis by which the fibrinous constituent of the blood is increased, and as from this condition we may deduce not only the abnormal stimulus by which the lining membrane of the uterus is excited to an abnormal action, but also the increased plasticity of the menstrual secretion which results, we think we may venture to affirm that the practice has something more than mere casual observation in its favour. Finally, we may observe, with reference to the supposed dependence of sterility upon constriction of the cervix, that we have at present the case of a lady under our care, in which pregnancy has supervened for the first time, many years after marriage, apparently as the result of general treatment specifically directed to the invigoration of the uterine organs; although, some months ago, it had been announced, by an eminent accoucheur, to be physically impossible, from the small size and great constriction of the cervix and os uteri.

The papers On the Physiology and Pathology of Pregnancy need not detain us long. The most important of the series, which is limited in number to five, is one On the Duration of Pregnancy, in which the author argues, from various facts observed in the human female and the lower animals, more especially the cow, that it is not so fixed or determinate as is commonly supposed, and that it may be prolonged from thirty to thirty-five days beyond the 280th, the limit usually assigned to it.

The third and concluding part of the volume contains several important papers On Natural and Morbid Parturition, which we will briefly consider, as they respectively relate to the subject of natural, tedious, instrumental, preternatural, and complex labour. In connexion with the first of these varieties, we may refer to some interesting observations on the mechanism of natural labour, and the mode in which the fœtal head enters the brim of the pelvis. It would appear that at different times very different views have prevailed upon this subject, but the more exact and extended investigations of Professor Naegelé, which are supported by those of the author and Dr. Martin Barry, tend to show that it almost invariably enters in the direction of one of the oblique diameters, and, with rare exception, with the vertex directed either to the left foramen ovale, or to the right sacro-iliac synchondrosis. The former is the most frequent of the two; but it is to be observed, that in the great majority of cases in which the vertex is primarily directed to the right sacro-iliac synchondrosis, it gradually rotates forwards, and emerges under the arch of the pubis, as in cases in which it originally presented at the left foramen ovale. The importance of attending to this circumstance in the application and use of the forceps must be too obvious to be insisted upon.

Of the various papers relating to the subject of tedious labour, we would more especially direct attention to those, On the Sex of the Child as a Cause of Difficulty and Danger in Human Parturition, On Irregularities of Head Presentations, the Treatment of Face Presentations, and On the Influence of Galvanism on the Action of the Uterus. In the first of these, the author adduces many facts to show that the danger to the mother and child is greatly increased in the birth of male children, and that the cause of this danger is the comparatively larger size of the head of the male than the female infant. In the papers On Irregularities of Head Presentations, and the Treatment of Face Presentations, he very judiciously enforces the principle of non-interference; advising merely the employment of such means as may act upon the mother, so as to render either the uterine action more decisive, or the parturient passages more dilated, or at least more easily dilatable. The paper, On the Influence of Galvanism on the Action of the Uterus, although very elaborate, is, in our opinion, far from being conclusive, and is opposed to the results of our own experience, as well as that of many other practitioners. Dr. Simpson's observations have led him to the conclusion that galvanism exercises no influence upon the uterus during labour, and he even intimates an opinion, that the uterine structure, like that of the dartos, is incapable of being excited by it. We have no room to discuss the question at length; but feeling that the results of clinical observation were open to several sources of fallacy, we determined to test the question in a more direct manner, by exposing the gravid uterus of a pregnant bitch, and directly observing the effects of galvanism upon it. The experiment was made at University College, in the presence of Dr. Boon Hayes, the teacher of Practical Anatomy and Physiology in that institution; and Mr. Statham, Assistant-Surgeon to University College Hospital, and the following were the results: In rather less than ten minutes after the two poles of a common electro-galvanic machine of moderate intensity had been applied to the uterus, a slight vermicular action of the enclosed portion of the organ was perceivable. In rather more than a quarter of an hour, the uterus became distinctly firm and tense during the period of the contact of the two poles, and it again relaxed on their removal—the tension of the parietes returning on re-applying the poles, and subsiding on their withdrawal; in rather more than half an hour from the first application of galvanism, labour had fully supervened, as indicated by the peculiar cries of the animal, the firm contraction of the uterus, and the propulsion of one of the fœtuses towards the vagina. It should further be observed, that uterine contraction was more powerfully produced when one pole was applied to the upper part of the spine and the other to the uterus, than when both were applied to the latter organ. We will not comment further upon these facts than to observe that they appear to be decisive, on the one hand, as to the influence of galvanism upon the gravid uterus, and on the other, of its greater influence when applied through the medium of the spinal cord and nerves, than to the uterus itself.

On the subject of instrumental labour the reader will find some useful observations on the mode of application of the long forceps, an account of the air tractor as a substitute for the forceps in tedious labours, and a very elaborate memoir, On Turning as an Alternative for Craniotomy and the Long Forceps in Deformity of the Brim of the Pelvis. The theory of the proposed practice is mainly based upon a consideration of the obstetric configuration of the fœtal head, and the comparatively smaller measurement of the bi-mastoid than the bi-parietal diameter. We fear, however, that the difficulty attending the selection of proper cases for the practice, and the increased danger to mother and child should the operation miscarry, will cause it to be rarely resorted to in this class of cases.

In treating of preternatural labour, the author lays down the following rules with regard to the operation of turning—1st. That it is not advisable to seize both feet of the infant, as some writers have recommended, except in cases in which the uterus is ruptured, or in those in which one lower extremity having been brought down, the other is firmly embraced by the os uteri. 2ndly. That it is preferable to seize the knee rather than the foot. 3rdly. That that extremity should be selected which is on the opposite side of the body to that which is presenting; and 4thly. That the practice of rotating the child when the toes are directed forwards is, for the most part, unnecessary, inasmuch as in the majority of cases there is a spontaneous tendency to this rotation as the birth of the child proceeds without any manual interference.

By far the most important contribution on the subject of complex labour is the well known memoir, On the Spontaneous Expulsion and Artificial Extraction of the Placenta before the Child, in Placental Presentations, wherein the author enforces this practice in certain cases of unavoidable hæmorrhage in which the other recognised modes of management are either insufficient or unsafe, or altogether impossible of application. He founds his justification of the practice mainly upon two considerations; the first relating to certain physiological views respecting the placental origin of these hæmorrhages; the second to the fact that in several cases the placenta has been spontaneously separated and thrown off from the uterus before the birth of the child, not only without any increase of hæmorrhage, but with either its diminution or entire cessation. With regard to the first of these considerations we have already laid before the profession the grounds upon which we are led to conclude that the chief source of hæmorrhage in these cases is not the detached portion of the placenta, as assumed by Dr. Simpson, but rather the torn utero-placental arteries which lie on the denuded surface of the uterus. We are, therefore, constrained to believe that the sole ground upon which the practice can be sanctioned is the fact that in certain cases the placenta has been spontaneously separated and expelled before the birth of the child, with a marked diminution or cessation of the previous hæmorrhage; whilst in others the same result has been attained by its artificial separation and extraction. On these grounds alone it appears to us the practice can be justified—never, indeed, as the rule, but always as the exception.

The number and variety of the papers contained in the present volume have obliged us to omit the notice of many of great value, and to touch upon others with greater brevity than their importance would otherwise demand. We have, as stated at the outset, confined ourselves to an examination of those which more particularly represent the peculiar views and practice of our author; and in venturing to differ from him on certain points, we trust that we have stated the grounds of our dissent with that deference which is due to a writer to whose labours the profession and society are so largely indebted.

F. W. Mackenzie.

Review XI.

1. *Elements of Chemistry, Theoretical and Practical.* By William Allen Miller, M.D., F.R.S., F.C.S., Professor of Chemistry in King's College, London. Part I., *Chemical Physics.—London,* 1855. 8vo, pp. 428.
2. *A System of Instruction in Qualitative Chemical Analysis.* By Dr. C. R. Fresenius, Professor of Chemistry and Natural Philosophy at Wiesbaden. Fourth Edition.—*London,* 1855.

A text-book intended as a guide in the pursuit of any of the branches of natural science stands in need of almost constant revisal; otherwise the rapid progress of science will soon detract greatly from its utility. And if this remark be more or less applicable to every branch of natural science, how much more evident does it appear when applied to chemistry and chemical physics, in which fresh discoveries succeed each other with such amazing rapidity, almost from year to year? We therefore hail with unmixed satisfaction the publication of the first part of Professor Miller's new book on chemistry, entitled 'Chemical Physics.' It is devoted to a subject upon which, as far as we know, no elementary work has appeared in this country since the publication in 1843 of Professor Daniell's 'Introduction to the Study of Chemical Philosophy;' a treatise which, however complete for the period at which it was published, is, we believe, out of print, and besides could hardly have been adapted, without extensive changes, to the teaching of the present day. Dr. Miller has, we think, judged rightly in bringing out an entirely new work on the subject; and, although he modestly describes it as having been originally intended as a text-book for the use of the students of King's College, we do not hesitate to assert that the result of his labours will be found useful not only to the pupils of King's College, but also to persons far beyond the circle of his own immediate class.

Professor Miller's book 'On Chemical Physics' constitutes an excellent, and in our opinion an indispensable introduction to the study of chemistry. That vast science has of late been so much extended as to require for its prosecution not only a superficial knowledge of, but an intimate acquaintance with most of the important laws of experimental physics. Chemistry, in fact, takes cognisance of all changes in the constitution of matter, whether these changes be effected by purely chemical agencies, or whether they be the result of heat, electricity, or other physical means. The mode of action of these different forces must, therefore, necessarily be rendered familiar to the student before he can attempt to become acquainted with the chemical results to which they may give rise.

The first part of Dr. Miller's work may be considered not only as an indispensable introduction to chemistry, but also as an important store of medical knowledge. There exists a peculiar class of physiological phenomena, which the distinguished physiologist, Magendie, has called *physical phenomena of life*, and Matteucci, *physical phenomena of living bodies;* consisting of certain physical functions of the animal body, as absorption and circulation (which include capillary attraction, gravitation, &c.), as the physical effects of animal heat upon the body, animal electricity, &c., &c. The study of these physical phenomena of life, except perhaps that of animal electricity, has been singularly neglected of late, principally because natural philosophy is erroneously considered as not connected with medicine. We beg, therefore, to impress upon our medical readers the importance of their being well acquainted with the physical laws of organic nature, with the view of applying this science to the pathology and treatment of certain abnormal states of the physical functions of the human body; and though Dr. Miller's work be not written exclusively with the view of imparting this knowledge, still it will afford the reader much useful information, to be applied to the above-mentioned branch of physiology and pathology. The second volume of this treatise, which will be devoted exclusively to Inorganic Chemistry, is expected to be ready by the end of the present year; and the third part, including Organic Chemistry, is advertised for the spring of 1856.

The volume which we have at present before us is divided into six different chapters—we would rather have called them "parts." The first chapter, devoted to a preliminary view of the nature and characters of chemical affinity, and to the laws of chemical combination, affords the author an opportunity of drawing a distinct line between the physical and chemical properties of bodies; though, as he himself observes, the limit between the two sciences is of no great importance, since the chemical nature of any substance can be but imperfectly studied without a tolerably complete knowledge of its leading physical characters. The characters and laws of chemical affinity, the different modes in which chemical compounds are formed,—namely, by direct combination of two substances one with the other, or, as is more commonly the case, by the displacement of one of the ingredients of a body by another substance, and the formation of a new compound,—are next successively treated. The laws of "definite and equivalent proportions," with their application to the construction of "tables of equivalent numbers," and the principles of symbolic notation, are rendered as intelligible as possible to beginners; but the difficulty of illustrating these laws by familiar examples to students who are supposed to be totally unacquainted with chemistry, makes us somewhat regret that the author did not defer this portion of his subject to a later period, when it might have been expounded with less difficulty and with a greater certainty of being fully comprehended by those to whom it is addressed.

After a short explanation (in chapter second) of the different systems ot weights and measures, the author proceeds, in the third chapter, to examine at considerable length the various modes in which the forces called *molecular*—that is, acting between the particles of bodies at inappreciable distances only—are supposed to operate. He classes them under the following heads: *Elasticity, Cohesion, Adhesion,* and *Crystallization.* The study of the first, elasticity, which ought, perhaps, rather to have followed than preceded cohesion and adhesion, furnishes him an opportunity of explaining the mechanical properties of gases, and describing the principal instruments depending upon the pressure and elasticity of the atmosphere. In adverting to adhesion, or the power which holds together particles of dissimilar kinds of matter, the author remarks on the importance of this property for the chemist, as being more nearly allied than any other force to chemical affinity. Adhesion, in fact, gives rise to a variety of important phenomena; it is the principal agent employed in the production of capillary action, of solution, of the diffusion of liquids, of endosmosis, and less directly in the process of the intermixture and diffusion of gases. Our author examines its action in each of these different phenomena, illustrating each case by useful or curious applications, whenever an opportunity offers. In treating of the adhesion between liquids, Professor Graham's late curious investigations on the diffusion or gradual intermixture of liquids of different densities, are minutely described. Intimately connected with the process of liquid diffusion, are the changes known under the name of endosmosis and exosmosis, which occur when the two liquids are separated from each other by a porous diaphragm, and in which, as Dutrochet first observed, the process of mixture goes on in opposition to the direct action of gravitation. Dr. Miller, after having observed that these phenomena have lately acquired additional importance in a chemical point of view, proceeds to describe the principal results obtained by means of Professor Graham's *osmometer*, and shows that in almost all cases of osmose or osmotic action, a chemical action on the material of the diaphragm, whether it consist of bladder or of earthenware, invariably occurs—indeed, appears in some degree inseparable from the existence of the osmotic phenomena, as a diaphragm composed of porous materials not susceptible of decomposition by the liquids, gives rise to little or no osmotic action. This circumstance is especially interesting in a chemical point of view, as is the following, which we also owe to Professor Graham:—that two salts, such as sulphate and carbonate of potash, for instance, when mixed together, often have an osmotic action very different from that which they exercise separately. Again, chloride of sodium, when added, even in the most minute quantity, possesses the power of reducing in a remarkable degree osmotic action in other salts.

The diffusion, or process of intermixture, of gases, is next examined. Our author arranges the dynamic conditions of gases under four heads: *Diffusion*, or intermixture of one gas with another; *Effusion*, or escape of a gas through a minute aperture; *Transpiration*, or passage of gases through long capillary tubes into a rarefied atmosphere; and *Endosmosis*, or passage of gases through intervening diaphragms. Professor Graham's curious experiments on this interesting subject, more particularly those on the rate of effusion and transpiration, are minutely described, and several new facts are adduced, showing the importance of the subject in a practical point of view, and the various applications of which it is susceptible. We cannot do better in this instance than cite our author's own words:

> "The process of diffusion," he says, "is one which is continually performing an important part in the atmosphere around us. Accumulations of gases, which are unfit for the support of animal or vegetable life, are by its means silently and speedily dispersed, and this process thereby contributes largely to maintain that uniformity in the composition of the aerial ocean which is so essential to the comfort and health of the animal creation. Respiration itself, but for the process of diffusion, would fail of its appointed end, in rapidly renewing to the lungs a fresh supply of air in place of that which has been rendered unfit for the support of life by the chemical changes which it has undergone." (pp. 83, 84.)

The fourth chapter of Dr. Miller's work, "upon Light," may be considered as constituting an elementary treatise on all those portions of the science of optics which bear directly or indirectly on chemistry. Till within the last few years, the subject of optics would have been thought to have but very little connexion with chemical phenomena; and indeed, as far as we are aware, this is the first time that a detailed account of the properties of light has been introduced into an elementary treatise on chemistry. Dr. Miller is, however, quite right; as he justly remarks, an acquaintance with the fundamental laws and properties of light has become of late indispensable to the chemist, pharmaceutist, and physician. A very few examples will suffice to test the truth of this assertion. In how many cases, for instance, does not the difference in the refractive power of the essential oils afford a good test of their quality and genuineness? Again, does not the action of polarized light furnish us a useful test for distinguishing the diamond and other precious stones from spurious imitations? The varieties that are remarked in the amount and direction of circular polarization, are well known to indicate, with a tolerable degree of accuracy, the varieties and proportions of sugar to be found in complex saccharine liquids; and the polariscope of Soleil, founded upon this principle, and intended to detect the quantity of sugar to be found in the urine of diabetic patients, is, or ought to be, familiar to all medical practitioners. We need not insist on the impossibility of rendering these facts intelligible to students totally unacquainted with the laws of optics. Some correct ideas on the different theories of light, on the laws of reflection and refraction, and on the different modes of ascertaining the refractive power, some elementary but accurate notions on the theories of interference, double refraction, and polarization, are evidently an indispensable preparation to the study of the chemical effects of light. And we may add, that these fundamental principles are treated in Dr. Miller's work with so much judgment and lucidity, that no chemical or medical student ought to be deterred from a careful study of this chapter, through an erroneous idea that the subject may not be completely indispensable to the practical chemist, or that it is too difficult to be mastered without the aid of mathematics.

The two last, and we may perhaps add, most important, subjects treated of in the present volume, relate to Heat and Electricity. No explanation is required to convince our readers that without the knowledge of these two essential branches of physics, all attempts to master the science of chemistry would, and must, prove utterly abortive. In regard to heat, as our author himself observes, "There is scarcely a chemical operation in which heat is not either emitted, absorbed, or purposely applied to produce the required result;" and with respect to electricity, three quarters of a century have now elapsed since the discovery by Volta of that most powerful of all chemical agents, the voltaic pile, which in the hands of Davy led to the discovery of the metallic nature of the alkalies and earths, effecting by the decomposition of these substances a complete change in the aspect of chemical science. Indeed, the absolute necessity of a thorough acquaintance with all the leading principles of heat and electricity as an introduction to chemistry, has been so long and so universally acknowledged, that although by far the greater number of the modern treatises on chemistry contain little or nothing on many of the important subjects to which Dr. Miller has thought proper to introduce the student of the present day, heat and electricity, at least, although not treated with so much method and detail as in the present work, have never been completely passed over in the numerous elements and manuals of chemistry, published both in this country and on the Continent, since the beginning of the nineteenth century.

Dr. Miller, after a brief review of the different sources from which artificial heat may be procured, divides the vast subject of heat into three distinct paragraphs or sections: the first comprehending the phenomena of *expansion*, with their applications to the measure of temperature; the second refers to the different modes in which the equilibrium of temperature is sustained or restored—namely, *conduction*, *convection* or circulation, and *radiation*; and the third relating to heat in *combination*, including *specific* and *latent* heat, with their different applications to the processes of congelation and liquefaction, as well as to those of ebullition and evaporation. In going over these different subjects, it is impossible not to be struck with the clearness and method which prevail throughout, and still more, perhaps, by the judgment and tact with which the author, although he leaves no important point untouched, avoids entering into useless discussions on delicate and contested questions, which would only embarrass the student, without affording him any additional facility for the prosecution of chemistry.

In the section relating to the equilibrium of temperature, our author, after having explained the different modes by which all bodies tend, either by conduction or radiation, to return sooner or later to the temperature of surrounding objects, adds a third mode, generally included in that of conduction, to which he gives the name of *convection* of heat. He understands by convection the well-known property by which liquids and gases, particularly the latter, deprived, as they are, almost entirely of the power of conducting heat, admit nevertheless of being rapidly heated or cooled, by a process of circulation producing currents, and depending on the extreme mobility of the particles that compose them. The history of the currents produced in gases by the expansive action of heat, affords the author an opportunity of entering into some interesting details in regard to their application to the ventilation of our dwellings, and also to the interesting phenomenon of the trade winds; which, as is well known, originate on a large scale in the processes of circulation produced by heat to which we have just alluded. The latter part of this interesting section is devoted to the experiments of Pictet, Leslie, Dulong and Petit, on the radiation, reflection, and absorption of heat; and finally to the still more delicate and important researches of Melloni, on the transmission of radiant heat through screens of different substances, and the singular consequences to which they have led.

The fourth and last section on heat treats of heat of *composition*, or, as it may be termed, heat of *combination*. The heat of composition exists either under the form of *specific* heat, and capable under this form of contributing to produce the ordinary phenomena of heat, such as expansion and increase of temperature in surrounding bodies; or else under the form of *latent* heat, when it disappears in large quantities, exhausted, as it were, by its own efforts, at the moment of producing the liquefaction of solid bodies, or the conversion of liquids into elastic vapour. Our author is naturally led to describe the different phenomena that accompany ebullition, and to mention some curious recently-discovered facts relative to the marked effect produced by the adhesion of the liquid to the surface of the vessel, in raising the boiling point many degrees above the usual standard. Professor Marcet, of Geneva, to whom the above experiments are chiefly due, ascribes to a similar cause the slight differences he has observed in the rapidity with which fluids evaporate, according to the nature of the vessel in which they are contained. The principal methods imagined for measuring the specific heat of bodies are next briefly described; the author, as might be expected, enters into a more detailed account of the phenomena appertaining to latent heat, and more particularly of those relating to the measurement of the latent heat of different vapours. This opportunity is not neglected of making the chemical student acquainted with the theory of distillation, and the different apparatus generally employed for this purpose. A detailed account of the process used for reducing gases to the liquid, and even to the solid state, by means of pressure and intense cold, followed by a short description of Boutigny's curious experiments on the *spheroidal* state of liquids, concludes this important section, and with it the long and interesting chapter on heat.

The sixth and last chapter of Dr. Miller's volume contains an interesting and elaborate treatise on electricity and magnetism,—forces which are now so intimately connected that it is hardly possible to study the operations of either separately. This chapter, which, as the reader may at once perceive, embraces an immense variety of matter, is divided into eight distinct sections. After some clear though elementary notions on magnetism, and on the leading characters of magnetic action, the author examines successively, in the second and third section, the phenomena of *static* and of *dynamic* or *voltaic* electricity. Under the head "Static Electricity" are treated the general phenomena presented by electricity in a state of rest, or, as it has been called, *tension;* such as those of attraction and repulsion, the laws of induction, the distribution of the electric charge, and also a few remarks on the two hypotheses by which these phenomena have been accounted for. The electric machines in general use, with their various applications, are next brought before the reader. A short account of the different sources of static electricity affords the author an opportunity for entering into some detail on atmospheric electricity, which, although its origin is still, to a certain extent, shrouded in mystery, may certainly be considered as the most important source of all. We regret that, in mentioning the phenomenon of the aurora borealis, and alluding to its electrical origin, the author has given no account of the remarkable explanation offered by Professor De la Rive, in which the aurora is ascribed to a series of electric discharges occurring in the Polar regions between the positive electricity of the atmosphere and the negative fluid of the earth; the previous separation of the two fluids being attributed to the unequal temperature produced in the atmosphere at different heights by the action of the solar rays, more especially in the Equatorial regions. This hypothesis has been admitted, we believe, by many philosophers, to offer a satisfactory explanation of most of the important facts connected with this singular phenomenon, more particularly of the connexion long since observed to exist between the aurora and the magnetism of the earth, as indicated by corresponding disturbances in the magnetic needle.* In section third, the author, after having described the origin of the voltaic pile, and the mode of measuring the intensity of the electric current by means of the galvanometer, enters into an examination of the conditions required for producing voltaic action. This affords him an opportunity of proving that, although contact of dissimilar substances may be necessary for the development of the electric current, no current is, in fact, produced without chemical action having previously occurred. The fact that the energy of the current has been shown to be proportionate to the intensity of the chemical action, and its direction dependent on the direction of this same action, is another satisfactory proof that chemical action is *essential* to the production of an electric current, and that Volta, in regarding the interposed liquid in his pile in the light merely of an imperfect conductor which allowed induction to take place through it, the electrical equilibrium being constantly disturbed by the contact of the two metals, totally overlooked the chemical changes which the liquid was constantly undergoing.

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* See Mémoires de la Société de Physique et d'Histoire Naturelle de Geneva, vol. xii.; and Bibliothèque Universelle, for 1853, vol. xxiv.

After these remarks on the theory of the voltaic pile, the reader is made acquainted with its recent modifications by Daniell, Grove, and Smee. The retardation or resistance experienced by the current from the very conductors by which its influence is transmitted is next examined, first in the case of a simple circuit, or when a single pair only of metals is employed, and afterwards in the case of a compound circuit. This affords the author an opportunity of describing Wheatstone's *rheostat* and *resistance* coils, an apparatus by which measured amounts of resistance may be introduced into the voltaic current; and also of giving an outline of Ohm's theory, in which the mutual action of the electro-motive forces, and the resistance of any current, simple or compound, are simplified by being represented under the form of a fraction.

The processes of voltaic discharge—first, by *conduction*, as when the circuit is completed by a wire, or any other good solid conductor; secondly, by *disruption*, when the current is greater than the conductor is able to convey, and when, as in the beautiful experiment with the points of charcoal, a luminous appearance is exhibited through a short interval of non-conducting matter; and, finally, the discharge by *convection* which takes place in liquids, and is accompanied by chemical action and transfer of the particles of the conductor, are next successively examined. The process termed *electrolysis*, by which compound bodies are resolved into their constituents by the action of electricity, naturally possessing peculiar interest for the chemist, belongs to this latter mode of discharge. Instead of being confined, as was formerly the case, to a certain number of insulated facts, this process is now shown to be dependent on certain definite laws, which the author passes successively in review. The presumed ignorance of the student in chemistry prevents his entering into many interesting details relative to the chemical effects of the voltaic battery, which are, no doubt, reserved for a future period.

Œrsted's celebrated experiment, in which the influence of an electric current upon a freely-suspended magnetic needle was pointed out for the first time in the year 1820, showing at once the close connexion which existed between magnetism and electricity, soon gave rise to a completely new science, known under the name of *electro-magnetism*. The general principles of this science, with their application to the various forms of galvanometers; the theory and mode of formation of electro-magnets, followed by the laws of electro-magnetism, due principally to Lentz and Jacobi, are successively treated in the fourth section of the present chapter. A clear and correct outline of Ampère's celebrated theory of electro-magnetism concludes this portion of the subject—which theory, as our author justly observes, "has satisfied hitherto the rigorous requirements of mathematical analysis, and explained all the phenomena of electro-magnetism that have been as yet discovered."

The limits of this article prevent us from referring at any length to the intricate but highly interesting subject of *volta-electric* induction, or production of secondary currents, obtained by inductive action from wires conveying currents in their vicinity, and that of *magneto-electric* induction, in which a current of electricity may be obtained in a closed conducting wire from the magnet. We can only say that, in our opinion, Dr. Miller has treated this complicated subject with a degree of method and lucidity which do him great credit.

The last section of this long chapter on electricity contains a summary o the late discoveries of Faraday and others on the relations of light and magnetism, and on the magnetism of bodies in general. The influence of magnetism on polarized light transmitted through certain uncrystallized transparent bodies, with the property these bodies possess, when placed between the poles of an electro-magnet, of rotating the polarized ray in different degrees and in different directions, according to the direction of the magnetic current, are succinctly but clearly described.

The author concludes by a more detailed account of Faraday's experiments on the magnetism of bodies in general, the distinction of bodies into magnetic and dia-magnetic, according to the position they assume with respect to the poles of a powerful electro-magnet; and finally, the influence which chemical composition and crystalline structure appear both to exercise upon the magnetic or dia-magnetic state.

We have now given the reader an insight into Dr. Miller's useful book, which, it will be perceived, is more particularly devoted to the description of the properties of matter in the statical condition; whilst a treatise on analytical chemistry, as the last edition of the valuable work of Fresenius, 'On Qualitative Chemical Analysis,' which we beg to bring before the notice of our reader, is especially calculated to convey a knowledge of the properties of matter in the dynamical state—or in other words, to give an account of the combinations and decompositions which .organic and inorganic bodies may be subjected to, with the methods employed to induce these phenomena.

The increasing importance of analytical chemistry, both from its connexions with arts and medicine, has caused a fourth edition of the treatise of Fresenius, On Qualitative Analysis, to be published, and we are much indebted to Mr. Lloyd Bullock for having edited this valuable book. The last edition of this work will be found particularly useful to the medical profession, from its containing the methods to be employed for the qualitative analysis of mineral waters, and common water, and for the detection of poisons. Arts and agriculture will also derive much benefit from this useful publication. To the farmer, the knowledge of analytical chemistry will prove of the greatest value, as affording him the means of becoming thoroughly acquainted with the nature of the soil he is cultivating, and of the manure he must employ; and to him not only may the ground yield food in abundance, but also concealed treasures, as coal, metals, or even certain substances, as phosphate of lime, which, in the form of manure, are invaluable gifts to the agriculturist. The applications of chemistry are becoming daily more extensive, and many physicians and medical students will undoubtedly consult the work of Fresenius. The connexions of practical chemistry with medicine may be considered under three heads—

1. Chemistry applied to Pharmacy.
2. Chemistry applied to Toxicology, or the Detection of Poisons.
3. Chemistry applied to Physiology and Pathology, or the Study of the Immediate Principles of the Human Body.

The author has given in this volume practical directions, which will be found extremely useful for the study of pharmacy and for the detection of mineral and vegetable poisons—as arsenic, antimony, lead, copper, and morphia, strychnia, nicotine, &c. He has admirably described the operations necessary for the detection of these substances in the contents of the stomach, and other parts of the body where they are likely to be found; and from his practical experience in this, as well as in the other branches of analytical chemistry, the last edition of his treatise becomes a most valuable addition to medical literature.

We must here express our satisfaction at the author having omitted to allude to physiological and pathological chemistry. This is a science which must be cultivated exclusively by the medical profession; for as chemical analysis consists of the methods employed to decompose chemical compounds into their primary elements, physiological and pathological chemistry teach the methods of separating *mechanically*, by means of water, alcohol, and ether, the various constituents or immediate principles of animal tissues and fluids; which immediate principles are mixed together and held in their relative positions by the vital force. We are, however, so little acquainted with the methods of conducting this mechanical separation, that it is found necessary in many cases to have recourse to pure chemical analysis; the medical student must therefore make himself acquainted with the latter branch of chemistry, and he will find Dr. Fresenius's book extremely useful, both from the simple and accurate account of the manipulations he describes, and from the admirable classification of the various stages of analysis.

The beginning of the work is devoted to the principal operations in qualitative analysis—as solution, crystallization, precipitation, filtration, &c. The subject of crystallization, one of great interest, and of especial use to the medical student, might have been dwelt upon at greater length; and we regret that the author has not alluded more minutely to the methods employed for obtaining substances in the crystallized form.

He proceeds afterwards to describe the apparatus and utensils required for chemical analysis, and then gives a correct and detailed account of the reagents in the humid and dry way. Water, alcohol, and ether are mentioned as reagents; but the author observes, that in very few cases indeed do they serve to induce chemical decompositions, so that these fluids are rather to be considered as dissolving agents, more especially adapted to produce mechanical separations than chemical actions. It is for this purpose that water, alcohol, and ether are employed so extensively in physiological chemistry.

Reagents are divided into two classes—1. Reagents acting in the humid way. 2. Reagents acting in the dry way. The reagents in the humid way are subdivided into—(1) General reagents, or those principally used as simple solvents—as chemical solvents, and also those serving principally to separate or otherwise characterize groups of substances; and (2)

Special reagents, or those which serve principally for the detection of bases and acids. The reagents in the dry way consist of fluxes, and of those employed for blow-pipe manipulations.

The author proceeds afterwards to describe the deportment of various substances with reagents; this subject is divided into—

A. Department or properties of the metallic oxides and their radicles.

B. Department of the acids and their radicles, with reagents.

Thus ends the first part of the work. It may be considered as an introduction, with which the student must make himself acquainted, in order to be able to understand thoroughly Part II., or *Systematic Course of Qualitative Chemical Analysis.*

This part of the work is divided into three sections. The first includes a systematic course of practical instruction in chemical analysis, which is divided into preliminary examination, solution, and actual examination. The second section contains a detailed account of the special methods employed to effect the analysis—of a few important compounds and mixtures which chemists are frequently called upon to examine; those methods becoming much simplified as the number of substances decrease which are involved in the analysis. The third section contains an explanation of the general analytical processes, with numerous additions to the practical operations, and to which the author more particularly calls the attention of the student. At the end of the volume is a chapter devoted to the deportment of most of the alkaloids with reagents, together with a systematic method of effecting the detection of those substances. Finally, the author adds some supplementary remarks on the separation and detection of arsenic, antimony, and tin, in presence of each other; in speaking of the methods of determining the nature of organic bases in cases of poisoning, he alludes to the processes employed for that purpose by Stass and Hofmann and Graham. In the former, an alcoholic extract of the parts to be examined is first obtained, with the addition of a small quantity of tartaric or oxalic acid. This solution being concentrated on a water-bath, and the residue treated with carbonate of soda and cold alcohol, a solution is obtained containing the alkaloid; finally, by means of ether, it is separated in a sufficiently pure condition to admit of its properties being determined. This method is therefore principally calculated for the detection of conine, nicotine, aniline, picoline, petinine, morphine, codeine, brucine, emetine, solanine, aconitine, atropine, and hyoscyamine, all of which are vegeto-alkalies soluble in ether. Hofmann and Graham have employed animal charcoal to effect the detection of strychnine in beer, charcoal having the property of absorbing this alkaloid, and yielding it to alcohol.

The systematic method described by Fresenius for the detection of alkaloids is based on the circumstance that certain of these substances possess the property of being precipitated by potash or soda, from their solutions, and of re-dissolving in an excess; others, of being precipitated by these alkalies without re-dissolving in an excess, and of being precipitated also with bicarbonate of soda, even from acid solutions; others again (forming a third group), of being precipitated by potash, and not re-dissolving in an excess, nor being precipitated from an acid solution by the bicarbonate of fixed alkalies.

The analysis of mineral and fresh waters is a subject of particular interest to the medical practitioner, and the method described by Fresenius for that purpose possesses undoubtedly the advantages of being complete, simple, and practical. The first stage of the analysis of mineral waters must be carried on at the well or spring, where the water is to be filtered, and where carbonic acid, sulphuretted hydrogen, and iron may be detected at once. The analysis is now to be continued in the laboratory, the clear filtrate is evaporated to dryness, and the residue treated with alcohol, to separate iodine and bromine; it is afterwards tested, to determine the absence or presence of silicic acid, fluorine, baryta, strontia, arsenic, alumina, and phosphoric acid. Another sample of the water is taken for the detection of lithia; and a third quantity, being previously concentrated, is tested for boracic acid; ammonia is detected with lime by the usual means. The methods employed for determining the presence of other substances which may occur in mineral water, as lime, potash, soda, &c., are described in the paragraph concerning the analysis of fresh waters (spring, well, brook, and river water). The constituents of the substance deposited by the water as it issues from the spring are now to be determined; the mass, previously washed, is treated with hydrochloric acid and filtered; the filtrate may contain baryta, strontia, arsenic, antimony, tin, lead, copper, alumina, phosphoric acid, sulphuric acid, fluorine, and silicic acid, which are separated from each other by a systematic method, and then tested. The residue, insoluble in hydrochloric acid, consists usually of silicic acid, clay, and organic matters, but it may also contain sulphate of baryta, sulphate of strontia, and chloride of calcium; if arsenic has previously been detected in the water, it will be advisable to boil a little of the deposit in a concentrated solution of potash and soda, with the view of ascertaining whether it exists there in the state of arsenious or arsenic acid.

To conclude our analysis of this treatise, we may be allowed to allude shortly to the method described by the author for the detection of arsenic. He recommends a process similar to that which is employed for the detection of grape and diabetic sugar. Where arsenious acid is dissolved in an excess of soda or potash, and mixed with a few drops of a dilute solution of sulphate of copper, a clear blue fluid is obtained; upon boiling, a red precipitate of suboxide of copper falls down: the solution contains arseniate of potash. This reaction the author considers as exceedingly delicate; but it ought only to be employed as a confirmatory proof of the presence of arsenious acid; this method is also useful as a means of distinguishing that acid from arsenic acid. Dr. Fresenius describes minutely the method of detecting arsenic acid, by its being converted into arseniuretted hydrogen. He observes that the spots formed on a porcelain plate by the gas evolved from the apparatus when kindled, have a rather blackish-brown colour and a bright metallic lustre; whilst those of antimony are of a deep black colour, but feebly lustrous. The arsenical stains may be distinguished, moreover, from the antimonial stains by pouring over them a solution of chloride of soda (a compound of hypochlorite of soda with chloride of sodium, prepared by mixing a solution of chloride of lime and carbonate of soda in excess, and filtering), which dissolves the arsenical spots immediately, whilst it leaves the antimonial spots unaffected, or removes them only after a considerable time.

In regard to the means of detecting the presence of arsenic in articles of food, in dead bodies, &c., for medico-legal investigations, Dr. Fresenius gives a minute account of the process he published some years ago, with Dr. V. Babo, where the solution of the poison is effected by means of hydrochloric acid and chlorate of potash; some sulphate of soda being afterwards added, and then a current of hydrosulphuric acid gas passed through the fluid, the arsenic is precipitated as a tersulphide.

We might add considerably to this abstract of the useful work of Dr. Fresenius; but we trust our remarks will suffice to make the reader acquainted with a general outline of the treatise, which we may recommend as one of the best practical works in the English language upon qualitative chemical analysis.

W. Marcet.

Review XII.

Eutherapeia; or, an Examination of the Principles of Medical Science: with Researches in the Nervous System. By Robert Garner, Surgeon to the North Staffordshire Infirmary, late President of the North Staffordshire Medical Society, &c. &c.—*London*, 1855. 8vo, pp. 282.

Not distinctly recognising from the title of this book the precise intention of its author, and imagining that our readers may feel a similar, if not an equal, difficulty, it appears to us most desirable to state at the commencement the object which Mr. Garner has in view, and to do this in his own words:

> "To elucidate the credibility of the principles of medicine, to show that these principles may not unfairly be placed in comparison with the accredited conclusions of other kindred sciences, and to vindicate our art amidst the pretensions and dogmas of charlatanism—to meditate for a short time on the origin and sad prevalence of disease, to investigate the degree of curative power furnished us in remedial agents, and to compare diseases and their cures and alleviations with corresponding evils and their remedies in the moral world—to elucidate, too, the existence of a governing power, or degree of inherent curative tendency in the human frame, manifested in the regular course or order of phenomena or symptoms in what are in this respect wrongly called disorders, but, above all, in that happy constitutional aid during their treatment, which we experience in most diseases—to consider to what extent these effects may be relied upon—lastly, the indication of a visible intention and even of goodness in the permission by Providence of disease and pain—such are the subjects which the author would wish to discuss in the following pages." (p. 2.)

Mr. Garner informs us in the preface, that "he has written for the profession in the first place, but also with an eye to the inquiring portion of the community;" and thus we were led to hail the appearance of his book as that of one, if accomplishing its author's object, calculated to supply a want which many must have felt it was most desirable to supply.

The idea of the work before us is emphatically good, for perhaps there is no subject upon which the public has more frequently to judge, and upon which it is less generally informed, and consequently judges more incorrectly, than that of medical practice. The numberless vagaries of pseudo-medical science find adherents, whose faith is firm in inverse proportion to its reasonableness, and whose consciousness of heterodoxy supplies them with a morbid pleasure to set in the balance, against those lamentable results which too often follow from their indiscretion. But it is not only in this direction that the public evinces its want of information; it is also in the erroneous and exaggerated notions which are entertained with regard to the nature of diseases, and the power which medicines can exert for their alleviation; and also in the frequent opposition of its own opinions to those of even its favoured medical advisers. At some of these erroneous ideas we cannot wonder; the public cannot get rid of those links of superstition and false system whose traditionary honour has conferred upon them tyrannical authority; but we must feel astonishment at the credence of many novel absurdities, the slightest examination of which, by the most ordinary rules of logic and common sense, could not fail to demonstrate their utter unworthiness of belief.

Those who are not of the medical profession are so constantly compelled to seek its aid, that comparison and selection are necessary processes for the individual to perform. Since we are not at all disposed to believe that at the present time, nor are we inclined to wish that at any future time, opinion and action are to be guided by the dicta of institutional authority, it appears of great importance that the public should be so instructed that it may form correct judgments, so far as it is called upon to judge. It is utterly impossible that the facts and doctrines of medical science, in their detail, should be appreciated, except by those who have devoted years to their laborious study; but it is possible, and the progress of general education renders it probable, that when a few more generations have passed away, the data for judgment upon all matters which it really concerns the public to judge will be familiar to the minds of all educated men and women. The principles to be applied in the appreciation of facts, together with the general phenomena and laws of life, of disease, and death, may and should form a part of polite education; and we feel confident that when this is accomplished, the pseudo-sciences, and that which is false in legitimate sciences, will be duly disregarded, the true and the useful being alone chosen as the guides and auxilia of humanity. But at the present time this is not accomplished, and it often happens that the exigencies of a certain period demand a special work which was not possible at an earlier, and which would be unnecessary at a later. It is important, sometimes, to forestall the slow progress of information in the mass, by presenting the better classes with results to which the former will arrive by a more gradual process; and this is, we imagine, the object of Mr. Garner in directing his "eye to the inquiring portion of the community."

Thus, then, we are led to consider the idea of the author of 'Eutherapeia' as an extremely good one; the general plan which he has adopted is likewise commendable, but the mode in which the idea is carried out, and the plan filled up in detail, must, we think, render the book of little value to the profession, and almost incomprehensible to even "the inquiring portion of the community." It details few facts with which the profession are not already acquainted; there is no new arrangement of previously established doctrines to give them increased value; while they are so constantly, almost invariably, expressed in technical terms, that the public will, we fear, form little idea of their meaning.

The book is divided into eight chapters, their subject-matter being respectively—Anatomy, the Nervous System, Organic Chemistry, Pathology (two chapters), Medicinal Substances, the Divine Dispensation in Disease, and the Pseudo-Medical Sciences A book having this general plan might, we conceive, be made of much value to the profession, and especially to its younger portion, some of whom often commence their studies without any definite idea of the inter-relations of their several branches. It might also be the means of conveying much instruction to the public, in its endeavours to know something of the mysterious organism with which each individual feels himself associated, something of the diseases which may make it an almost intolerable burden, and the objects of these diseases, as parts of a "Divine dispensation," in their relation to the individual, and to those means which Providence has supplied for his relief. Thus there might be gained some direction, not only of external life under the pressure of its immediate physical derangements, but direction of mind and of heart in regard to the exercise and tenure of that life in its bearing upon individual prospects and social claims.

A few quotations from the chapters, the titles of which have been already mentioned, will enable our readers to judge for themselves with what success Mr. Garner has carried out his scheme, and they will, we think, show that the general criticism we have given above is not without foundation.

As a specimen of the chapter on Anatomy, or the "survey of different systems of organs, particularly connected with their functions," let us take that with which Mr. Garner commences, "Digestion, or assimilation." After stating that the food is swallowed, and that it may be watched down the œsophagus of a horse, we have the following:

> "The movements of the stomach are, no doubt, curious; we see in animals a result of them, in the hair balls sometimes found there, polished and round externally, internally consisting of hairs which the animal has licked and swallowed. Galen and Vesalius pointed out many peculiarities of this organ, the former its muscular coat, as well as the mucous follicles, which last, however, have received their names in this organ, and in the intestinal canal, from later anatomists—Brunner, Lieberkühn, and Peyer. According to Bernard, the gastric juice is secreted only in the pyloric third of the stomach. After the food is converted into chyme, the pylorus admits it into the small intestines, or progressively into the duodenum, jejunum, and ileon, and thence it passes into the cœcum, colon, and rectum, or large intestines, divisions distinguished by the ancients. The small intestines deliver their contents into the cœcum by the valve named after Bauhin, previously, however, described by Rondeletius. Achillinus and Béranger had also studied this curious portion of the intestinal canal, the latter, seemingly, viewing it as a second stomach. It is extremely large in some vegetable feeders, as rodents and solipeds. The villosities giving origin to the lacteals, the valvular duplicatures, the mucous follicles, and the muscular coat, arranged in the large bowels in longitudinal bands, also the processes and duplicatures of the peritoneal coat, often containing fat, were described by Vesalius, and other early anatomists." (p. 15.)

After stating that the "chyme is mixed with the bile in the duodenum," and also with the pancreatic juice, the result being "the production of the chyle and fæces from the chyme," we are informed that "in the present day Kiernan and Paget in this country have investigated the more minute structure of the liver," and then, with a few remarks upon the gall-bladder and spleen, the anatomy of the digestive system is completed, and that of the circulating and respiratory organs is passed under similar review.

The above passage is sufficient to indicate the deficiency of Mr. Garner's book. The account of the stomach and intestines is so trivial, that the youngest student of anatomy cannot but be discontented; he is made acquainted with every fact which Mr. Garner has mentioned during his first session of attendance upon lectures, except, perhaps, the individual merits of Rondeletius, Achillinus, and Béranger; and it is of very little use to tell the public of mucous follicles, the pylorus, gastric juice, and chyme, and that the latter passes through six long names of intestine, unless some definition is given of the terms employed. The remark upon the "more minute structure of the liver" would be, perhaps, more intelligible to the community if something had been told them of its less minute structure, or if a reference had been given to the original papers of Messrs. Kiernan and Paget; but there is nothing of the kind to be found, and the statement is scarcely less complimentary to the tyro in anatomy, than it would be to tell him that Shakspeare had written some dramas, and that Bacon was the author of the 'Novum Organon.' We think it would have been better to have left the liver out altogether, for there certainly is no subject upon which the public holds more erroneous ideas. It is looked upon as a very Moloch of an organ, to be pleased and displeased in a thousand ways, to be set wrong when right, and to be set right when wrong, by means that are totally at variance with anything in the shape of facts, or sound physiological doctrine.

The chapter on the Nervous System is far superior to the others; it contains a succinct and interesting *résumé* of our knowledge with regard to its anatomy in the different classes of animals; and though much more valuable to the student than any other part of the book, is, perhaps, even less so to the public, as it is extremely difficult to avoid the frequent use of technicalities. There are, however, many passages which we think open to misconstruction: volition and perception are not distinguished, as they should be, from motor impulse and sensation; and there is a looseness of phraseology which cannot but give rise to indistinct ideas. As examples, we may quote the following:

> "When the organs of motion become more perfect, and sensation less diffused, there must be nerves and ganglia, the latter the seats of volition, or motor impulse and perception; the former the conductors of such impulse from, and of the sensation to, the ganglionic sensorium." (p. 26.)
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> "From injury of the spinal cord or of a large nerve, it is proved that sensation and motion are destroyed in all parts below the injury, the medium of communication being destroyed." (p. 39.)
>
> "Paralysed limbs are, however, sometimes seen to move when pricked or pinched, without the sensorium being conscious of it." (*ib.*)

It is quite unnecessary to point out the indefinite, if not erroneous, manner in which the words, "motion," "sensation," "perception," and "conscious," are employed in these sentences. It is also, we think, unnecessary to make any comments upon the three pages which contain "pathological conclusions" with regard to "diseases of the nervous system," but we select the following as a specimen: "If the fornix is affected, the *speech* is imperfect, there is *double vision, contractions* and *convulsions of the limbs, incoherence, loquacity, delirium, stupor, and coma.*"* (p. 73.)

The chapter on Organic Chemistry is very defective in many points, but represents with fairness the present position of our knowledge with regard to others. We may mention, in proof of the former observation, the manner in which the question of animal heat is treated; as an example of the latter, the paragraph upon the functions of the liver. We do not think that our author, in speaking of the blood-corpuscles, has been fortunate in his illustration, as the following quotation shows:

> "It (the blood) also contains a vast number of flattened globules floating in the serum, which appear to be as much organizations as the cells of the areolar tissue, and, like them, have commonly central nucleoles. On these corpuscles or discs the colour of the fluid depends." (p. 90.)

We do not see that in any previous portion of the book Mr. Garner has described (or has even referred to any one who has described) the "more minute structure" of areolar tissue. If he had done so, we cannot but think that "the inquiring portion of the community" might ask some unpleasant questions; but as he has not, we will not attempt to pry into the analogy between the blood-corpuscles and the "cells of areolar tissue." The chapters on Pathology, are introduced by a history of opinions with regard to the nature of disease; and this, although sketchy, is decidedly the best part of these chapters. There is a want of arrangement, an absence of anything approximating to such a general view of the question that the public might gain any valuable information, whilst the details are so imperfect that the profession can derive little benefit from their perusal.

Inflammation—that great "bugbear," as it has been termed, not only of the community, but of the profession itself—is the first object of which our author treats. It is spoken of as "an exaltation of vital action," and the buffy coat is said to be a "sign which seldom deceives;" but there is no attempt made to present such a view of the process that old and erroneous ideas may be removed. The inflammations of various organs are very briefly commented upon in separate sections, four or five lines being devoted to meningitis, about twice that number to pleuritis and empyema, and so on. We then come to varieties of fever, and under the title of "Continued Idiopathic Fever" find the following:

> "Twenty years back, continued fever, in some parts of England, might be termed cerebral, and some considered its essence to consist in inflammatory action of the brain. Latterly it has been much more commonly attended by inflammation of the gastro-enteritic† mucous membrane, and not unfrequently with a minute or miliary eruption, or with bronchitis," &c. (p. 128.)

This is all that is said upon the important differences to be observed in the group of continued fevers, and it is only one of numerous specimens of pathology very far behind that of the present day. The following will serve as another example of the same chapter:

> "LARYNGITIS.—True inflammation of the larynx is a very serious matter, requiring to be subdued at almost any expense to the system, or else it is necessarily fatal in a short space of time. The great Washington died of this disease, and it has been actively canvassed whether, in the case of an old man like him,

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* The italics are Mr. Garner's own.

† A misprint, we presume.

the above rule of practice should not have been much relaxed; he was a hero, and certainly in his last hours appears to have been treated heroically." (p. 185.)

In the sixth chapter, medicinal agents are classified in accordance with their differences of action upon systems of organs; but there is nothing in this classification to demand notice, and we again fail to find, what we should have expected, some general statements of the modes and limits of medicinal operation.

The chapter on the Divine Dispensation in Disease occupies a relation to natural theology similar to that which the preceding chapters occupy to physiology and pathology. The pages of this journal are not the proper place for a discussion of the question raised by Mr. Garner's treatise; for, although a belief in the Divine dispensation of all things, can alone place man in the position from which he may so view, as to appreciate correctly, the phenomena of this world, as displayed in his own life, in that of the daisy at his feet, or in the wider ranges of cosmogony and history, yet the attempt to establish inductively the existence of that dispensation, can be successful only upon the admission of certain premises, the granting of which is tantamount to an admission of the whole; it is still further incorrect in method, inasmuch as it is based upon the consideration of facts, which can be fully appreciated only by the admission of that which it seeks to prove. Allowing the Divine dispensation to be a great and universal truth, the history of man in disease, as well as in health, affords abundant illustration of its presence; but if we possessed nothing beyond the phenomena of earth to teach us theology, if we had no surer convictions of the power, the order, and the guardian care of a Supreme Being, than those which we could obtain by inductive reasoning, the little faith that we now have would become almost extinct, and life would in reality be a dream.

The chapter on the Pseudo-Medical Sciences contains an examination of the principles of hydropathy, homœopathy, &c. The remarks are many of them good, and the treatment of the subject fair, but they are not such as to demand any special notice.

We wish that our task in reviewing this book had been more pleasant; but although it is impossible to avoid seeing that much labour has been bestowed upon it, we cannot regard it as likely to accomplish the very laudable and desirable object which its author had in view, and we can only trust that he may be more successful in some future endeavour to "set the claims" of the medical profession "in a fair position," and to "demonstrate that considerable reliance may be placed upon the present theories and practice of medicine." Mr. Garner has endeavoured to write for the profession and "the inquiring portion of the community" at the same time—and this is always a difficult work to undertake, but perhaps with no subject more so than with the one which forms the topic of Eutherapeia.

PART SECOND.

Bibliographical Record.

Art. I.—*The Diagnosis of Diseases of the Brain, Spinal Cord, Nerves, and their Appendages.* By J. Russell Reynolds, M.D. Lond., University Medical Scholar, Assistant-Physician to the Hospital for Sick Children.—*London,* 1855. 8vo, pp. 251.

Nothing can tend more to remove from therapeutics the reproach of vagueness and want of certainty, than the improvement of diagnosis. Given, even in the present state of medical knowledge, a precise lesion in a tissue, the conclusion as to the powers or inefficiency of a remedial agent is not likely to be far removed from truth. We may not know how to cure the malady, when we have arrived at a precise knowledge of its characters, but we shall at all events know with tolerable certainty how much or how little we are able to do. The diseases of the nervous system at large, more than the affections of other portions of the human frame, require what Dr. Reynolds calls "differentiation," in order to enable the physician to arrive at a satisfactory system of neuro-therapeutics. To the task of classifying and comparing the various symptoms characteristic of diseases of the nervous system, Dr. Reynolds has applied himself with great candour and discrimination, and the result is a book which, while it does not profess originality of matter, brings before the student, in a form that is original and clear, a summary of what may be regarded as the ascertained facts of neuro-pathology. The subject is treated under four heads. In the first Part we find the general pathology of the diseases in question, embracing the objects of diagnosis and its limits, the elements of diagnosis, an exposition of the classification pursued, and the diagnosis of locality generally. The second Part is devoted to the diseases of the brain; the subjects, treated in ten chapters, are as follow: 1. The diagnosis of brain diseases as to their general nature. 2. The differential diagnosis of acute febrile diseases affecting the brain. 3. The differential diagnosis of apoplectic diseases. 4. Differential diagnosis of diseases marked by delirium. 5. Differential diagnosis of convulsive diseases. 6. Differential diagnosis of acute hyperæsthesiæ. 7. Chronic diseases generally. 8. Diseases characterized by exalted activity. 9. Diseases marked by diminution of function. 10. Diseases characterized by the combination of increased and diminished function.

Part the third comprises, in four chapters, the diagnosis of diseases of the spinal cord, under the following heads: The diagnosis of the special locality affected—The diagnosis of spinal diseases as to their general nature—Acute disease of the spinal cord and its meninges—The chronic diseases of the spinal cord. Two chapters on the diseases of the nerves form the fourth and concluding Part of the work.

As a careful digest of the acknowledged phenomena accompanying the diseases in question, the work of Dr. Reynolds cannot fail to be valuable to the student and interesting to the practitioner.

ART. II.—*Atlas of Cutaneous Diseases.* By J. MOORE NELIGAN, M.D. Edin., M.R.I.A., Honorary Doctor of Medicine Trinity Coll., Dublin, Fellow of the King and Queen's College of Physicians in Ireland, &c. —*Dublin*, 1855. 4to, plates 16.

THE art of chromo-lithography has, in the present instance, achieved a very satisfactory result in the production of coloured illustrations of cutaneous diseases; great credit is due both to Dr. Neligan in supplying to the student so useful a work, and to Messrs. Forster for the manner in which it is executed. Extreme care is necessary in adjusting the various stones required to represent a coloured object, independently of the difficulty of graduating the tints. The latter difficulty is necessarily very great in so delicate a subject as the multiform hues accompanying skin diseases. Another great difficulty in their representation by chromolithography consists in the distinction of minute differences—as of small vesicles and papulæ—upon which, however, often the diagnosis depends. In the work before us some of the drawings are particularly close representations of nature: we would instance the first plate, giving the various forms of erythema, and an admirable one of erysipelas of the face; the forms of herpes (plate 4); of impetigo (plate 7). Plate 13 gives a most artistic representation of purpura, which cannot fail to impress the student, so that he would at once recognise the disease at the bedside. Nor are the illustrations of lupus and porrigo favosa less characteristic. We need scarcely add that we cordially recommend the work.

ART. III.—*Unsoundness of Mind in Relation to Criminal Acts. An Essay to which the first Sugden Prize was this year awarded by the King and Queen's College of Physicians in Ireland.* By JOHN CHARLES BUCKNILL, M.D. Lond., Licentiate of the Royal College of Physicians, Fellow of University College, Fellow of the Royal Medical and Chirurgical Society, and Physician to the Devon Lunatic Asylum. —*London*, 1854. Small 8vo, pp. 148.

THERE is scarcely a more melancholy spectacle than that of a member of society being treated as responsible for acts which disease had rendered him incapable of seeing in their true bearing; there is scarcely one that is more calculated to bring the administration of the law into disrepute than that of moral delinquency finding sympathy and palliation under an erroneous plea of mental aberration. As long as the accidental bias of a jury composed of men who have never seriously reflected upon or in any way studied the subject of mental derangements, determines the momentous questions involved in a plea of insanity, and as long as medical men are put into the witness-box for the purpose of making out a case rather than of eliciting truth, so long we shall be liable to witness the melancholy spectacle of moral guilt unpunished, of innocence subjected to the extreme penalty of the law. The study of insanity is one demanding so much attention and careful study; the operations of the mind are so intricate, and all language defining and delineating them so liable to misinterpretation, that nothing is easier for a skilful advocate than to elicit from a medical witness statements which may suit his own purposes. It is manifestly unjust to place a medical man who has not specially studied the subject of insanity, and does not even know the terminology of psychopathy, in so false a position. In regard to this point, as in regard to many other questions connected with forensic medicine, nothing is more imperatively demanded in this country, than the institution of offices to be held by men qualified by research and practical experience to give, not a judgment, but an authoritative opinion on the matters alluded to. A *master in medicine* would be an appropriate title, and one which, if remunerated as such a post must be to secure the exclusive services of a superior class of men, would be more suited to the genius of the country than others taken from foreign countries. He would correspond to the government physicians or *physici* of Prussia, or the *experts* of France. Upon these and allied points Dr. Bucknill dwells with much force.

The relation of cerebro-mental diseases to the law, and their interpretation in the medico-legal sense, is given by the author in a form which carries conviction with it. The book is one that deserves to be studied by all who may have to deal with forensic questions bearing upon insanity.

We have followed Dr. Bucknill with much interest in his arguments on the fallacies resulting from the practice of determining by solitary interviews with an individual on the intricate questions of sanity and insanity. We had marked several passages for extract, and regret that we are compelled to confine ourselves to referring the reader to the book itself, with our hearty commendation both of its matter and style.

Art. IV.—*Wiesbaden als Heilquelle, und als Climatischer Heilort dargestellt.* Von Dr. C. Braun, Prakt. Arzt. Zweite Auflage.—*Wiesbaden,* 1855. 8vo, pp. 144.

The Mineral Waters and Climate of Wiesbaden. By Dr. C. Braun.

Among the numerous watering-places that abound in the volcanic regions of the Rhine, Wiesbaden has held a prominent place from the days of Pliny downwards. The springs that are employed medicinally are, with one exception, thermal, and their temperature varies considerably—that of the chief brunnen, the Kochbrunnen, which is the highest, is, according to Fresenius, 55° R. (156° F.); the others range from this to 10° R. (54° F.). The main constituents are chlorides of sodium, calcium, potassium, and magnesium, with minute traces of iodide of magnesium, carbonate of iron, and of other salts. More or less free carbonic acid and nitrogen are found in most of the springs.

We draw attention to Dr. Braun's work, because it is distinguished from the great bulk of writings of the kind by the attempt to withdraw balneology from the sphere of crude empiricism. He has performed a series of experiments to determine the physiological action of the waters of the Kochbrunnen upon the system. As we shall shortly have occasion to enter more fully into the question of the metamorphosis of tissues produced by these agents, we now confine ourselves to the following summary of the physiological effects produced by the waters of Wiesbaden.

In the internal administration we are first met by the improved digestive powers. This is produced, in the first instance, by the water, which is demulcent and solvent; in the second, by the chloride of sodium, which is a gentle stimulant, liquefying the mucus and protein compounds; next, by the volatile stimulus of the free carbonic acid, and also by the tonic influence of the small quantity of iron present. In small doses, frequently repeated, the action is confined to the upper portion of the intestinal canal, and to the general stimulation of the urinary organs and lymphatic system. In larger doses the water becomes purgative, and the secretions generally are more powerfully excited. Dr. Braun does not assert any specific action of the waters in definite diseases, but claims for them a more or less powerfully alterative effect in various morbid conditions of an asthenic character. As, in 1854, the number of patients (kurgäste) is said to have amounted to twenty-six thousand, many of whom were undoubtedly our own countrymen, it is not a matter of scientific importance alone that we should turn our attention to the physiological and therapeutic effects produced by such waters as those of Wiesbaden.

We may add, with regard to the climate of the place, that Wiesbaden, owing to its protected site at the southern declivity of the Taunus mountains, has the advantage of a more uniform and a higher average temperature than the country generally, enjoys. The atmosphere is of a dry character, and the amphitheatrical form of the adjoining mountains guards the locality much against winds,—a point of considerable importance, when it is selected as a residence for patients affected with pulmonary disorders.

Art. V.—*The Medical Profession in its Relations to Society and the State. An Oration delivered on the Eighty-second Anniversary of the Medical Society of London.* By J. F. Clarke, Esq., late Vice-President of the Society, &c.—*London,* 1855. pp. 24. (Published at the request of the Society.)

From whatever point of view the present position of the medical profession be regarded, one thing is certain, that we must adopt and act upon the motto, *Pax domi, foris Bellum.* The moral force which we are gradually acquiring we owe to the education, the liberality, the intelligence, of the members of the profession; and by perseverance in the prosecution of objects, which it is good for the commonwealth, more even than for ourselves, that we should obtain, they will be realized sooner or later, because they are the inherent necessities of the development of an important and integral part of the State. How soon that realization shall be effected must depend mainly upon the energy, the temper, the unanimity, with which the goal is followed. Neither the flippant impertinences of a Prime Minister, nor the supercilious condescension of men who are unable to appreciate the noble deeds of the past, and the achievements and aims of the present generation of medical men in behalf of their country, need for an instant divert the profession from the pursuit of that goal; but above all things it is necessary to be firm and united. Mr. Clarke eloquently dwells upon the points at issue between the profession and the general public,—the improper constitution of all the public boards in which medical questions are paramount, the deplorable system of gratuitous advice, the encouragement of quackery, the absence of all recognition of the profession as a body in the houses of legislature; the absence of public rewards to members of the medical profession, whose services to their country, had they been yielded by others, would have secured to them the highest honours. The sentiments expressed by Mr. Clarke are essentially those of the profession at large, and by promulgating them in the position which was afforded him by being selected the orator of the Medical Society of London, he has added one more stone to the edifice which it is the bounden duty of each individual member of the profession to assist in erecting and completing.

ART. VI.—*Observations on Diseases of the Rectum.* By T. B. CURLING, F.R.S., Surgeon to, and Lecturer at, the London Hospital. Second Edition, revised and enlarged.—*London*, 1855. 8vo, pp. 129.

WE have on a former occasion expressed ourselves favourably on Mr. Curling's work, and have a pleasure in reiterating that opinion. The present edition contains an additional chapter on the very troublesome complaint, prurigo podicis; Mr. Curling adverts to the various local and general causes to which it may be due, and we may mention that among the local applications, he recommends as one of the best lotions for relieving irritation of the part, one composed of a drachm of sulphuret of potassium and eight ounces of lime water.

ART. VII.—*An Essay on the Action of Medicines in the System, or on the Mode in which Therapeutic Agents introduced into the Stomach, produce their peculiar Effects on the Animal Economy.* By FREDERICK WILLIAM HEADLAND, M.B., B.A., F.L.S., M.R.C.S., &c. Second Edition, revised and enlarged.—*London*, 1855.

THE rapidity with which the first edition of this work has been disposed of, shows that the subject upon which it treats, is one upon which the profession anxiously seek information; it is *à priori* evidence in favour of the author's merits. The second edition contains additional matter to the extent of about fifty pages. The care with which the literature bearing upon the subject has been studied by the author up to the most recent period, and the use he makes of it, considerably enhances the value of the work. We may instance the questions relating to the theory of endosmosis, in its bearing upon the action of medicines; Dr. Headland, both by his own experiments and by the evidence he adduces from other inquirers, further strengthens the strong case he had made out in his first edition, that the purging or diuretic effect of saline solutions depends rather on the quantity of the salt contained in them, than upon their degree of dilution.

Though a work of this kind necessarily contains much that is speculative, and much that will therefore excite controversy, still the earnest and thoughtful labour and candid criticism that the author has bestowed upon the subject, insure to his essay a high rank in medical literature.

ART. VIII.—*A Universal Formulary, containing the Methods of Preparing and Administering Officinal and other Medicines, the whole adapted to Physicians and Pharmaceutists.* By R. EGLESFIELD GRIFFITH, M.D. Carefully revised and much extended by ROBERT P. THOMAS, M.D.—*Philadelphia*, 1854. 8vo, pp. 651.

THIS work presents, in a very complete and well-arranged form, "a compendious collection of pharmaceutic processes and formulæ, with such additional information as may render it useful to the physician and apothecary." The first seventy-five pages contain an analysis of the weights and measures used throughout the civilized world, chapters, on specific gravity, on the temperatures required for pharmaceutical operations, on the specific gravities of some of the preparations of the Pharmacopœias, and on the relation between the different thermometrical scales; an explanation of the abbreviations used in formulæ, a vocabulary of words employed in prescriptions, and some observations on the management of the sick-room follow; the introductory part concludes with some general rules for the administration of medicines, the utility of which we should much question, as they convey too little information to the educated medical man, and would only be likely to mislead the uneducated or the layman. The body of the work contains the simples and chemicals, arranged alphabetically according to their Latin names, and under each the pharmaceutical and other preparations in which they are employed, with such further information as to mode of preparation, dose, and administration, as may be legitimately looked for in a book of this kind. Under the head of addenda are to be found "a large number of practical receipts which could not have been introduced with propriety under any officinal heads," such as anatomical injections, cements, poisons for vermin, inks, and the like. Recipes for the dietary of the sick-room, a list of incompatibles, a full posological table, and a comparative table of pharmaceutical names employed in the Pharmacopœias of the United States, London, Edinburgh, and Dublin, follow. We then find a long chapter of directions with regard to pharmaceutical processes, followed by one giving the symptoms, post-mortem appearances, and antidotes for poisons. A full and complete general index concludes the work; this is preceded by an index, which is less complete, and therefore very likely to mislead, of diseases and their remedies.

On the whole, the work is an excellent compilation, and the English student will find it useful as a means of becoming acquainted with many medicinal substances yielded by the rich flora of America, besides those with which he is already familiar. The following are a few of those given in Dr. Griffith's work:

Apocynum Androsæmifolium, a native of the United States, emetic and diuretic; forty grains are emetic; *Cahinca*, the root of *Chiococca Anguifuga*, a Brazilian shrub, a diuretic used in dropsies, in doses of from thirty to forty grains; an alcoholic extract and decoction are given; *Ceanothus Americanus*, a native of the United States, with a bitter and astringent principle, residing chiefly in the root; *Coptis trifolia*, the orange-yellow roots contain a pure bitter, without tannin, and are employed in powder, infusion, and tincture, as a stomachic; the whole herb of *Cunila maritima* is employed in various forms, as a stimulant, carminative, sudorific, and emmenagogue; *Heuchera Americana*, or alum root, a powerful astringent of the order of saxifrages; *Monarda punctata*, a perennial North American herb, abounding in a volatile pungent oil, and employed in flatulent colic and as an emmenagogue; *Phytolaca Decandria*, a large herbaceous plant, the berries and root of which are officinal; the root is emeto-cathartic, with some narcotic properties, and is employed as an alterative in syphilis, rheumatism, and chronic eruptions; it is alterative in doses of from one to five grains, emetic in from ten to thirty; *Podophyllum peltatum*, an herbaceous plant, the dried root of which exhibits the same properties and apparently the same strength as jalap; the resinous principles may be separated in the form of a dry, amorphous mass, and is termed podophyllin.

ART. IX.—*The Essentials of Materia Medica, Therapeutics, and the Pharmacopœias. For the use of Students and Practitioners.* By ALFRED BARING GARROD, M.D., Professor of Materia Medica and Clinical Medicine in University College, London, and Physician to University College Hospital.—*London*, 1855. Small 8vo, pp. 282.

THIS excellent little book is essentially a class-book, and as such will be found very useful to those who, especially among students, are likely to be bewildered by the larger compendia that treat of materia medica and therapeutics. It would, however, require some additions, in order to adapt it for the Scotch or Irish student, as little account is taken of the Pharmacopœias of the sister countries. We probably express a very general wish felt by the profession in saying, that we hope a 'Pharmacopœia Britannica' may be in existence before a second edition of Dr. Garrod's book is called for, and thus render unnecessary the addition of all the variations contained in the existing Pharmacopœias.

ART. X.—*Introductory Lecture, delivered to the Class of Military Surgery in the University of Edinburgh, May 1st,* 1855. By Sir GEORGE BALLINGALL, Regius Professor of Military Surgery.—*Edinburgh*, 1855. pp. 44.

IT is gratifying to receive the testimony of so distinguished a representative of the military department of medical science as Sir George Ballingall, to the ability and foresight of Dr. Andrew Smith in reference to the present war, and to find it to be his opinion, with which we coincide in the main, that the only defect of the medical department consisted in an excess of good nature in mixing itself, or permitting itself to be mixed up, with the duties, and saddled with the responsibilities, of another department. Sir George specially alludes to the purveyor's department. It is manifestly unjust to expect one man efficiently to attend to the sick soldier, and at the same time to provide the supplies, either of food or physic.

Sir George implies that the excess of good nature prevented the medical officers from acting with that independence, and raising their voices against abuses, with that energy which they might have employed, not only with justice to themselves, but with a certainty of the legitimate object being gained.

> "In former days," he says, "I have known a successful representation to issue from the weakest voice in the department. I have known an assistant-surgeon of three years' standing to bring down the censure of the Government upon the medical storekeeper at one of the Presidencies of India, for hesitating to supply him at once with the articles he required. The young man's requisition was sent back to him for amendment, but instead of doing this, he stated the facts to his commanding officer, saying that the requisition should stand upon record, and if the storekeeper was unable or unwilling to comply with it, it was for him to say so, and to state the reason why. The colonel, who at the time commanded a large army about to take the field, galloped off with the correspondence to the Governor, by whom the storekeeper was reprimanded, and the medicines were in camp in less than twenty-four hours."

Had such moral independence always characterized all the members of the profession, in or out of the army, we should not now hear of the "degradation" of the profession (a term to which we, however, do not subscribe), nor would the occasion have been given for the introduction of the civil element into the army, which Sir George loudly deprecates.

We cannot enter further into Sir George's treatise than to express our admiration of that generous feeling with which he speaks of eminent men of an earlier as well as a more recent date, in the military medical service of our country.

Art. XI.—*Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of* 1853. (Published by authority of the City Council of New Orleans.)—*New Orleans*, 1854. 8vo, pp. 542.

In consequence of a severe outbreak of yellow fever in New Orleans in 1853, the municipality took alarm, and established a sanitary commission, of which Dr. Barton, the main author of the above Report, was the chairman. The commission, as soon as the pressure was removed, was, as we have been given to understand, allowed to expire, but fortunately not before the Report, which is a monument of zeal and industry, was published. We now only give the title and advert to it, in order to introduce it to the notice of all sanitary reformers, lay and medical; as the facts and deductions accumulated in the volume, though immediately bearing upon yellow fever, have a much wider range. Neither time nor space will allow us to attempt even a summary of its contents at present, but we hope ere long to lay before our readers an analysis of the labours of Dr. Barton and his coadjutors.

PART THIRD.

Original Communications.

ART. I.

On the Varieties and Metamorphoses of Chronic Pulmonary Tubercle. By C. RADCLYFFE HALL, M.D., F.R.C.P.E., Physician to the Hospital for Consumption, and to the Institution for Ladies with Diseases of the Chest, Torquay.

(*Continued from No.* 30, *p.* 501.)

EXCLUDING the results of softening, pulmonary tubercle is found in different instances under the following forms:—Bayle's semi-transparent granulation; miliary grey semi-transparent tubercle; grey tubercle in various shades of whitish or buff opacity; circumscribed yellow tubercle; circumscribed gelatinous tubercle; diffused infiltration of tubercle, grey, or yellow, or gelatinous.

Bayle's Granulations.—Considerable discussion respecting the exact nature of the granulations described by Bayle might probably have been spared by mere reference to his original account of them. Bayle designates his second species of phthisis "Granular Phthisis." He states that in this form of the disease—

> "The lungs are stuffed with transparent, shining, miliary granulations, which appear to be of a cartilaginous nature and consistence. Their size varies from that of a millet seed to that of a grain of wheat; they are never opaque, and they do not dissolve. These different characters perfectly distinguish them from miliary tubercles, which are of the same size, *but which are always grey or white and opaque*, and in the end totally dissolve."*

This granular form of phthisis constituted more than a fifth of Bayle's cases, being found in 183 out of 900 autopsies of phthisis. It is evident that under this designation Bayle includes every instance in which he found the bright, transparent, hard nodules in the lungs, whether these were associated or not with other forms and conditions of tubercle. In each of his fully narrated cases in which the granulations existed alone, death resulted either from hæmoptysis or from pulmonary congestion, or else from some cause foreign to the tubercles. In every case in which the death was due to phthisis pursuing its more ordinary course, the granulations were in company with common tubercle. It is clear that an object which Bayle met with so frequently as once in every five inspections must be something with which we are sufficiently familiar.

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* Recherches sur la Phthisie Pulm. Paris, 1810.

Bayle's statement contains an observation and two opinions. The one opinion is that the nodules appeared to be of a cartilaginous nature. By this we need scarcely now understand more than that in density these granulations were almost as hard as cartilage; just as elsewhere Bayle several times observes, "they are very hard, like small hailstones." At that time it was customary to view every morbid product which was at all like cartilage in density or other outward character, as an accidental cartilage; and, indeed, by the naked eye it is difficult to distinguish the condensed fibrous tissue around a concretion or an old abscess from some forms of cartilage. The other opinion is, that the transparent nodule never softens; although it is also stated, that "these miliary granulations occasion at length ulcerations of the parenchyma of the lungs." More recently, Rokitansky likewise asserts emphatically that his "simple fibrinous tubercle—grey tubercle, the grey tuberculous granulation of Laënnec"—never softens.*

Putting aside these opinions, what is the nature of the objects really observed by Bayle? The answer, as given by Broussais, is, minute pulmonary lymphatic glands, enlarged! Andral considered them to consist of old inflammatory exudation, which had originally been poured out into distinct air-vesicles (vesicular pneumonia); and he imagined that they had passed through a previous stage of softness and redness.† He was evidently misled by the appearances seen by him in the lungs of glandered horses, which he everywhere confounds with true tuberculous disease. The vesicular pneumonia of infants does, it is true, at first glance, appear like disseminated tubercles, but the resemblance is to crude yellow softening tubercles, and not to transparent hard granulations. Rokitansky remarks, that when croupous pneumonia leads to vesicular tuberculous infiltration, "it is the same thing as Bayle's pulmonary granulations, regarding which there has been much discussion."‡ And in an excellent recent work, 'On Pathological Anatomy,' the granulations of simple hepatized lung in non-tuberculous pneumonia are styled "Bayle's granulations."§

The confusion has arisen mainly from Bayle having carefully distinguished his "miliary granulations" from his "miliary tubercles" of the same size. He appears to have called "tubercles" such nodules only as were somewhat opaque. "Tubercles," he says, "are always opaque, of a white or dirty-white colour, at one time yellowish, at another greyish." A seed-like transparent nodule, which we should now designate a grey miliary tubercle, before its stage of opacity, Bayle would not probably have called a tubercle at all, but a "semi-transparent granulation." What we should now style a more or less opaque grey tubercle, is Bayle's tubercle *par excellence*. Laënnec considered Bayle's granulations to be tubercles in their earliest stage; and pointed out that in Bayle's fourth case many of these granulations, which "were more numerous in the superior lobes than in the inferior, and resembled small grains of hail," were opaque in the centre.

By Bayle's granulations, then, is to be understood nothing more than isolated, often thickly-disseminated, grey tubercles, which happen to be unusually small, hard, and transparent.

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* Sydenham Society's edition, vol. i. p. 296.

† Clinique Médicale, tom. ii. pp. 10, 11.

‡ Vol. iv. p. 103.

§ Jones and Sieveking's Pathological Anatomy, p. 422.

Miliary Grey Tubercle.—Seed-like semi-transparent grey tubercles may be found in small number in the apex of one or both lungs, the remainder being free from tubercle; or, strewn as innumerable isolated grains throughout a great portion of one or both lungs; or, clustered in several close groups. Whether disseminated or grouped, the tubercles may appear to be immediately surrounded by healthy lung. It is crepitant, and neither inflamed nor discoloured. On the other hand, grey tubercles may occasionally be found set in lung solidified by any form of inflammatory exudation. Semi-transparent grey tubercles in apparently otherwise healthy lung, without any other kind or stage of tubercle co-existing, are only met with in cases where the patient has been cut off by something foreign to the tubercle, as, for example, in the case of death by accident. Still, in most autopsies of phthisis, specimens may be found of grey tubercles in portions of lung comparatively normal.

Each miliary grey tubercle comprises several air vesicles filled with transparent tubercle, the divisional walls of the vesicles yet remaining cemented in the tubercle. The tuberculous material itself presents cells in great abundance, and free molecular matter in small quantity. Compound tubercle cells and tubercle corpuscles abound, and are set in a homogeneous matrix; the whole, both cells and matrix, having the transparency of isinglass. In the smaller, harder, and more transparent forms (Bayle's granulations), the matrix possesses a faint fibrillation, and contains proportionably fewer cells (see Fig. 10).† It never forms real fibres, and its fibrillation of surface is quite distinguishable from the bold pulmonic fibres. In the adjoining air-vesicles the epithelium has undergone the degenerative changes already described, but the oil-dots are smaller and more equal, and less altered by ether, than in the case of yellow tubercle.

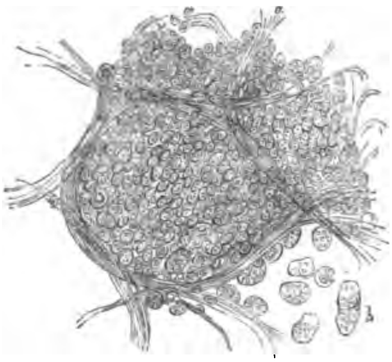

Fig. 13. Semi-transparent grey tubercle.

a. Pulmonic fibres seen distinctly amidst the glassy tubercle cells, and surrounding them.

b. Fatty epithelium and compound tubercle cells, loose near margin of tubercle.—(Mills.)*

When a semi-transparent tubercle becomes opaque, it assumes a dull white colour, but does not at first lose anything of its firm resistancy. Nor as it proceeds towards softening does it become cheesy, like the tubercle which is yellow from the first. Examined when opaque but still solid, the whole tubercle has lost its transparency, the cells of every kind have largely disappeared, oil-like molecules are very abundant, and the pulmonic fibres no longer perfectly mark out the natural arrangement of the air-vesicles, but are spread out and partially broken up amongst the rest of the tubercle.

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* The names attached to several of the woodcuts refer to the cases by which the specimens drawn were furnished. They are sufficiently altered from the real names to escape recognition.

† British and Foreign Medico-Chirurgical Review, No. 30, p. 493.

Primarily Yellow Tubercle.—Tubercle, yellow from the first, is found in small and seed-like deposits (miliary yellow tubercle); or, in distinct circumscribed masses, commonly larger than the largest circumscribed grey tubercle; or, in irregular masses larger still. It is dull, opaque, of a whitish-yellow, or a canary-yellow colour; tough and leathery in consistency; or unctuous and cheesy; or dry and friable. Less hard than grey tubercle at any period short of actual softening, it is more readily examined with the microscope.

Yellow tubercle consists of numerous tubercle cells, both compound and single, but the latter greatly predominating; and of a large proportion of free granules, in a homogeneous matrix; the whole being muddy or nebulous. The pulmonic fibres are less noticeable than in grey tubercle, owing partly to the want of transparency, but chiefly to the greater readiness with which a minute portion of yellow tubercle can be removed from the texture of the lung for the purpose of examination. Whilst the yellow tubercle is still consistent, tough, and leathery, the pulmonic fibres do exist within it; but under examination they pass to one side of the object-glass, and the tubercle elements to the other, and thus occasion the latter to appear less connected with the former than had really been the case. When the tubercle has become cheesy, either dry and friable or moist and soft, the pulmonic fibres have disappeared, and the molecular matter has increased.

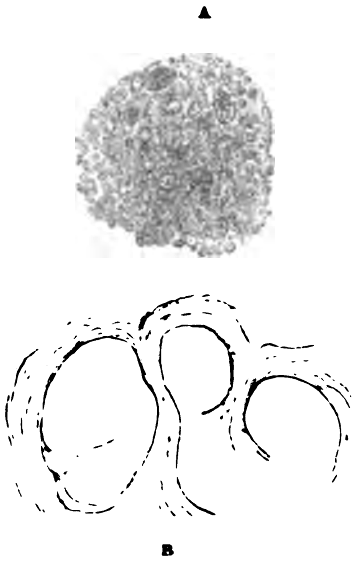

Fig. 14. Crude yellow tubercle.

A. Tough, brittle, structureless matrix. Free granules more abundant, large cells less so, than in grey tubercle. Tubercle-corpuscles non-nucleated, and more thickly studded with granules. Whole more opaque. (¼-inch power.)

B. Skeleton fibres of pulmonic air-vesicles in midst of crude yellow tubercle, tubercle-corpuscles having been floated off. (¼-inch power.)—(Ford.)

What is the relationship between the yellow and the grey tubercle? Two opinions are current. The one more generally adopted is that of Laënnec, that "there is no other difference between them than that which exists between the green fruit and the ripe." The other is that most prominently supported by Rokitansky, who declares positively and impressively that grey tubercle never softens; that when it appears to do so, it is really because yellow tubercle has been mixed up with it, and that this yellow portion alone undergoes softening. Consequently, that the grey tubercle never changes into the yellow. Bayle also, as we have seen, believed that his grey semi-transparent granulations never softened. It may be at once conceded that grey semi-transparent tubercle never does liquefy *as such*. It invariably loses its transparency, and assumes more or less of a dull whitish colour before it softens. Upon the following points every one is agreed. Tubercle may be yellow from the first, or grey and semi-transparent at the first. Sometimes (though very rarely) no other than grey semi-transparent tubercles exist. Commonly, in the same lung with the semi-transparent tubercles we find others which are partially transparent, partially opaque; others which are opaque throughout; some which are softened in the centre, the softening being invariably surrounded by opaque, never by transparent, tubercle; and others again entirely liquefied. Now the moot point is this: is the opaque part of a grey tubercle about to soften really yellow tubercle laid down as such at the first, or so laid down in the substance of the grey after its first formation; or is it merely the grey tubercle itself undergoing a certain kind of molecular change? And, if we assume the latter, does grey tubercle when so changed become precisely the same as primary yellow tubercle? The reply to the last question, if in the negative, will at once decide the first two. Grey tubercle may become opaque, and soften, and yet be a different product all the time to primary yellow tubercle. I believe that this is the fact, and chiefly because the primary yellow tubercle and the opaque portion of grey tubercle are not quite identical. Yellow tubercle always becomes moist and cheesy before it liquefies. Opaque-grey tubercle does not, but remains tough and coherent up to the moment of softening. Softening yellow tubercle is more unctuous. The pulmonic fibres remain intact and entangled amongst the tubercle elements later in the opaque-grey than in the yellow tubercle. The yellow tubercle is, from first to last, more fatty than the other. It is in the air-vesicles adjoining primary yellow tubercle that the fatty degeneration of the epithelium is most characteristic. The oil-dots are larger, more unequal in size, and are more cleared up by ether than in the case of grey tubercle. When an opaque-grey tubercle is about to soften, but is still tenacious and tough, it presents a multitude of even-sized small oil-like molecules, which brighten on the application of ether, but are without ether less nebulous than the molecules which abound in softening yellow tubercle.

Thus, the difference between a tubercle yellow from the first, and an opaque tubercle which was in the first instance transparent, is, that the yellow tubercle is yellower than the other, less resistant, and in composition more fatty and molecular.

Rokitansky's opinion, so often and so positively reiterated, that grey tubercle never becomes opaque and never softens, is open to question by inference from his own remarks. He states, "The combination of grey with yellow tubercle is frequent. Where, in this combination, the *latter* passes into softening, the grey tubercle, like textures in contact with tubercle-pus, *becomes destroyed*."* He admits, therefore, that grey tubercle may undergo a kind of destruction, which can only take place by liquefaction and molecular disintegration; and this, when effected slowly, is precisely the process of ordinary tuberculous softening. He also states that a grey tubercle may become "a partly cornified, partly ossified nodule." Now, when a tubercle becomes calcified (for, of course, the term ossified is only a verbal inaccuracy), its elements have previously undergone a molecular change of the same nature as that which in another instance, where it takes place more rapidly and is attended with more liquefaction, constitutes common softening. In these two facts, therefore, as stated by Rokitansky—viz., that grey tubercle can "become destroyed," and can undergo cretifaction—there is evidence in favour of its capacity for softening, and if so, of its capability of undergoing the antecedent alterations of opacity and change of colour. On the other hand, there is nothing but the assertion adduced in support of the opposite opinion, that all softening of grey tubercle is due to its concurrence with primary yellow tubercle. One cannot help suspecting that even such an observer as Rokitansky, in supposing that the fibrinous tubercle, as he terms it, never softens, may unconsciously have been biassed by his theory of the necessary connexion of tubercle with fibrinosis. "In the first place," says he, "the groundwork of rapidly solidifying tubercle blastema is, without the least doubt, fibrin. Again, in the two cardinal forms of tubercle, it is easy to recognise the two principal forms of fibrin, the simple and the croupous."* Accordingly, he styles grey tubercle the fibrinous; yellow tubercle, the croupo-fibrinous variety. And no doubt there is some broad analogy between the grey and yellow forms of tubercle respectively, and the two leading varieties of exudation-lymph (the nucleated-blastematous, and the corpuscular), so fully described by Mr. Paget.† In grey tubercle, there is more of blastema; in the yellow, more of low corpuscular forms. As the blastematous lymph is better than the corpuscular, so in all probability is grey tubercle a less degraded form than the yellow. It has long been a prevalent opinion, that yellow tubercle betokens a more intense cachexy than the grey. Against it may be urged that we sometimes find tuberculization more universal throughout the body when grey tubercle predominates, than when yellow. But this is readily explained away by the greater chronicity of grey tubercle having allowed time enough; whereas copious yellow tuberculization of the lungs usually kills quickly. When its course is sufficiently chronic, there is no want of extension to other organs on the part of yellow tubercle.

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* Syd. Soc. ed., vol. i. p. 203.

The twofold condition of the part affected and of the constitution, never to be disunited in our reasonings upon phthisis, must be considered to be as influential in governing the exact kind of tubercle deposited, as it is known to be in the instance of common exudation-lymph. It is therefore only what we should expect, seeing how greatly in the course of chronic phthisis both the nutrition of the lungs and the state of the constitution are modified, that the majority of cases would present several forms of tubercle in correspondence with the several stages of the disease. This is so. In an average instance of tuberculized lung, we see enough to justify our assuming that tuberculization has taken place in the following order:—First series in point of time: Miliary grey semi-transparent tubercles; firm opaque-grey tubercles; the same beginning to soften. So far no inflammation. Opaque-grey tubercles in more advanced stage of softening, now surrounded by inflamed lung. Grey glistening induration, with or without miliary grey tubercles here and there strewn through it; perhaps, a certain number of small tubercles, hard, and partially, or completely, calcified: others softer, white, and mortary. Cavities.—Second series:—Miliary or larger-sized distinct yellow tubercles, surrounded by inflammation; or, masses of the same. Cavities.—Third series:—Jelly-like circumscribed tubercles, or diffused gelatinous infiltration.

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* Syd. Soc. ed., vol. i. p. 298.

† Lectures on Surgical Pathology, vol. i.—The affinity between lymph and tubercle is drawn much more close by Dr. C. B. Williams. See Principles of Medicine, art., Tubercle.

The two varieties are variously combined, but in most autopsies of phthisis one is able to say that either the grey or the yellow tubercle predominates.

Can we anticipate this information during the life of the patient? The reply must be furnished by a comparison of symptoms with *post-mortem* appearances. I suppose the following remarks to express the probable truth; they at least serve to represent facts which have occurred in the coincidence stated.

In one case (*grey tubercle*) we shall find a few large cavities, or one immense cavity and several lesser ones; multitudes of miliary grey tubercles; large patches of grey induration; gelatinous infiltration; local spots of œdema and emphysema; plenty of opaque tubercles, but none cheesy or quite yellow. Working remnant of lung in state of purple congestion. Most of the lung externally iron-grey in colour, very contracted, and pimpled over, where not adherent, with semi-transparent nodules.

In another case (*yellow tubercle*) we shall find the lungs less contracted generally, bulged forward in places by masses of yellow tubercle; elsewhere, dotted over with yellow nodules, having purple or damask lung between. Cavities less ample but more numerous; and evidences everywhere of more acute and diffused pneumonia.

In a third case, in which the stress has fallen upon the abdominal viscera, there may be only the miliary grey tubercles present in uninflamed lungs; whilst there is yellow tubercle in Peyer's patches.

When grey tubercle predominates, there is more frequently transparent tubercle dotting the peritoneum, and merely an unduly nebulous or granulous state of the hepatic and renal cells.

When yellow tubercle predominates, there is more commonly fatty hypertheliation within the bronchial and mesenteric glands; perhaps more connexion with enlargement of the external lymphatic glands; and fatty heart, liver, and kidneys. This last remark is not intended to express any positive rule. We see cases in which there is fatty kidney with no other than grey tubercles in the lungs; and others, in which yellow tubercle exists in the lungs without fattiness of either liver or kidney. I merely wish to imply a qualified affinity. Whether the lardaceous disease of liver and kidney bear any relationship to one rather than to the other type of pulmonary tubercle, is not ascertained. However unprecise it may be to assert that the cells of the liver are rarely found to be quite healthy in appearance in chronic phthisis, if no abnormal exudation or marked alteration of structure exist to substantiate the opinion, I nevertheless believe that in those cases in which the liver, to the naked eye, seems healthy—being neither fatty nor lardaceous, inflamed, greatly congested, nor tuberculized—the hepatic cells will still habitually be found more loaded with their ordinary contents than in the perfectly healthy liver.

When consumption is very chronic, for the most part non-febrile, attended with considerable retraction of chest, and with very slow, alternating, but eventually progressive emaciation, and is comparatively free from severe pain of a persistent character, it may be predicted that grey tubercles predominate.

When consumption is more acute, febrile continuously, with little retraction of chest, attended with persistent pain and more hurry of breathing, it is probable that yellow tubercles predominate.

These, like all general remarks, require some qualification. We may see a case where there is intense shortness of breath, and yet the grey tubercles predominate. Here we have, generally, a large cavity in the lung, with a greatly congested and very inactive liver. A very small amount of pulmonary tuberculization, conjoined with an inactive liver, will be attended with more distress of breathing than far more extensive tuberculization where the liver does its duty well. The evil is augmented if at the same time the other supplementary respiratory organ—the skin—be dry and harsh. I have lately attended a case in which subacute hepatitis, with great dyspnœa, constant cough, and night sweats, occurring in a young lady aged nineteen, had been mistaken for acute phthisis. It was impossible to decide at the time that latent tubercles did *not* exist in the lungs, but the present good health of the patient negatives such an assumption.

Pain of chest, whether neuralgic or pleuritic, is generally more urgent before than after softening. It by no means necessarily indicates which is the worse side as regards the lungs. There may be sharp pain complained of on the left side, where, on examination, merely coarse respiration is heard; whilst over the right apex, where there is no pain, moist crackling is extensive. Slight local pleuritic irritation on the left side may be the cause here. But immense pleural adhesion may occur over large cavities when grey tubercle predominates, without occasioning any pain. Whereas, when yellow tubercle predominates, there may be less extensive adhesion, and yet constant severe pain of chest. The more acute nature of the inflammatory action, and the greater irritability of the nervous system, in the latter variety, will perhaps account for the difference.

To recapitulate:—We have reason to conclude that the grey semi-transparent tubercle can soften; that previously to softening it always becomes opaque; that this opaque stage is due to molecular alteration in the elements of the previously semi-transparent tubercle, and not to a combination from the first with yellow tubercle, nor to any subsequent addition of yellow tubercle; that the grey tubercle can thus proceed to complete softening without becoming precisely the same thing as primary yellow tubercle, although in both the process of softening is essentially the same; the difference being that primary yellow tubercle throughout its entire course, both before and after softening, has more of oil molecules and granular detritus than the grey tubercle.

Circumscribed Gelatinous Tubercles.—In acute phthisis we sometimes find distinct small deposits, of the consistence only of firm jelly, set in highly-congested lung. They consist of the same elements which we find in the more common gelatinous infiltration—viz., common grey tubercle-elements, in a more plentiful and softer matrix.

Tubercular Infiltration.—"Miliary tubercles," say Barthez and Rilliet, "by their agglomeration form masses as voluminous as those of infiltration, and it is only by the presence or absence of the surrounding vascular network that we can distinguish between the two."* Following Van der Kolk and Guillot, these authors describe miliary tubercles as being surrounded by a fine network of newly-formed vessels, which serves to isolate the tuberculous matter (which they consider it is probably destined to secrete) from the tissue of the organ. These vascular networks were imagined by Guillot to anastomose, through the intervention of pleural adhesions, directly with twigs from the intercostal and internal mammary arteries, and thus to surround each tubercle with arterial blood direct from the heart, in place of that normally found in the system of pulmonic capillaries, which, when such an obstruction as that of tuberculous deposit lay in front, would necessarily be venous. Unable to trace any such network around certain irregular masses, Barthez and Rilliet applied the term infiltration, as above, exclusively to these. But the whole, possibly with a few exceptions, is a fable. The only vascular arrangement to be found surrounding a miliary tubercle consists of the pulmonary capillaries pushed aside somewhat by the deposit, when this is primarily limited to a few connected air-vesicles. A more diffused exudation taking place, at once encloses all the vessels, and hence infiltration is not surrounded by any ring of capillaries. Rokitansky applies the term infiltration to yellow tubercle, which he considers to be formed in the cavities of the air-cells, whilst grey tubercle (according to him) is formed in the interstices of the pulmonary tissue. He uses the term interstitial as synonymous with grey tubercle; infiltrated as synonymous with yellow. Even were the anatomical facts correct (which I believe they are not), if we mean by infiltration the filtering of a morbid fluid into and amongst the elements of a texture, we should reverse this assignment, and call that tubercle infiltrated which was interstitial, rather than that which was only vesicular. In common use, the term infiltration of tubercle is vaguely employed to signify during life either the sudden supervention of acute phthisis, or else any rapid addition to solidification in a lung known or suspected to be tuberculous. And in morbid anatomy, it is customarily applied to any consolidation of lung of which tubercle forms a part.

Under this designation have thus been comprised—1. The sudden pouring out of tubercle into multitudes of air vesicles, with intense congestion and sanguineous œdema of the whole lung (acute tuberculization). In this case, strictly speaking, the term is a misnomer; serum being infiltrated, tubercle restricted to the air vesicles. 2. One or other kind of simple inflammatory exudation, extensively diffused around pre-existing tubercles. 3. A large mass of yellow tubercle monopolizing all the elements of the portion of lung affected. 4. A conjoined diffusion of tubercle-corpuscles, and some kind of simple exudation.

In a strict sense, every ordinary pulmonary tubercle is, to a certain extent, an infiltration, since, whilst it begins in the air-vesicles, it subsequently invades the interstitial tissue also. But so long as it is distinctly circumscribed, the term is not applied. By tuberculous infiltration, then, we understand every kind of diffused exudation in which tubercles are contained, whether in distinct nodules, or molecularly distributed throughout. Every kind of diffused consolidation in a tuberculized lung (pulmonary apoplexy excepted), is a consequence of inflammation. Tuberculous infiltration, therefore, is simply tubercle mixed up with inflammation-matter;* and its consideration necessarily involves that of the mutual relationship customary between inflammatory exudations and tubercle.

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* Maladies des Enfants, tom. iii. p. 8.

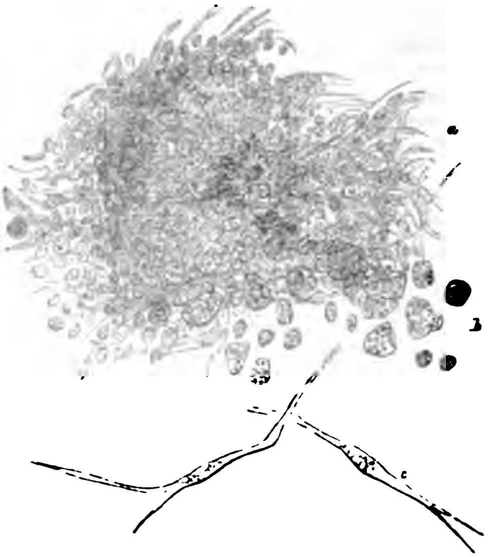

Fig. 15. Infiltration of grey semi-transparent tubercle, made up of fibrillated matrix, imbedding—

a. Numerous cells, compound and free, nucleated; few non-nucleated: few free oil-dots; few granules.
b. Glomeruli; fatty epithelium; compound tubercle cells, nucleated and non-nucleated cells; seen loose at margin.
c. Transparent fibrils, loose, in process of development.—(Weale.)

Diffused tuberculous solidification in the lung is grey and hard; yellow and firm; or soft and gelatinous.

Grey Infiltration is found in large smooth patches, contracted, tough, shining and slippery. To the naked eye, the texture of the lung in it is lost; but under the microscope, the pulmonic fibres are seen cemented irregularly in the semi-transparent exudation. The air-vesicles have lost their normal form and relative arrangement, and are represented only by the interspaces which exist amongst the bands of fibres. Distinct tough grey miliary tubercles may or may not be sparsely or thickly present, and may stand out in relief on cutting through the part; but neither by section nor by tearing can any other kind of granulousness be produced. A thin colourless serum bedews the part. No bloodvessels and no bronchial tubes remain pervious within the centre of the mass. Small bronchial tubes may be pervious for a short distance beyond the margin of the induration. If so, this part of them contains muco-pus, and has its lining membrane thickened, softened, and sometimes reddened. The lung bordering the grey induration may be in the state of red hepatization, but more frequently it is crepitant, mottled black, blue and red, and only moderately congested.

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* See Cyclopædia of Anatomy and Physiology, vol. iii. p. 105, art., Products, Adventitious, by Dr. Walshe.

In the midst of the semi-transparent induration, we may find distinct grey miliary tubercles; the same in their opaque stage; spots of cheesy-yellow tubercle; and *depôts* of softened tubercle. Or, on the contrary, a portion of tuberculous lung may be glazed with grey, smooth, semi-transparent induration, in which no distinct tubercles are to be seen.

In the lung which is close adjoining grey induration, we find nebulous and fatty epithelium; compound tubercle cells and granule cells. In the induration itself, a translucent homogeneous, or else faintly fibrillated matrix; a few fibre cells, partially developed; the pulmonic fibres matted together; and numerous cells of various sizes, nucleated and unnucleated. Of these cells, many are the round granule cells, only differing from those common to all inflammatory lymph in being more transparent. When distinct tubercles lie in the induration, they add, of course, their own proper microscopic appearances.

Yellow Infiltration is also found in large irregular patches, but is not contracted, and is less tough than the grey; being rather unctuously tenacious, or even friable, than firm, coherent, and resistant. The adjoining lung is always inflamed, and is more commonly the seat of local red hepatization than is the case around grey infiltration. We find a greater abundance of free granules and of oil molecules; of cloudy granule cells and tubercle corpuscles. The matrix is never fibrillated. The pulmonic fibres are less distinct, and apparently in part destroyed. The whole is semi-opaqu and muddy.

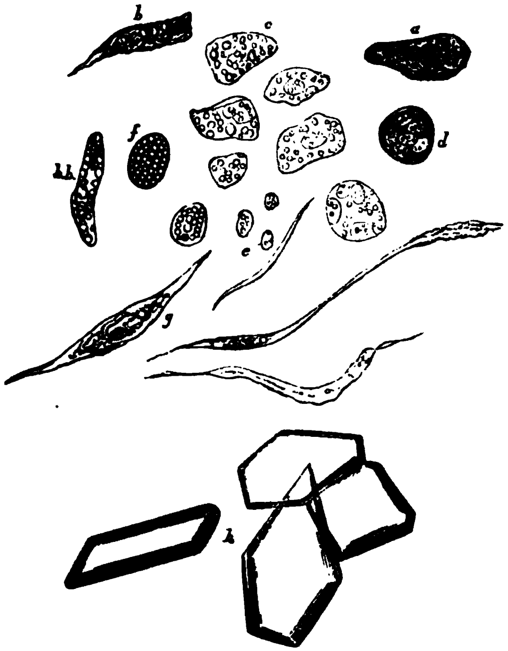

Fig. 16. Gelatinous infiltration.

Constituent elements of grey, firm, gelatinous tubercle.—(Gedd.)

a. Large pigment cell.
b. Bronchial epithelial cell, with its cilia extended, and so cemented in the matrix.
b. *b*. Bronchial epithelial cell, fatty.
c. Fatty vesicular epithelial cells.
d. Compound tubercle cells.
e. Simple tubercle-cells.
f. Glomerulus.
g. Fibre cells, developing.
h. Bright colourless crystals.

Gelatinous Infiltration is pink, yellowish, or ash-coloured. The part of lung thus affected is smooth and softly solid, but compressible. The air-vesicles within it retain their normal arrangement, excepting where miliary tubercles happen to exist; or it may be softly nodulated, from the gelatinous exudation being firmer in some of the air-cells than elsewhere. The colour depends merely upon admixture with extravasated red blood-globules, and their dissolved hæmatin. That regarded, we find indifferently in the reddish and the more colourless forms the same objects: fatty epithelium; compound tubercle cells; tubercle corpuscles; pigment cells; granule cells; fibre cells; and sometimes crystals of mineral salts.

These tuberculous infiltrations follow the usual rule of parenchymatous inflammations, by which an acute inflammation tends to soften, a chronic one to harden.

As to their *rationale:*—In the dense, contracted, glistening, grey induration, we see a very chronic inflammatory exudation of common induration-matter, variously mixed up with the elements of tubercle. We may assume that the plasma, when exuded, is very contractile, and but slightly tuberculous: more blastematous than corpuscular.*

In yellow infiltration we see an acute inflammatory exudation; the plasma not contractile, largely tuberculous; more corpuscular than blastematous.

In gelatinous infiltration we see an acute exudation, but thinly adhesive. An effusion of liquor sanguinis, too feebly contractile for the lymph to separate itself as much as usual from the serum. We may infer that it is poured out rapidly, and that it coagulates to its jelly-like extent immediately, from the fact of the chance-setting within it of a bronchial epithelium cell, with its cilia extended (*b*, fig. 16). The plasma is poor in fibrin, but more blastematous than corpuscular.

Greyness indicates that the exudation is chronic; yellowness that it is acute; jelly-like consistency that it is recent. We constantly find specimens of all in the same lung. The grey induration is the oldest, and represents the earliest reactive response to the irritation occasioned by the tubercles, whilst there is yet a fit state of blood to pour out contractile plasma. The yellow infiltration points to a deteriorated condition of blood, and to more of local inflammatory process. It occurs subsequently to the grey, when the progress of the disease had injured the constitution more. The jelly is a still later exudation, and takes place towards the close of life, when the blood has become poor in every respect, and unable to furnish firmly-coagulable lymph.

Besides these tuberculous infiltrations, in which the tubercular matter is intimately associated with inflammatory exudation of a characteristic kind, tubercle has also an habitual relationship to common forms of inflammation.

A grey miliary tubercle, whilst yet semi-transparent, may be surrounded by lung which is quite permeable, crepitant, and to the naked eye healthy. Examined by the microscope, the small bloodvessels around it are somewhat enlarged and clustered, as if they had been obstructed and pushed outwards by the tubercle.† The adjoining vesicles present the appearances already dwelt upon. There is no evidence of inflammation here.

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* By blastematous, is here implied only a tendency on the part of the exudation to form induration matter. It may not be quite a correct use of the word.

† See. for illustrations of this, p. 443; and, for the explanation, p. 425, in Jones and Sieveking's Pathological Manual.

A miliary tubercle may become opaque, and still have no inflammation around it; it may also undergo centric softening without inflammation; but as a rule, when softening commences in the tubercle, inflammation around it, if absent hitherto, commences likewise. Perhaps we never find a distinct tubercle softened throughout to complete liquefaction without its being surrounded by a zone of lung affected with inflammation in some shape. What is true of one miliary grey tubercle is equally so of groups. Innumerable distinct grey tubercles may be strewn throughout a lung, or a portion of lung; and for so long as they remain small, distinct, and unsoftened, there may be no inflammation. This does not apply to primary yellow tubercle. We never see them thickly strewn without some kind of attendant pneumonic change. An ancient yellow tubercle, it is true, may be found capsulated by a fibrous envelope, around which the lung is all but healthy. Here there was once a certain amount of inflammation—small in amount and adhesive in type, it may be,—although there is none now.

When grey miliary tubercles are thickly clustered in groups, local chronic inflammation is common around each group, in the form of grey semi-transparent induration. When grey tubercles become entirely opaque, and still more constantly, when they begin to soften, if not already enclosed in a patch of grey induration, they are now surrounded each singly by a circle of red hepatization, the intervening lung remaining crepitant and not congested, or greatly congested but still permeable ; or a portion of lung including numerous distinct tubercles may be entirely in a state of red hepatization. Hence, when a number of distinct tubercles are softening, we have co-existing either an equal number of local miniature pneumonias, or else one patch of pneumonia including all the softening tubercles.

As a softening tubercle increases in extent, it never does so at the expense of healthy lung, but always of lung which has previously been the seat of inflammatory exudation. To the ravages of softening lung, therefore, inflammation is quite essential. Our first indication of commencing softening—the fine moist crackle—is due to hypersecretion in the minute bronchial tubes close around the tubercle, which is occasioned by local inflammation.

The process by means of which, out of a series of neighbouring tubercles, a large vomica is formed, is as follows:—The intervening lung inflames, and assumes some form of solidification,—if slowly, the grey induration; if quickly, red hepatization; ordinarily the former. Molecular disintegration and liquefaction of the solidified lung ensue, and so permit the already softened separate tubercles to open into each other, and form one common vomica. As long as the exudation seals up the bronchial tubes, so long does the vomica remain closed. Such a closed vomica, full of pus-like fluid, may be found the size of a walnut. After a time, by the same destructive agency which has enlarged the dimensions of the vomica, a bronchial tube is opened into, and a cavity results.

Where there is a number of minute local pneumonias, there is usually bronchitis in local patches only. During the sleep of miliary grey tubercles there is not, as a rule, any bronchitis or any pleurisy whatever. When the grey tubercles begin to soften, they begin to irritate; they now become "thorns in the flesh," usually not before. The reactive local inflammations correspond.

Pleurisy, when spontaneous in its origin, whether general or in local patches, usually responds only to the presence of tubercles near to the pleural surface of the lung. If a superficial tubercle is small, not prominent, and is semi-transparent, the pleura over it may remain bright and unaltered. If the tubercle projects, or becomes opaque, the pleura over it is white and thickened. Any irritation beyond this is shown by every possible degree and result of pleurisy, with or without the deposition of tubercle in the adhesions which may be formed.

The bronchial tubes and the pleura near to primary yellow tubercle are never found free from inflammation.

The morbid anatomy of the bronchial tubes in tuberculized lungs may be briefly summed up by saying, that every result of bronchitis is found; the only peculiarity being, that as long as the tubercles allow a large portion of the lung to remain healthy, so long is the bronchial inflammation more or less limited to the vicinity of those tubercles which are pursuing the destructive course. There may be great local destruction of lung without extension of bronchitis beyond the immediate neighbourhood of the part.

In that part of the lung which is free from tubercles we may find, to any extent, congestion, œdema, emphysema (partial), red granulous hepatization, red smooth solidification, or grey hepatization. These possess no peculiarity in their anatomical features dependent on their occurrence in a tuberculous lung.

With non-inflammatory simple blood-exudations the connexion of pulmonary tubercle is small. Pure blood is found in the air vesicles in the rare instance of death during hæmoptysis. Such extravasation of blood, with laceration of lung tissue (pulmonary apoplexy), is very rare. Clots of blood have also been found in cavities. Point-extravasation of red globules interstitially in some part of the lung tissue, or in the tubercles, is never wanting. Of simple fibrinous effusion without inflammation in a tuberculous lung, there is no proof. Passive œdema it would be difficult to distinguish from the common active kind.

Destructive Course of Chronic Pulmonary Tubercle.

The *destructive course* of tubercle comprises degenerative changes in the tubercle itself, inflammatory action in the tissues adjoining, and, commonly, increased deposition of tubercle.

Softening.—After remaining passive, or quietly increasing in magnitude, for an indeterminate period, varying probably from a few weeks to an unknown number of years, tubercle begins to soften. Semi-transparent tubercle first becomes opaque, and remains opaque and firm for an indefinite time. It next loses its firmness, usually first in the centre, which assumes a buff colour, and by degrees softens to the consistence of thick paste. Liquefaction proceeding, the whole tubercle changes into a creamy fluid, and the softening is complete.

During the firm opaque stage of the grey tubercle, in the tubercle cells we discern no other change than that they are no longer as translucent as before; but in their relative proportion to the free molecular matter there is a marked difference. Compound tubercle cells are few. Tubercle

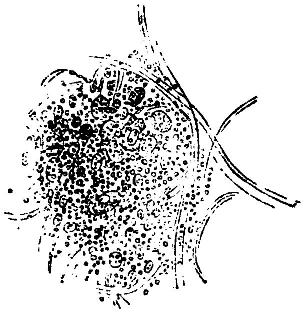

Fig. 17. Grey semi-transparent tubercle, turning yellowish-white—i.e., opaque-grey tubercle. It presents—

More free oil molecules;
More free granules and free tubercle cells;
Fewer compound cells;
Fewer nucleated cells than in the semi-transparent stage;
Still tenacious and tough—not in least cheesy.—(Weale.)

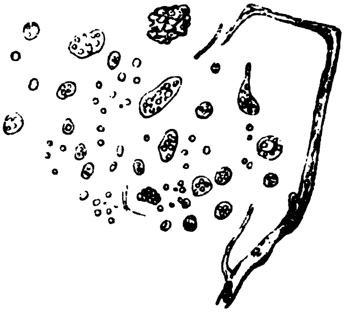

Fig. 18. Opaque-grey tubercle, liquefied. From creamy fluid in a small closed cavity. (Size of a pea.)—(Weale.)

A few shreds of fibrils;
A bit of loose bloodvessel with shrivelled red globules in it;
Free fat dots and granules;
Various forms of cell, all fatty—a few tubercle corpuscles;
The whole loose and floating;
No well-formed pus cells.

corpuscles are also less abundant. Oil molecules abound. The outlines of the air vesicles are imperfectly traceable. The matrix is still tenacious and tough, and the whole moves together under examination.

In the pulpy stage the matrix has become liquid, the pulmonic fibres are broken into lengths, and molecules are still more abundant.

In the creamy stage, there is simply a larger proportion of fluid. We find shreds of pulmonic fibres; bits of small bloodvessel, either fattily degenerating, or merely shrivelled; various forms of nebulous cells; occasionally large fatty epithelium plates and compound tubercle cells, which have escaped disintegration; oil molecules and granules in abundance; and pus cells or not, according to circumstances. If there be communication with the air by means of an opening into a bronchial tube, pus cells are always found, and make up the bulk of the fluid; but when the vomica is closed, a small one (size of a pea) may not present any pus cells in the pus-like liquor which fills it. A large one (size of a walnut), though closed, I have invariably found to contain pus cells; and in addition there may be seen in the contained fluid bronchial epithelium, enveloped red blood globules, and pigment cells; bits of lung-fibres; dots of mineral salts, and small blood crystals.

If active inflammation surrounds the vomica, however small this may be, pus will be found in the liquor of the tubercle.

As crude yellow tubercle is opaque from the first, the first indication of softening in it consists in loss of firmness. It first becomes cheesy, then rough and creamy. Softening more rapidly affects the entire tubercle than in the grey variety, but occasionally when a yellow tubercle is large, the centre will be cheesy before the rest. A greater abundance of yellower oil molecules, and of free granules, fewer cells of any kind, excepting tubercle corpuscles, and a more muddy appearance altogether, are the microscopic distinctions. In the liquor obtained from centric softening, whilst it has

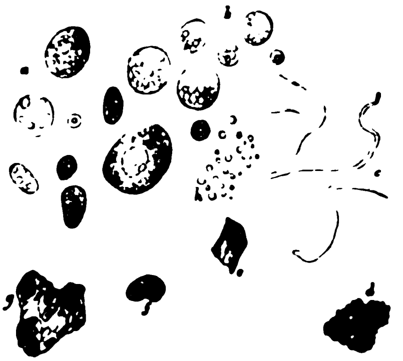

Fig. 19. Opaque-grey tubercle, liquefied. From creamy, thick pus-like contents of large closed cavity. (Size of a large walnut.)—(Weale.)

a. Granule cells } of all sizes.
b. Pus cells }
c. Bits of fibril. *d.* Mineral dot.
e. Orange blood crystal. *f.* Black pigment.
g. Enveloped blood globules.
h. Free fat and granules abundant.
No special tubercle cells.
Granule and pus cells and free fat most abundant of all; pus cells did not show the special pus nucleus with acetic acid. Otherwise with water not distinguishable from ordinary pus cells.

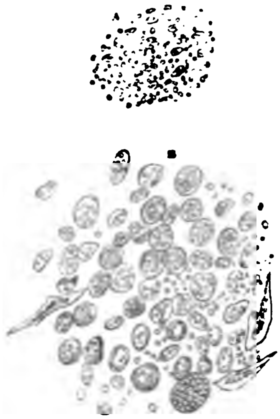

Fig. 20. Yellow tubercle, liquefied.

A. Creamy liquor from centre of otherwise crude yellow tubercle.—(Ford.)
No pus; no compound tubercle corpuscles;
Oil molecules, granules, and small tubercle corpuscles only.
B. Puriform liquor of large yellow tubercle, liquefied throughout.—(Ford.)
Pus cells; granule cells; columnar epithelium; oil molecules; free granules;
A few single tubercle corpuscles;
No compound tubercle cells.

not yet implicated the rest of the tubercle, and consequently not yet reached the lung tissue, we find only tubercle corpuscles, oil molecules, granules, and liquid; never pus nor inflammation globules. As soon as the entire tubercle has liquefied, and the softening has thus reached the lung tissue, we find pus and granule cells. (See fig. 20, A and B.)

The whole of a tubercle does not usually soften at once. Softening may commence at the centre; or at the periphery; or at both at once; or when the tubercle is large, it may commence in several points at the same time. The rule is, that when the tubercle is distinctly circumscribed, and no active inflammation exists around it, softening commences at the centre, and may be exceedingly slow in reaching the circumference. But when softening commences at the circumference, it does so in consequence of inflammation in the adjoining lung tissue, and is never long before it involves the entire tubercle. The irregular softening of diffused tubercle is due to the circumferential or centric commencement of softening in each or several of the small tubercles, of which the larger mass is but an aggregation. In every instance that part of the tubercle is first to soften which is first deprived of its means of support:—the part most distant from supply when the centre commences; the part most immediately interfered with by adjoining inflammation when the circumference.

A cavity may be of any size, from that of a pinhead to that of one capable of holding a quart; and may be surrounded by lung in every imaginable condition, excepting that of health. A thin filmy membrane may be all that separates it from the lung tissue; or its boundary wall may be a dense white fibrous layer, of very variable thickness. The surrounding lung may be merely congested, or hepatized, or, as is most common, it may be the seat of grey induration. However dense, white, and shining the wall of an old cavity may appear, it is never anything more than imperfect fibrous tissue, and is cartilaginous only in outward appearance.

It is generally stated that cavities enlarge chiefly by the melting down of fresh tubercle deposited in their wall. This is not quite exact. Fresh tubercle is met with either in the shape of distinct nodules, or of grey induration on the outside of the wall of the cavity, but in the actual membraniform wall we do not ordinarily find characteristic tubercle cells. Proceeding from within outwards, we find next to the contained fluid a layer of granule cells and fat molecules, forming a sort of pyogenic pavement epithelium. This rests upon a delicate network of fibrils, amongst the meshes of which more granule cells are freely scattered. The cells for the most part are nucleated. Some are pus cells, but the majority have not exactly that character. A few pigment cells, and blood globules, and glomeruli are sometimes found. Fibre cells, fine and transparent, are largely seen, more and more developed as they are farther from the cavity. When the neighbouring lung is permeable, it is separated from the outside of the fibrous wall by a layer of soft gelatinous exudation, which shades off into the tissue of the lung. More frequently, grey induration is in contact with the fibrous wall of the cavity.

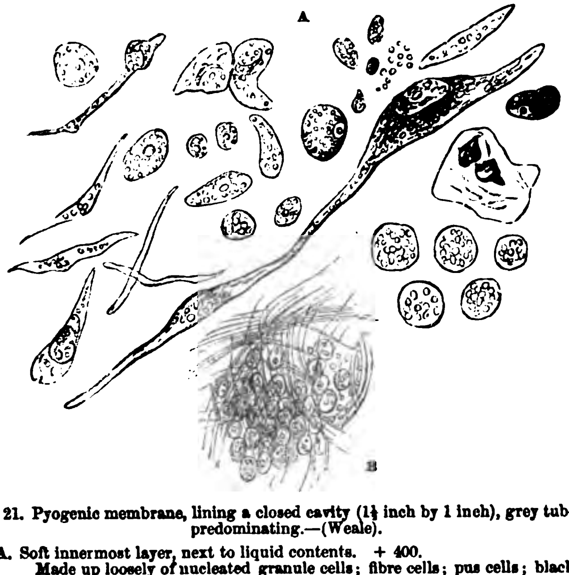

Fig. 21. Pyogenic membrane, lining a closed cavity (1¼ inch by 1 inch), grey tubercle predominating.—(Weale).

A. Soft innermost layer, next to liquid contents. + 400.
Made up loosely of nucleated granule cells; fibre cells; pus cells; black and orange pigment cells; blood crystals in filmy envelope; no specific tubercle cells.

B. Basement layer. + 250.
Made up of granule cells and free oil molecules and granules set in the meshes of soft fibrillation membrane.

The contents of an old cavity are little else than pus.

Beneath the soft pyogenic lining of a large cavity I have in two instances found bloodvessels in the state of fatty degeneration. In another instance I found fatty bloodvessels in a portion of distensible lung half an inch distant from any deposit of tubercle, the epithelium of the air vesicles here being fattily degenerate. This makes four cases in which I have now seen fatty degeneration of the small bloodvessels of the lung in phthisis.—Near to, but not mixed up with, tubercle, in one; within grey tubercle, on the enclosed wall of an air vesicle, in one; in the wall of a large cavity, in two.

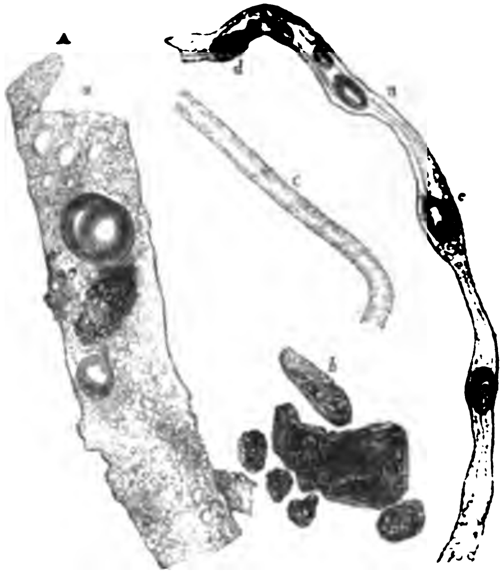

Fig. 22. Fatty vessels.

A. *a.* Bloodvessel in state of fatty degeneration, from wall of closed cavity next to pyogenic membrane. From 2nd case, slight hæmoptoë.
b. Exuded and clustered red blood globules, appearing as patches of brown and blue pigment, in the relative position to bloodvessel in which they are drawn.
B. *c.* Another small bloodvessel, in a state of simple atrophy, with fatty degeneration in places. From 3rd case, slight hæmoptoë.
d. A heap of blood globules within the vessel.
C. Small bloodvessel commencing fatty degeneration, epithelium fatty around it, from soft, distensible portion of lung, half-an-inch from grey tubercle. From 4th case, never had any hæmoptoë, except the microscopic kind.

If we trace a large bronchial tube towards the wall of a closed vomica, its blind extremity is first stopped up by a plug of yellow tenacious concrete pus, and then by firm grey gelatinous matter, into which by degrees the tube seems to be transformed.

Nature of Softening.—The softening of a tough tubercle does not necessarily imply absorption of the solid and deposition of fluid in its place. It is a physical molecular change in the original material of tubercle, such as is exemplified in the centric softening of fibrinous clots, and fibrinous exudations, which has so often been mistaken for pus. Such, also, as occurs, according to Mr. Paget, when a large firm collection of inflammatory exudation is suddenly converted into the fluid of an abscess. Spontaneous softening, therefore, is not a special characteristic of tubercle, but belongs to it in common with many other firm morbid formations. Just before the fluid stage, when the tubercle is on the eve of liquefying, but is yet only soft and moist, the tubercle cells are larger, plumper, and more opaque than at any previous period. They also now manifest changes of shape under manipulation, as observed by the microscope, which they cannot be made to do at any other time. This larger size and more uniform outline probably indicate physical imbibition rather than growth; impending disintegration rather than development. It shows that the tubercle cells are about to change their condition; that they can no longer remain stationary; that the power which they have hitherto possessed of maintaining their own feeble nutrition is gone. As liquefaction advances, most of the cells disappear, and in their stead we have oil molecules, granular detritus, and liquid. Besides the true oil molecules, highly refracting particles of a peculiar modification of albumen (Dr. Parkes), and particles of crushed phosphate of lime (Dr. Jenner), cannot by the eye alone be easily distinguished from oil molecules. A little micro-chemistry is requisite. The lime effervesces with, or is dissolved by, an acid; the albumen is scarcely altered by ether; the oil molecules are either greasily dissolved away, or else much brightened, by ether, The lime indicates calcareous degeneration; and the albumen in minute particles also indicates degeneration equally with the oil molecules.

Softening of tubercle, then, essentially consists of two combined modes of disintegration—fatty degeneration and liquefaction. The two do not seem in all cases equally to participate. The more there is in the softening of molecular transformation, the slower and safer and more likely to admit of arrest, is the disease. The more there is of liquefaction, the quicker and more destructive is the progress. The former change is more allied to normal disintegration of tissue. The latter to disease. The former admits of slow ulterior and not unsafe transformations. The latter is connected with more inflammation of surrounding tissue.

Destructive Inflammation.—When a tubercle is fully liquefied, the surrounding lung tissue, if not already inflamed, always becomes so; just as the integuments over an enlarged lymphatic gland inflame when this has fully suppurated. But as the skin in most cases does not wait until suppuration has been completed, before it begins to inflame, so, in like manner, the pulmonary texture seldom waits for the completion of softening in the tubercle ere it inflames. Inflammation, molecular death, and suppuration are the terminal steps in the local destructive course of tubercle; and when united, by their havoc, so riddle the lungs with channels and cavities, as constantly to excite our wonder that life could have been at all compatible with such an amount of disease.

Increase of Tubercle.—Tubercles are found of obviously different age and condition in almost every microscopic examination of chronic phthisis. The deposition of tubercles is therefore progressive. What causes this progressiveness? In the first place, and mainly, the persistence and increase of the cachexy. Then, inflammation around existing tubercles, provided the cachexy is great. We have a right to infer this, if only from the fact that tuberculization, in the shape of tubercular infiltration (which might be called tubercular inflammation), is more extensive, more rapid and destructive, in the later stages of chronic phthisis, when the general cachexy has attained its worst. And lastly, it is possible that the mere presence of tubercle may serve to some extent as an attractive focus for more.

As tubercle corpuscles possess no power of growth after they have once attained their ordinary small size, which they probably do at once, and no power whatever of self-multiplication by cell-reproduction, a tubercle can only increase in size (as already remarked) by the accretion of fresh material from without. Now, does the existence of some tubercle in the lung exert a strong attractive influence for more? Whilst the diathesis remains unimproved, does deposition of tubercle, after it has once commenced, go on fast in proportion to the number of separate deposits already laid down?

That a tubercle is to some extent a centre of aggregation is certain, or it would never increase at all. But it is less clear that this aggregation results from any such attraction of like-for-like as that which operates, for example, in crystallization. A tubercle, however small, obstructs a number of capillary vessels, produces a stasis in the circulation, which, so long as inflammation has not obliterated the vessels, is most considerable close to the tubercle. Exudation is consequently favoured here more than elsewhere, and provided the blood plasma be tuberculous, and in proportion as it is so, will such exudation tend to increase the quantity of tubercle. At the same time, we cannot prove that there is none of the like-for-like attraction (homogeneous affinity) between the elements of a completed tubercle and the blood plasma; but there are reasons for considering that if any such do operate, its influence cannot be strong. If it were strong, the dissemination of small tubercles would be the exception, and not the rule. And, unless we assume the first deposition to have exhausted the tubercle material for a time, existing tubercles would continue to increase in size in a geometrical ratio; tubercles would scarcely be laid down at all in fresh situations; and temporary lulls of the disease would be greatly less common than they are. There is another reason for inferring that tubercle has not any *strong* tendency to invite the formation of more. Such attraction in the living economy generally proceeds from, and depends upon, active vital changes, in which active cells draw from the blood what they require for growth and multiplication. In this way cancer acts as a centre of attraction, for its cells grow, multiply, and for a while remain as cancer matter. But not so in tubercle. Here, cell life is at its minimum; development is small; multiplication null. Material is not wanted for growth, for after the first there is no growth; and material is therefore not attracted. Still tubercles do enlarge; and if a tubercle does in some way promote further deposition around itself, of what moment is the question—whether it does this by means of direct attraction exercised upon blood plasma, or merely by mechanical obstruction at the spot favouring exudation? The question has this importance and interest. In the one case—Tubercle must ever keep up Tuberculosis, for Tubercle makes Tubercle. In the other, it is the tuberculous diathesis alone which occasions increase of tubercle, and the tubercle already deposited has not, from the mere fact of its presence, at all events before the period of softening, any direct influence in keeping up the tuberculous diathesis. In the former case, the mere existence of a tubercle would offer an all but insuperable obstacle to correcting the diathesis. In the latter, could we correct the diathesis, the existing tubercle would not of necessity cause more to be laid down.

These remarks apply only to tubercle when completely formed. Whilst forming, the compound tubercle cells which line the air vesicles probably do attract their plasma from the blood; and, therefore, at this stage of unformed but forming tubercle, the spot of lung affected must certainly be considered as a focus of attraction for tubercle; but this ceases as soon as the given air vesicles are crammed with exudation, and the tubercle has thus become completed.

Progressive increase in the quantity of tubercle is the principal element in the destructive course of chronic phthisis, not only because it proves that the cachexy still continues in active operation, that "the snake is not even scotched," but also because the larger the tubercles become, the more certain are they to soften quickly (and therefore dangerously); for the farther the oldest portions of each tubercle are pushed inwards towards the centre of the deposit, the farther are they removed from the source of nutritious supply, and from the influence of living tissue.

We have so far been engaged with the development and destructive course of Tubercle. We shall have another opportunity of completing our attempt to describe the Natural History of this morbid deposit, by considering the changes which attend its arrest, and which constitute the Conservative course, or so-called Curative changes, of Chronic Pulmonary Tubercle.

(*To be concluded.*)

ART. II.

On the Morbid Appearances in Death by Cold. By FRANCIS OGSTON, M.D., Aberdeen.

THE extent of our practical information relative to the effects of intense cold on the human body is but very limited. Though instances of death from this cause are less infrequent, even in this country, than is generally supposed, very few inspections of such bodies are known to have been undertaken, and those which have been recorded by medical writers are brief and defective. In these circumstances, it may be useful to give a detailed account of a few cases of this sort which have been seen and examined by the writer.

The three first are recent cases, the parties having perished in the vicinity of Aberdeen during the severe snow-storm of last spring.

CASE I.—W. M., a male, aged 17, of weak mind and wandering habits, was known to have passed the night of Saturday, the 3rd of February, 1855, in a turnip shed. On the Monday following, after sunset, he came to a farm-house in the parish of Newhills, about five miles from his usual place of residence, where he had some oaten bread and milk presented to him, of which he partook freely, and left the house, having been refused his request of a night's lodging. About an hour and a half subsequent to this he was found lying on the snow, not far from the farm-house, in an insensible state, and foaming at the mouth. He was scantily clad in ragged clothes, which were wet at the time. Without delay he was carried to an outhouse and laid upon some loose straw, where he calmly expired in about three quarters of an hour. Deceased had never been affected with epilepsy.

The body, inspected thirty-eight hours after death, well formed and well nourished. The head nowise deficient in size or shape. The lips and portions of the cheeks of a florid red. The rest of the surface, including the dependent parts of the trunk and limbs, unusually pale. The countenance presenting a smiling aspect. Joints rigid. Pupils dilated. Scrotum corrugated. Scalp, cranium, membranes, and substance of the brain, all unusually pale and bloodless. A marked degree of the same pallor and bloodlessness of the mouth, throat, and air passages. A little frothy mucus in the trachea. Lungs collapsed, and containing less blood than usual. Right cavities of the heart, and both venæ cavæ, enormously distended with a continuous fibrinous mass, surrounded with a thin layer of watery blood. Blood, in a clotted state, completely filling the left cavities of the heart. Liver congested: the blood in this viscus, as well as that in the heart, approaching more to the colour of arterial than that of venous blood. Stomach, intestines, and urinary bladder unusually pale and bloodless. Farinaceous food and milk-curd in the stomach. Spine healthy.

The only structural changes encountered in the cavities of the body were, dryness and considerable firmness of the brain, flattening of its convolutions; slight old adhesions of the lungs to the walls of the chest; partial emphysema of these organs; and buff-coloured patches on the surfaces of the kidneys. The urine, which was in some quantity in the bladder, was free from albumen.

Case II.—M. M'D. F., a female pauper, aged 70, of intemperate habits, was accustomed to pass the night in a barn at Craibstone, in the parish of Newhills. Between nine and ten on the evening of the 13th of February last (1855), she was seen not far from, and walking in the direction of, the barn. About seven o'clock on the following morning she was found dead on the snow close to the door of the building.

On the 15th, at 1 P.M., the body was inspected, when the following appearances presented themselves:—A blush of bright redness on the front of both knees. The same redness, but less bright, over both cheeks, and the lower border of the right wrist. Lips and finger-nails bluish. Rest of the surface, including the dependent parts of the body, pale. Joints rigid. Pupils moderately dilated. Tip of the tongue in contact with the front teeth. Veins on the outer and figured surfaces of the brain full of blood. Interior of the brain containing more blood than usual. A quantity of frothy mucus in the throat. Right cavities of the heart unnaturally distended, and containing a large fibrinous clot and a quantity of fluid blood. A very large quantity of blood, partly clotted, in the left cavities of the heart. Both venæ cavæ, and the aorta and pulmonary artery, distended with fluid blood; which, like that in the heart, except when viewed in mass, was of a much brighter red than usual. Lungs partially collapsed, and containing only a moderate quantity of blood. Liver gorged with fluid blood, of the same appearance as that in the heart.

Minute injection of the capillaries of the peritoneal coat of the smaller intestines generally, giving them a uniform rose hue.

The structural changes in the body were as under:—A minute patch of fatty degeneration in the coats of the basilar artery. Attenuation, to a moderate extent, of the right ventricle of the heart. Slight thickening of the tricuspid, and of two of the aortic valves. Atheromatous patches in the coats of the ascending aorta. Portions of the liver cirrhosed. Melanotic oval or rounded deposits, under the mucous coat of the stomach. Cortical portions of the kidneys attenuated, and of a buff colour. A little milky urine in the bladder, becoming more opaque by heat.

Case III.—B. A., or F., or R., a female, 83 years of age, of intemperate habits, was seen on the 28th of February last (1855), at Cotton, going northwards. On the morning of the 9th of March her body was found on the snow, at the side of a footpath through a field, from three to four miles to the westward of Cotton. Her clothes were soaked in water, and it was conjectured that her body had been covered with snow, and only exposed to view on its melting by the thaw, which had commenced a few days previously. She was lying in a crouching attitude.

The body examined on the same day (9th):—Joints flaccid; lips, and instep of the right foot, of a bright-red colour; dusky redness of the ears, forehead, and upper part of the face; rest of the surface, including the dependent parts of the body, pale; countenance placid; pupils moderately dilated; soles of the feet blanched and plaited; scalp bloody; a thin layer of clotted blood, an inch and a quarter in greatest breadth, under the integuments of the forehead, to the right of the mesial line; a thin layer of clotted blood on the surface of the anterior lobe of the right hemisphere of the brain, at its right side and back part; two small clots of blood, immediately below the surface of the brain, at the same part; brain, generally, firm; tongue retracted; frothy mucus at the root of the tongue, and in the larynx; lungs pale, and containing but little blood; right cavities of the heart, and the vessels connected with them, distended, and containing blood, partially clotted, and a large fibrinous clot, which also filled the trunk and larger branches of the pulmonary artery; left cavities of the heart, and the bloodvessels connected with them, containing an unusually large quantity of blood, partially clotted, and a tenacious fibrinous clot, which was traced as far as the descending aorta and half way up the common carotid arteries; the blood in the heart and large bloodvessels, except when viewed in mass, appeared almost as bright-hued as arterial blood; a little glairy mucus in the stomach; spleen pale and shrivelled; a moderate quantity of bright-hued blood in the liver; spine healthy.

Besides the above, the following appearances were met with in the cavities of the body:—Cerebral arteries mostly everywhere dilated, thickened, and inelastic, but otherwise natural; heart large, and its left ventricle hypertrophied; aorta, and pulmonary, subclavian, and carotid arteries, in the same condition as the cerebral; mitral and tricuspid valves thickened and traversed by firm cartilaginous bands; portions of both the large lobes of the liver cirrhosed; kidneys mottled with yellow patches, their cortices attenuated, and the urine in the bladder albuminous.

CASE IV.—On January 2nd, 1837, J. G., aged 60, a street porter, had been engaged in delivering goods from an early hour till between six and seven o'clock in the evening. Returning from the Bridge of Don (about two miles from Aberdeen), and feeling wearied, he sat down on the bank of the Aberdeenshire Canal at Nelson-street. The night being very frosty, he could not resist the tendency to sleep. After passing some time in this state, he awoke confused, and forgetting where he was, he advanced in the direction of the lights in the town, and fell into a lock in the canal, up to the neck in the water, then covered with a thick crust of ice. Some men passing, hearing his cries for assistance, drew him out of the lock, and conveyed him to a hovel, without fire, in the vicinity. At this place he had a glass of spirits, and after resting for a little, and relating the above particulars, the men undertook to convey him to his own house. He accordingly set out with them, in his wet clothes, although cold and numbed, and proceeded with difficulty about a hundred yards, when he became insensible. After carrying him about four hundred yards further, and being refused admittance into different houses, he was taken into a shop, when it was found that he was dead. This was at half-past nine, P.M. A few minutes later he was seen by an assistant, who found his lips, nails, and general surface pale, the pupils dilated, and the limbs very cold, with only a little heat remaining at the precordia.

The body was examined forty-one hours after death. Prominences of the elbows bright-red; red patches, not so bright, on the fronts of the thighs, and on the right shin; lips, and remainder of the front of the body, very pale; dependent parts of the head, trunk, and extremities reddish, the colour of the trunk approaching to lividity; scalp bloodless; a moderate quantity of blood in the back part of the longitudinal, and in the lateral sinuses; membranes and surface of the brain bloodless; a considerable number of bloody points in the interior of the brain; epiglottis of a uniform bright-red; slight redness of the back parts of the larynx and trachea; blood, of a purplish hue, filling the cavities of the heart on both sides, the venæ cavæ, the subclavian veins, and the aorta and pulmonary artery throughout their whole course—the blood, with the exception of some clots in the inferior cava, in a fluid state; frothy mucus in the air cells of the lungs; liver, spleen, kidneys, the left lung, and the dependent parts of the right lung, all moderately congested with fluid blood, of the same appearance as that in the heart and large vessels; food in the stomach; bladder full of clear urine.

The brain was of a firm consistence throughout. The lining membrane of the lateral ventricles, at their foreparts, and that of the third ventricle, was thickened, and of almost cartilaginous firmness. There were some old adhesions of the left lung to the chest and diaphragm. With these exceptions no structural changes were discoverable in the cavities of the body, or about the spine.

Remarks.—These cases present so many points in common, and several of these of so peculiar a kind, that little room is left for hesitation in assuming that the cause of death had been the same in all. Without, however, formally enumerating the particulars in which they coincide, it may be sufficient to direct attention to a few of the more prominent of these.

One striking feature in all the cases was the colour of the blood in the heart and elsewhere, so different from that which presents itself in ordinary inspections of the dead body. In one of the cases this was so marked that the appearances exhibited by the thoracic and abdominal cavities, when laid open, suggested the comparison of them to those of a living animal.

Another marked peculiarity, disclosed by all the cases, was the amount of blood accumulated within the cavities of the heart on both sides, and in the large vessels connected with them, arterial as well as venous. In three of the cases large fibrinous clots had separated from the blood both in the heart and bloodvessels.

Corresponding with this accumulation of blood in and around the central organ of the circulation, was its marked absence or deficiency in other parts of the body. Thus the general surface in all was pale, and the usual suggilations wanting in the dependent parts of the head and trunk. In three of the cases the scalp was pale and bloodless. In the first case the same paleness and bloodlessness was observable in the skull and its contents. In the fourth case, though there was blood in moderate quantity in the longitudinal and lateral sinuses, with a considerable number of bloody points in the interior of the brain, the surface of the brain and its membranes were pale and bloodless. In the second case, while the veins on the outer and figured surfaces of the brain were full of blood, the sinuses were empty, and the membranes natural. In the third case the sparing effusions of clotted blood at the corresponding parts of the scalp, and of the surface and interior of the right hemisphere of the brain, pointed to a fall about the time of the woman's death; a conjecture favoured by the circumstances, that she had been seen previously in drink, and that the footpath beside which her body was found was bordered by a deep trench, filled with snow to the same level. In the first case the mouth, throat, and air passages were bloodless. In the three first cases the lungs contained less blood than usual. In Case 1 the stomach, intestines, and bladder are noted as unusually pale and bloodless.

Another peculiarity in the above bodies is the existence in all of diffused patches of bright redness of the surface, at various parts of the front of the face and limbs; and perhaps also the minute injection of the exterior of the smaller intestines in Case 2, and the mucous froth in the air passages in three of them, and in the air cells of the lungs in the remaining instance, may be regarded in the same light.

The paucity of details in the few published cases of death by cold renders it impossible to institute any complete or satisfactory comparison between these and the cases just adduced. From the notices collected by Dr. Copland* we gather that Quelmalz found "the large veins and arteries filled with polypous concretions;" and Cappel, "the blood and fluids accumulated chiefly in the pectoral and abdominal viscera." Dr. Kellie again, in the two cases he has published,† met with the same injected appearance in the intestinal tube which was encountered in Case 2. On the other hand, Quelmalz, Rosen, and Kellie speak of cerebral congestion, to a greater or less extent, in their inspections.

In these circumstances, and till we have a larger collection of cases before us, it would be unsafe to draw any positive conclusions as to the immediate cause of death from cold, whether from syncope, as the foregoing cases would suggest, or from coma, as contended for by Dr. Kellie* and others. With the view of throwing some further light on this obscure point in pathology, I shall subjoin a few instances of the deaths of infants from exposure, in which it appeared to me that cold had played the principal part in leading to their fatal issues. In regard to these I shall only remark, that while they will be seen to exhibit several phenomena in common with those observed in the bodies of the adults as given above, it will be noticed that they show differences in other respects; a result which, assuming that the cause of death was the same in all, may have in part been owing to the less intensity of the cold to which the infants were exposed, and in part to peculiarities of constitution at their early age.

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* Dictionary of Medicine, vol. i. p. 357.

† Transactions of the Medico-Chirurgical Society, Edinburgh, vol. i. p. 84.

CASE V.—J. B., a male infant of six weeks, was exposed, thinly clad, during a cold night in the beginning of June, 1827, to a journey in an open vehicle. At first the child cried a good deal, and refused to suck, then lay quiet, and at the end of the journey was found to be dead.

When inspected next day the following appearances were noted:—Front of the chest, sides of the belly, and back part of the pelvis, of a deep-red colour. Scalp natural. Dura mater minutely injected. Veins on the surface of the brain and in the choroid plexuses turgid. A number of bloody points in the interior of the brain. Tip of the tongue protruded beyond the gums in front. Reddish frothy mucus in the trachea and bronchi. Minute injection of the inner face of the sternum, the surface of the right lung, the exterior of the aorta, and of the pulmonary artery at their origin, the omentum, the outer coat of the stomach and of the whole of the peritoneal surface of the intestinal tube throughout its course, but brightest over the smaller intestine. The blood in the injected vessels of a bright hue. Cavities of the heart gorged with blood—colour not noted.

CASE VI.—A male infant, seven hours after birth, was laid down alive in a lane in Aberdeen, about 8 o'clock, P.M., on August the 24th, 1833. It was discovered shortly after apparently dead, its limbs cold, and only a little heat remaining at the precordia. It was merely wrapped in a piece of dark cotton cloth. Thinking the child was in a fit, the person who found it had it immediately placed in a warm bath, when a medical gentleman, called in, pronounced it to have been some time dead.

Inspection, forty hours after its discovery.—Trunk and limbs rose-hued, approaching to scarlet on the extremities, and to lividity on the back; lips, points of the fingers, soles of the feet, and nails, all livid; joints rigid; face anxious; pupils dilated; tip of the tongue fixed between the gums in front; scalp bloodless; pia mater injected; brain natural; serum in the upper part of the spinal canal; epiglottis of a uniform bright-red; diffuse dull-redness of the interior of the trachea and bronchi; lungs of a scarlet hue; dark fluid blood filling the right cavities of the heart, the left auricle, the coronary veins, the venæ cavæ, the subclavian and jugular veins, and the aorta throughout its whole course; frothy fluid in the air cells of the lungs; blood, in some quantity, in the lungs, partly of a florid hue, but chiefly of the usual venous appearance; liver and kidneys congested with dark fluid blood; bladder empty.

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* Dr. Kellie's inspections are unfortunately incomplete. Nothing is said, in his narrative, of the state of the heart; and it is at least doubtful if the chest was opened at all, in either of his cases.

Case VII.—A female infant, five days old, had been exposed alive on the banks of the Dee, near Aberdeen, on January 18th, 1841. It was found dead on the following morning.

Inspected on the morning of the 20th. Face reddish; surface of the neck, hips, and vulva also reddish; lips and nails livid; rest of the surface bloodless; joints rigid; pupils natural; tongue protruded beyond the gums in front, and its tip livid; glairy mucus at the lower part of the trachea; pericardium, thymus, surfaces of the heart, and of the aorta and pulmonary artery, minutely injected; cardiac veins turgid; cavities of the heart on both sides, the venæ cavæ, and the aorta and pulmonary artery, distended with dark blood, mostly in a clotted state; lungs collapsed, bright red externally, and very much loaded with dark fluid blood; liver of a deep purple hue; liver, spleen, pancreas, and kidneys much congested with dark fluid blood; surface of the pancreas injected; intestines minutely injected; mesenteric and gastric veins turgid with blood; bladder empty; moderate injection of the sheath of the spinal cord; scalp vascular; pia mater minutely injected; veins on the surface of the brain turgid; interior of the brain mottled with red patches.

Case VIII.—A female infant was discovered after some days' exposure in a deserted quarry-hole at Bourtie, in February, 1849, where it had been laid down alive.

The following particulars are borrowed from the notes of the inspection by Dr. James Jamieson, now of Edinburgh.* Except slight redness of the dependent parts of the trunk and extremities, the rest of the surface, including the lips, pale and bloodless; portions of the surfaces of both lungs vermilion-hued, their interior much congested with fluid blood; blood of the same character in both sides of the heart, but in greatest quantity in its right cavities; scalp, cerebral membranes and sinuses, and the surface and interior of the brain, all remarkably pale and bloodless, as were the muscles of the face, neck, and arms.

Art. III.

Critical Examination of the Evidence for and against the presence of Epithelium in the Air Cells of the Human Lung. By George Rainey, M.R.C.S., Lecturer and Demonstrator of Practical and Microscopical Anatomy at St. Thomas's Hospital.

Scarcely was the existence of a layer of nucleated cells (called "epithelium") on the free surface of all the internal membranes, whether situated within the cavities of the body, where they exist in the form of shut sacks, or found lining the various passages and becoming continuous with the dermis, discovered, and its presence in such situations considered as constant and universal, before the opinions of anatomists became divided as to the correctness of this inference, its existence in particular parts of certain organs being by many observers altogether denied. Thus, whilst no doubt whatever was entertained of the general presence of the newly-discovered structure in the tubuli uriniferi of the kidney, some observers denied its existence on the Malpighian tufts. Also on the membrane lining the articular cavities, where it encloses capillary plexuses called synovial glands or fringes, the presence of a distinct epithelium was allowed by all; whilst on the non-vascular parts of the same membrane, especially those which are by some supposed to cover the articular cartilages, no such structure was by many considered to exist. In certain parts of the lungs also, as the lining membrane of the bronchial tubes, the epithelium, being particularly apparent, was universally admitted without any hesitation, yet its existence in the air cells was, and still continues to be, a point much disputed by anatomists and physiologists. This marked contrariety of opinion upon points which at first sight appear so easy of demonstration as the mere existence in certain parts of structures so characteristic as epithelia are generally described to be, can scarcely fail to strike with astonishment those who do not employ the microscope, and, in their opinion, to detract greatly from its utility as an instrument of research. But it is apparent also from these statements that there is a bright as well as a dark side to this subject; for they show that although anatomists differ widely in their opinions about some points, they perfectly agree upon others. Microscopical observers do not express any doubt as to the fact of the existence of epithelium in the uriniferous tubes, on the synovial fringes, or in the bronchial tubes; nor do even their descriptions of these epithelia, though they are possessed of different characters in these several parts, materially differ. And this positive knowledge could never have been attained had not these facts been revealed by the microscope, so that upon some of the most important points of minute anatomy there is obviously sufficient uniformity in the opinions of those who have employed this instrument in their investigations to convince every person whose mind is not fettered by prejudice, that its aid ought not to be refused or all its indications to be discredited because there are points upon which anatomists are not yet unanimous. On the contrary, these diversities of opinion upon the same subjects only show that there is something more or less directly connected with these disputed points, which has not been perfectly apprehended, and that they require more minute and extensive examination than they have yet received in order that they may in their turn be raised from the misty regions of uncertainty, where they are liable to an indefinite number of interpretations, according to the individual bias of those who contemplate them, to that state of absolute certainty when they will appear in too clear a light to allow of being any longer made subjects of discussion. Thus considered, it will be seen that these discrepancies, in the place of being put down to the score of the inapplicability of the instrument, are chargeable on those who use it, or who do not use it with sufficient care.

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* This experienced medical jurist was engaged with me in the investigation of the three adults examined in February and March (Cases 1, 2, 3).

Although I have alluded in these general observations to three instances in which the opinions of anatomists are at variance concerning the existence or non-existence of epithelium, I shall confine my observations entirely to the part of the dispute which has reference to the air-cells.

Now, whilst on the one hand the mere circumstance of a pavement epithelium having been described by several anatomists in the air-cells of the human lung furnishes an amount of evidence of its existence which, if unanimously confirmed by all future observers, would be regarded as conclusive; so, on the other hand, the fact of its existence being denied by anatomists who entertain no doubt whatever of the presence of epithelium in the smallest bronchial tubes, may fairly be adduced as negative evidence in favour of its non-existence; and as the strength of this evidence rests upon a variety of incidental circumstances, such as the want of agreement in the accounts of those who have described the epithelium in question, the absence of those characters, both positive and negative, which distinguish the same class of structures in other parts of the body, the general accuracy of the descriptions, &c. &c., all these points have a right to be fairly and fully considered. But the strongest argument against the presence of epithelium in the air-cells, the one upon which I chiefly dwell, is based upon the fact that the cause of the epithelium-like appearance which they present under the microscope admits of being accounted for independently of any supposition implying the existence of a lining of epithelial cells. Those which I consider as the best and only satisfactory proofs of the presence of an epithelium, are derived from the facts of its admitting of demonstration on the same part both when *in situ* and when detached. In the latter case, the true character of its cells can be best determined, and in the former, its situation and relations; but it is only the combined evidence of both which will show whether the appearances which any part presents, are due to an independent structure, or whether they are an integral part of the tissue upon which the epithelium is supposed to be placed.

From Kölliker's account of the epithelium lining the air-cells of the human lungs, it would appear that there is great difficulty in demonstrating it *in situ*, in consequence of the remarkable readiness with which it is said to be detached from the cell-wall; and as this difficulty was not found to exist in animals, in which the lungs were examined a shorter period after death, it is, I presume, attributable to the extraordinarily rapid post-mortem changes of which this epithelium is thought to be so very susceptible in the human body. Dr. Andrew Clark informs me "that the lung ought to be removed from the body as soon after death as possible within five hours," in order that it may be in the most fit state for demonstrating the epithelium of the air-cells. Now, according to these statements, the investigation of this epithelium in the human subject seems to be so hedged in with difficulties, that the opportunities of examining it must be very rare, for it is not often that post-mortem examinations are allowed so short a period after death; consequently there is the greater probability that those who have described this structure in the human air-cells have fallen into that class of errors which arise from mistaking that which is merely accidental for that which is constant; and in this instance it seems much more probable that such a mistake should have been made than that there should exist in the human lungs a structure endowed with such an anomalous property. I am perfectly aware that very rapid post-mortem changes do take place in some of the more concealed and delicate epithelia—such is particularly the case with that lining the capsule of the lens—but that this should be so pre-eminently the case in one which is always exposed to the contact of the atmosphere, is most improbable, especially as it is not a property of the epithelium lining the smallest bronchial tubes, which in all respects would be similarly circumstanced. Besides, the pavement epithelia in other parts of the body are not of this evanescent character; they can all be demonstrated in the ordinary post-mortem subjects, both with ease and certainty. The cells of this epithelium are said by this author to repose immediately upon the fibrous layer of the membrane forming the air-cells; which is another anomaly, as the cells of most other epithelia are situated upon a thin membranous film, devoid of fibres, well known by the name of "basement membrane."

There is another anomalous character belonging to this epithelium, especially insisted upon by those who have described it, which is the distinctness said to be acquired by its cells when they are in a state of disease. It is said* that the epithelium of the air-cells is normally shed, though not readily detected in health, yet that it is easily discovered in disease. This, I may observe, is the reverse of what is observable in this class of epithelia in other parts of the body, in which, on the contrary, their specific characters become obscured and masked by disease. So that in those parts on which pavement epithelium is well known to exist, it is always sought for when they are in the healthiest state possible; and the more free they are from disease, the more distinct and better marked are the characters of their epithelia. Besides, the mere fact that a structure, whose presence in its normal condition is so remarkably uncertain as to be demonstrable only by a kind of accident, should only acquire that degree of distinction which is necessary to render it recognisable when the membrane upon which it is placed is diseased, is certain evidence that at least some part of the change which it has undergone to give it its distinctness is the effect of abnormal causes, and it is a strong argument in favour of the entire appearance being due to the same agency. Such evidence as this is more in favour of its being an abnormal product than a healthy structure. This supposition agrees with appearances which I have myself observed in diseased lungs. In some of the apparently healthy air-cells of the human lungs, situated near to those in which tuberculous deposit existed, I have sometimes found a glassy-looking substance, mixed with large globules of oil, which might be mistaken for epithelium. This, when I first heard of Dr. Thomas Williams's hyaline epithelium in the air-cells, I thought might possibly have been something similar to that which he gives this name. This however, when closely examined, I found to be devoid of the characters of epithelium, and also, on referring to Dr. Williams's paper, that it does not agree at all with the newly-named structure which he designates Hyaline Epithelium. The want of agreement in the statements of those who have described the epithelium of the air-cells, is so striking, that it is impossible that all who have intended to write about this epithelium can have had precisely the same structure in view, and have applied the same term to the same appearance. Kölliker, notwithstanding the great difficulty he complained of in meeting with epithelium in the healthy human lung still adherent to the pulmonary membrane, makes no mention of want of distinctness or completeness of its individual cells; but, on the contrary, in these respects the cells must have been remarkably perfect to have admitted of the measurement of their breadth and thickness, and to have even allowed of their exact distance from the capillaries over which they are situated being estimated, which is stated to be about half of the thickness of the membrane. Dr. C. Radclyffe Hall, on the contrary, makes no allusion to the difficulty or uncertainty of finding this epithelium in the human lung, but describes its cells as wanting that distinctness of outline and regularity of form which characterize other pavement epithelia. The epithelium of the air-cells in man, according to Dr. Hall, has less sharply-defined outlines than most other varieties of pavement epithelium; but the flat cells are bounded by a dim line of limitation. In appearance they are thin, almost transparent, and have a slightly nebulous, somewhat ill-defined nucleus, very different from the bright, sharply-cut nucleus of the pavement epithelium of the mouth, for instance. In size and shape these cells vary greatly. They are pentagonal, hexagonal, or polygonal, with angles more or less acute or rounded, according to their mutual fitting into each other. Their general character is, that they constitute a fine but dimly-defined pavement epithelium of a single layer. Now, if the attention of these two observers had been directed to the same structure, it is difficult to understand how the deficiency of distinctness, and want of definition of the alleged epithelial cells, and more especially their extreme irregularity in form and size, mentioned by the one, should have escaped the notice of the other, particularly as the attention of the latter, in taking the actual dimensions of these cells, could not have failed to have been arrested by the negative peculiarities described by the former. As such an oversight could scarcely have been possible, there is every reason to conclude that the epithelium mentioned by Kölliker is not the same as that described by Dr. C. Radclyffe Hall.

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* See Cyclopædia of Anatomy and Physiology, part xlv. p. 271.

Dr. Thomas Williams, author of the article, Organs of Respiration, in the 'Cyclopædia of Anatomy and Physiology' for March, 1855, observes, "That the nuclei and granules of the epithelium of the air-cells are less declared than those of any other description of epithelium;" and therefore, in consideration of these negative characters, he proposes to distinguish it by the term "hyaline epithelium." Dr. T. Williams does not describe his hyaline epithelium minutely, but adopts the views of Van der Kolk, from whose work he introduces a plate, showing the hyaline pavement epithelium which lines the interior of the air-cells. The representation of this imaginary epithelium agrees very well with the description given of it by Dr. Hall, especially in respect to the extreme irregularity in the form and size of the epithelial-cells; but their outline and nuclei are represented with more distinctness than Dr. Hall's description would justify. However, I have no doubt but that the description of the one and the delineation of the other apply to the same structure.

Sometimes there is a greater degree of exactitude and precision in the description of microscopical structures than their extreme minuteness or intrinsic deficiency in positive anatomical characters, with our present means of examination, render attainable. This practice, though inspiring great confidence with some, is always a source of suspicion with those who have had much experience in the use of the microscope, and who, consequently, know the difficulties attending the examination of minute parts with this instrument.

Both Dr. Thomas Williams and Dr. R. Hall describe the epithelium of the air cells as forming only a single layer. It is observed by the former, that "they (the epithelial cells) are adjusted accurately, as a single layer, edge to edge." As this statement implies the utmost degree of distinctness in these cells, and conveys an idea that they admit of being easily examined, and their relations to one another and to the structures with which they are connected accurately determined, it is completely at variance with the accounts which they themselves have given. For if these cells really are as deficient in outline and as irregular in form and size as described by one of these authors—or if they possess a degree of deliquescency which renders an examination of them whilst *in situ* in the perfectly healthy human lung a very uncommon occurrence, as stated by Kölliker—and lastly, if to these characters be added a degree of tenuity and transparency likening them to glass, and thus requiring them to be distinguished from other pavement epithelia by the term hyaline, or vitreous, as has appeared necessary by Dr. T. Williams, I do not see how such a degree of precision and exactitude, under circumstances so unfavourable, could possibly have been ensured. Besides, if it be a fact, as intimated by Dr. R. Hall, that different growths of these cells exist together, the statement that there is only one layer cannot be true; for if we suppose that on each side of the very thin membrane between adjacent air-cells it is only the middle-aged corpuscles which, by their "mutual fitting into each other," form the single epithelial layer in question (and this supposition is by no means inconsistent with the alleged facts, as it is only cells of a similar form and size which would admit of the accurate adjustment of their edges mentioned by Dr. Williams), there is no room for such cells as are advancing to a state of maturity, or for those which, having passed the middle period, are declining into a state of decay, and therefore there must be more than one layer of epithelial cells of some sort or other. This involves a contradiction which I must leave for others to reconcile. The statements of Kölliker respecting the distance of the alleged epithelium of the air-cells from the capillary plexuses in their walls, implies a degree of regularity and uniformity in the structure and position of these parts which have no existence in nature, and therefore leave the accuracy of his measurements, his plates, and his description of the epithelial cells, open to suspicion. Such is the negative evidence, or rather the want of consistent positive proof, of the existence of a pavement epithelium in the air-cells of the human lung.

I will now consider the cause of the epithelium-like appearance presented by the air-cells which has led to the belief that they are lined with epithelium. But first, in justice to myself, I must correct a misrepresentation of my views upon this question occurring in Dr. Radclyffe Hall's paper,* entitled, The Mode of Development of Tubercle in the Lungs in Chronic Phthisis, its connexion with fatty degeneration of the epithelium of the air-cells, &c. &c., in which he says, "That the existence of an epithelium at all in the air-vesicles has been questioned by physiologists of high repute, but is now very generally admitted. BLOOD CORPUSCLES, seen through the walls of the capillaries, were indicated by Mr. Rainey as having possibly been mistaken for epithelial cells." I wish to observe, that no such statement ever proceeded from me, or that I ever entertained so improbable an opinion. Dr. Hall can scarcely have seen what I had written upon this point, or he would not have so completely misunderstood my meaning.

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* British and Foreign Medico-Chirurgical Review, No. xxx. p. 477.

But before proceeding to show to what the epithelium-like appearance, which has led to the belief in the existence of an epithelium in the air-cells, is due, I must again advert to the want of agreement which is observable in the descriptions of those who have written upon this structure. The microscopic appearances which are described by Kölliker are so dissimilar to those by Van der Kolk, that they cannot apply in any respect to the same corpuscles. Van der Kolk found the employment of acetic acid necessary to give the nuclei of the corpuscles which he has described their utmost distinctness. Kölliker, on the contrary, does not state that any chemical substance was employed for that purpose; and if these corpuscles, as seen by this observer, were sufficiently distinct to admit of the precisest measurement, and if, at the same time, they were so loosely connected with the fibrous membrane with which they are in contact, it may be fairly inferred that he did not use acetic acid, as under such circumstances it would have been of more injury than benefit. But there is a still greater want of agreement in the delineations of this epithelium by these two authors, although they are both professedly intended to represent the same structure. I may observe, that Van der Kolk's plate, exhibiting the hyaline pavement epithelium lining the interior of the air-cells, is a good but slightly exaggerated representation of an appearance which I have myself frequently seen; but that Kölliker's figure, showing a thin section of a few air-cells from the human lung, with the epithelium projecting from the pulmonary membrane, has no resemblance to Van der Kolk's, nor is it in the least like any microscopic appearance in the human lung which has ever occurred to me. I cannot help thinking that the corpuscles described so precisely by Kölliker were some of the imperfectly-developed epithelial cells from the smallest bronchial tubes, which had been detached in the process of manipulating, and had got by accident into the air-cells. This is so common an occurrence, that such corpuscles are generally found in greater or less quantities in these cells, but they have not the most distant resemblance to pavement epithelium, as seen in other parts of the body, nor to the imaginary hyaline pavement epithelium represented by Van der Kolk, as copied by Dr. Thomas Williams in the 'Cyclopædia of Anatomy and Physiology.' As to the appearances represented in Van der Kolk's plate, and the greater part of those described by Dr. R. Hall, which in most respects agree with the latter, I am satisfied that they are produced almost entirely by the capillary network in the walls of the air-cells. This appearance I noticed several years ago, and at that time convinced myself that it was not due to the presence of epithelium, as can be seen by my papers On the Lungs, published in the 'Medico-Chirurgical Transactions' of 1845 and 1848; but as this still continues to be a point which is much disputed, I have lately repeated the examination of these parts, and the result has served only to confirm my former convictions of the accuracy of the opinion which I then expressed.

Since the period alluded to, the question has been brought, by the published results, either in the form of descriptions or plates, of several anatomists who have paid great attention to this subject, into a much narrower compass; its decision will consequently be more easy. Dr. C. Radclyffe Hall's description of an epithelium in the air-cells of the human lung, Van der Kolk's plates, and Dr. Thomas Williams's observations, and some preparations which Dr. Andrew Clark has lately had the kindness to lend me, showing what he considers to be an epithelium in the air-cells of the human lung, and what I had myself observed both before and after I had become acquainted with the labours of these observers, has convinced me that we are perfectly agreed as to the actual existence of the same microscopic appearances in the air-cells of the human lung, and therefore that the only point to be decided is whether this appearance is due to the presence of a pavement epithelium in these parts, or to the cause I before mentioned. The method which I have employed to determine this point has been to compare, side by side, perfectly fresh specimens of lung with those in which the vessels had been filled with colouring matter; by this mode of procedure, there is no difficulty in recognising many of the capillaries in the uninjected, or imperfectly injected, parts, and in distinguishing their course, to a greater or less extent, along the membrane which connects them together. This is especially easy in those capillaries whose arched inosculations can be seen projecting beyond the circular free border of the pulmonary membrane, where it forms the openings of communication between the air-cells, in which capillaries the "slightly nebulous ill-defined nuclei," as described by Dr. R. Hall can be seen.

That these nuclei belong to the walls of the projecting capillaries, and that they are identical with those irregularly dispersed through the membrane, where the capillaries are so blended with its tissue as not to present a distinct outline, as figured by Van der Kolk, is so evident as to admit of no doubt whatever, at least in my mind. That these nuclei are confined to the bloodvessels is further proved by the fact that none of them are visible in injected specimens in the areolæ of the capillary plexuses, where nothing but the pulmonary membrane is present, whilst in the same specimens the epithelial cells are perfectly distinct in the smallest bronchial tubes. The capillary plexuses in the external wall of the peripheral air-cells, having the largest meshes, are best adapted for this examination. However, as this fact might be attributed to the improbable and perfectly gratuitous supposition of a much greater liability to rapid post-mortem changes in the epithelium of the air-cells than in that of the ultimate bronchial tubes, I have particularly examined this point in frogs which have been scarcely dead, in which the same facts and appearances are obvious. In these reptiles, the ciliated epithelium does not extend over the capillaries which are directly concerned in the process of respiration, but is confined to the folds situated between the lateral sacculi or pouches, where the cilia can be seen beautifully in action; whilst in the pouches, which correspond to the air-cells of the mammal, no epithelium of any kind can be seen, but only the persistent nuclei of the capillaries of the air-sacculi. These nuclei are present also in the wall of the capillaries upon which the ciliated epithelium is situated, and are visible when it is removed. The latter capillaries are of a different size, and have a very different arrangement from those in the sacculi: this is especially remarkable in the female frog.

The next thing which may be mentioned as contributing to the epithelium-like aspect presented by the air-cells, is the obscure retiform appearance produced by the irregularly oval spaces, corresponding to the meshes of the capillary plexuses, and bounded in the recent lung by the vessels containing more or less of the fluid part of the blood. Considering the remarkable regularity in the shape and size of the epithelial cells or scales composing a layer of true pavement epithelium, and the distinct outline which they ordinarily present, it would appear impossible that such appearances could ever so far assimilate to pavement epithelium as to admit of being mistaken for it. I consider that such a mistake could not be made if the real characters of this epithelium were kept in view; but it must be observed that Dr. R. Hall's description of the appearances which he takes for an epithelium in the air-cells, is so distinguished by its negative characters, which he is under no temptation to exaggerate, that they would serve much better to prove the converse of his position, than to establish the position itself. The most unepithelial-like part of the appearance referred to in his description, is that which alludes to the shape and size of the epithelial cells, from which they may much more easily be imagined to resemble the capillary network, with its irregularly oval meshes, as they are obscurely seen in the walls of the air-cells, than any well-marked pavement epithelium which has ever been described. In this respect Van der Kolk, judging from his plate of the epithelium lining the air-cells, seems to me to have fallen into the same error.

The sharp threads of elastic tissue are the last structure to be noticed as having a part in producing the confused epithelium-like appearance in the air-cells. They are especially distinct and defined in the lungs of quadrupeds. In these animals also, especially in the younger ones, as also in children, the persistent nuclei in the capillaries of the air-cells are particularly distinct, much more so than in the adult human subject; hence probably the reason why Kölliker had so little difficulty in finding what he considered to be epithelium in the lungs of these animals; and Dr. Andrew Clark, in speaking of the lungs of children, observes "that he never fails to find in the child's lung a lining epithelium to the air-cells." I wish particularly to observe that it is not to any one of these structures, taken separately, that I attribute the mistake in question, but to the appearance produced by the whole taken together; although I believe that the persistent nuclei in the coat of the capillaries have the largest share in causing it, and next to that, but in conjunction with it, the imperfectly-developed epithelial cells which had been detached from the terminal bronchial tubes, and got into the air-cells during the manipulation. The oval meshes of the capillary network will also contribute their part, and serve, as before stated, to give to these imaginary epithelial cells their characteristic irregularity of form and size, and to furnish the "dim line of limitation" by which they are surrounded.

Having now shown that there is quite sufficient in the healthy textures of the human lung, when seen under favourable circumstances, to account for that amount of resemblance to epithelium which has led to the belief of its existence in the air-cells, I shall conclude the discussion of this subject by adducing the evidence furnished by comparative anatomy against this opinion. This I do not consider to be necessary, from a conviction that the facts and arguments which have been advanced have not been sufficient to establish this point, but the deductions from this source being, in my opinion, so strong and legitimate, and even of themselves sufficient to decide this question against the unsatisfactory evidence on the opposite side, could not with propriety be omitted in this communication. In birds, the air, after having passed through the bronchial tubes, which are lined by ciliated epithelium, as in mammals, is not received, as in man, into comparatively large irregularly-shapen cavities formed by a distinct fibrous membrane, but it enters minute irregular passages, which soon lose themselves in the meshes of an extremely dense plexus of capillaries, into which this air freely passes, becoming, as it were, extravasated between and around the vessels. These meshes, the analogues of the air-cells of mammalians, and the air-sacculi of reptiles, are so exquisitely minute, especially in those birds whose habits require the highest state of development of their pulmonary organs, as not to equal in size the smallest epithelial cell; and, therefore, they cannot contain a lining of epithelium, or even a basement membrane, without its encroaching so much upon their capacity as totally to prevent the admission of air, and thus to render them altogether incompetent to the performance of their function, and to cause their structure to resemble liver more than lung. And hence the lungs in this class of animals furnishes an incontrovertible proof that an epithelium is not required in those parts of a respiratory organ in which the process of respiration is carried on, and therefore that it is not an indispensable part of the air-cells. But this kind of negative evidence is not confined to the lungs of birds; there are many mammalians in which the respiratory powers are much more energetic than in man, whose air-cells are too minute to admit of a lining of pavement, or any other kind of epithelium recognised by anatomists, without unfitting them for the performance of their function. To complete this evidence, and to show how simple the means are, and how few the conditions necessary for a perfect organ of respiration, I have only to notice the breathing apparatus in insects. In this class of living beings many of the tracheæ are so remarkably minute as scarcely to be distinguishable by the highest and best magnifying powers. In these instances there can be no room for pavement epithelium, either on the surface of these tubes in contact with the air, or on that which is said to be bathed in their blood. It will be seen, then, from all that has been advanced, that as epithelium is not an essential constituent of the air-cells or interstices of birds and some mammalians, it is no abuse of analogical evidence to infer that it does not exist in the air-cells of man, especially as the proofs to the contrary have been shown to rest upon appearances in the air-cells perfectly unconnected with the presence of epithelium, and which admit of easy and rational explanation.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON FORENSIC MEDICINE & TOXICOLOGY.

By BENJAMIN W. RICHARDSON, M.D.

Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Forensic Medicine at the Grosvenor-place Medical School.

I. TOXICOLOGY.

Fatal Poisoning by Nitrate of Potassa.—Dr. JOHN SNOWDEN relates the case of a German, who spoke English perfectly, who went into a store and asked for "bitter salt," meaning sulphate of magnesia. The attendant supposed he meant saltpetre, and gave him half a pound. The man took three ounces and a half at one dose. His bowels were opened three times within three or four hours. He complained of a slight sense of heat in the epigastrium, and drank a good deal of water. About five hours after having taken the saltpetre, he suddenly fell out of his chair and died. There was no post-mortem examination. The *rigor mortis* was very imperfect; the lips were of almost a natural pink hue, and the appearance of the countenance was so life-like, that some persons who were present doubted the propriety of interment on the third day.—*New Jersey Medical Reporter, and Philadelphia Medical Examiner*, April, 1855.

Nitrate of potash not unfrequently poisons cattle when given to them as medicine by injudicious persons. Mr. Truckle has reported, in the 'Veterinarian' for March, that in two cows out of three to which a fluid of a pound of saltpetre was given, two died within three hours, and apparently without a struggle. The post-mortem examination showed that the third and fourth stomachs were inflamed, especially the latter. The editor of the 'Veterinarian' remarks on this subject, that he was consulted some years since respecting the cause of the deaths of several cows, and on inquiry he found that their owner had been giving them a half-pound dose of Armenian saltpetre (nitrate of soda), which he had bought as a cheap purgative. The same writer remembers to have seen three heifers poisoned by taking three quarters of a pound of nitre each. They all died within twelve hours. The agent had been sold by a grocer in mistake for Epsom salts.

Post-Mortem Appearances in Poisoning with Phosphorus.—Dr. REISIG relates the post-mortem appearances observed in a man and his wife, poisoned with lucifer matches. Both patients died in four days. The similarity in the appearances observed is remarkable.

THERESIA SCHOBESBERGER.	JACOB SCHOBESBERGER.
External Appearances.—Slight loss of hair, as in poisoning with arsenic. Mucous membrane of mouth very pale.	*External Appearances.*—Slight loss of hair. Oral cavity pale. Face and breast jaundiced.
Internal Appearances. (*a*) *Abdomen.*	*Internal Appearances.* (*a*) *Abdomen.*
1. The external surface of the stomach was red; the vessels at the small cur-	1. The same appearances were observed, except the dark spot.

vature of the stomach were distended, and filled with dark-brown blood. At the posterior surface was a roundish dark spot, five inches long by two wide.	
2. On opening the stomach there was a strong smell of phosphorus. The whole mucous membrane of the stomach was of a brownish-black colour, and covered with a thick layer of mucus. At the cardia, base, and lesser curvature were many streaky dark-red spots. The mucous membrane was remarkably soft and loosened; the muscular coat was red.	2. On opening the stomach, there was a very faint smell of phosphorus. The mucous membrane of the stomach was slightly reddened, covered in several places with dark-brown spots, and so soft that it could be easily removed by the nail from the muscular coat. The mucous membrane was nowhere corroded.
3. The mucous membrane of the upper third of the small intestine presented the same appearances as that of the stomach; it was covered with a thick brownish mucus. There was no smell of phosphorus.	3. The same appearances were noticed. The peritoneum and mesentery were everywhere covered with numerous extravasations, as were also the abdominal muscles.
4. The liver was of a remarkable yellowish-white colour on the surface and on section: it was void of blood. The gall-bladder was filled with a large quantity of dark-green bile. The urinary bladder contained dark-brown urine.	4. The liver was of a light colour, and void of blood. The gall-bladder and urinary bladder presented the same appearances as in the other case.
(*b*) *Thorax.*—The lungs were very dark in colour, and contained much blood. The heart was soft, pale, and empty. Each pleura contained five ounces of red fluid.	(*b*) *Thorax.*—The lungs were very dark-red, congested, and soft. The pleura and pectoral muscles presented numerous dark-red spots of extravasation. Each pleura contained four ounces of sanguineous fluid.

Wochenschrift der Gesellschaft der Aerzte zu Wien, No. 20, 1855.

Albumen and Hydrated Magnesia as Antidotes in Poisoning with Corrosive Sublimate.—L. Schrœder has performed a number of experiments on dogs and rabbits, with regard to the use of albumen and hydrated magnesia as antidotes in poisoning with corrosive sublimate, from which he arrives at the following results:

1. Albumen is not to be depended on as an antidote. The albuminate which is formed is soluble, not only in an excess of albumen, but also in such albuminous substances as may be present in the stomach and intestines, and is especially liable to be taken up by the acids with which it may there meet.

2. Albumen can only be useful when given so as to produce vomiting, or where vomiting is excited by tickling the throat.

3. Hydrated magnesia cannot be regarded as an antidote, as it forms from the corrosive sublimate an oxide of mercury, which is itself poisonous.—*Deutsche Klinik*, 1854, No. 8; and *Prager Vierteljahrschrift*, 1855, Erster Band.

Case of Poisoning by German Sausages.—Mr. W. H. Michael, of Swansea, relates the following case:—On March 22nd, 1855, Mr. Michael was desired to see a child living in Postern-lane, Swansea. Upon arriving at the house, he found a fine little boy, between four and five years of age, lying on his grandmother's lap. The mother had been given the evening previously a German sausage, of which the eldest son had partaken at once. This had made him ill through the night; vomiting and purging had taken place to a considerable extent. The little boy now ill had eaten some of the sausage (according to the statement of the mother, only one or two very thin slices) for breakfast, about two or three hours before Mr.

Michael arrived, at two o'clock p.m. Shortly afterwards he had vomited. About half an hour before he was seen, convulsions had come on; he had also been violently purged. When seen, the general surface was cold; the limbs rigid; the teeth very firmly clenched; the pupils largely dilated, and insensible to stimulus; and he had occasional convulsive spasm of the lips. The lips were livid; the face was deadly pale; no pulse could be felt at the wrist; and the respirations were only three in the minute. He died in about ten or fifteen minutes, and about three hours after eating the sausage, as nearly as could be learnt from the confused statements of the mother.

The remaining portion of the sausage, which was one of the German smoked and dried kind, showed some incipient softening and decomposition (not putrefactive) at the surface; the interior both looked and smelt good. Careful analysis detected no traces of metallic poison. The mouldiness frequently spoken of by authors could not be seen.

The post-mortem examination showed the stomach half full of pieces of sausage, floating in a pulpy mass, half digested, of the same. Considerable irritation and mammillation of the mucous coat existed, especially towards the pyloric orifice. The mucous coat of the small intestines was irritated throughout, small puncta of blood being observable over the surface, which was bathed in increased mucous secretion. The brain was congested, as were also the thoracic organs. The other portions of the body (which, although well formed, was much attenuated) were healthy.

In Germany such cases are fearfully prevalent. In Wurtemburg alone, according to official returns, more than four hundred cases have occurred in the past fifty years, of which a hundred and fifty died. Of these, forty per cent. occurred in the month of April; and this has been put down as a matter of some importance in determining the character of the poison, which is said usually to manifest its symptoms in from twenty-four to forty-eight hours after ingestion. Recent researches appear to have proved, contrary to what has long been supposed, that unprepared meats, far advanced in the putrefactive process, or belonging to diseased animals, may be often eaten with impunity. Mr. Michael inclines to the doctrine lately put forth by M. Van der Court, that the poison of sausages may be due to the development of an elementary vegetable—the *sarcina botulina.*—*Association Medical Journal,* August 17, 1855.

Poisoning by Cantharides.—On the 27th of October, at two A.M., Dr. C. H. HILDRETH, of Gloucester, Massachusetts, was called to a patient who, early on the preceding evening, purchased from an apothecary about half an ounce of a powder supposed to be the pulvis aloes cum canellâ, commonly called *pikery*.

The medicine was delivered by a boy in attendance. The patient put the powder into a bottle, added to it a tablespoonful of gin, and took two spoonfuls of the mixture—the usual dose for the irritation caused by ascarides. He slept well until twelve o'clock, when he woke with severe pain in the lower part of the abdomen, extending into the lumbar region, but most intense just above the pubes. This rapidly increased, and in two hours became almost unendurable. There was some nausea, but no pain anywhere except as above mentioned. On examination, the mixture, the supposed pikery, proved to be powdered cantharides. Free vomiting was at once produced by sulphate of zinc and free draughts of warm water. The powdered flies were expelled at each act of vomiting; but the pain in the abdomen was not in the least relieved. Large injections of warm water were frequently administered, and ten grains of camphor, with one grain of sulphate of morphia, were given every half-hour, until four doses had been taken. Great relief resulted. Three hours afterwards, the patient passed water freely; his urine was natural, and without any trace of blood. He had suffered from priapism to a painful extent for a short time, but it had entirely subsided. The patient was sitting up; the pain was very slight, and did not return. He had suffered no inconvenience from the large dose of morphia. Four days later, he had pain in all his joints, especially the knees; his eyes were inflamed and painful; slight effusion was apparent in the knee-joints; and there was some inflammation of the sclerotic, which yielded to simple remedies, or, more probably, subsided spontaneously. His perspiration emitted a strong cantharidine odour, especially in the axillæ. Ten days afterwards he was able to resume his work.

The points of interest noted in the report of this case, are—

1. The length of time (about four hours) which elapsed before any perceptible effect was produced by the cantharides. Is not this analogous to the results of its external application?

2. The apparent want of action upon the stomach.

3. The large quantity of morphia taken without producing narcotism. This, however, is often observed in painful diseases of all kinds.

The quantity and quality of the cantharides taken could not be accurately estimated. The patient had taken only a slight supper, consisting of a cup of tea and a piece of bread.—*Boston Medical and Surgical Journal, and Philadelphia Medical Examiner*, April, 1855.

Fatal Poisoning of Five Persons by Tincture of Colchicum.—On Dec. 7th, 1851, M. Jules Roux prescribed sixty grammes (somewhat more than fifteen drachms) of quinine wine to five patients, named Steinger, Rougier, Ahmet-Ben-Salah, Gandillon, and Paradis. Four of these patients had undergone severe surgical operations, and were progressing favourably. One had tubercular disease of the testicle. They had all for some time been taking quinine wine, which was administered habitually in the presence of M. Roux during his visits. They were patients in the hospital of the convict prison of Toulon. On the present occasion, by a deplorable mistake, sixty grammes of tincture of colchicum were administered to each patient at eight A.M., in lieu of the quinine. No ill effects were at first felt, and some even said, in a low voice, that the wine was better than that which they had had before. In two hours, two patients were seized with heat in the epigastrium, colic, and vomiting. The surgeon on guard soon saw them, and found the five patients pale and cold, with small pulse, intense griping, nausea, vomiting, and abundant and frequent evacuations from the bowels. Warm water, tannin, and coffee were administered; sinapisms were applied to the limbs, and warmth to the surface of the body. M. Roux saw these patients at half-past two P.M., when the following symptoms were presented:—Paleness of the skin, general coldness, circulation very slow, pulse very small, and in two of the cases quite imperceptible; burning pain in the pharynx and œsophagus, unquenchable thirst, great heat of surface, intolerable pain in the epigastrium and entire abdomen, repeated vomiting, and numerous serous, yellowish stools, but without mucosity or streaks of blood. Intellect and speech, sensation and motion, remained entire. In one patient alone there had been, since the morning, some troublesome noise in the left ear. Nothing remarkable was observed in the state of the pupils. At this time, M. Roux could not discover what the patients had taken; but as the symptoms were evidently those of a narcotico-acrid poison, he prescribed mucilaginous and albuminous mixtures and injections, and ordered emollient poultices to be applied to the abdomen. A consultation was then held, when it was concluded that the poisoning arose from tincture of colchicum, and a draught of infusion of balm, sulphuric ether, laudanum, and syrup was ordered to be given every quarter of an hour. The poultices, mucilaginous drinks, and calorific means were continued. At five P.M. the symptoms remained the same in four patients. In Gandillon the vomiting and purging had ceased, the skin was warm and moist, and the pulse was raised. In spite of constant care, three patients died during the night—Ahmet-Ben-Salah and Gandillon at a quarter past three, Paradis at a quarter past four. At six A.M. the two surviving patients presented the following symptoms:—Burning pain in the throat, violent thirst, gripings, tenesmus of the rectum and bladder, pains in the loins and limbs, heaviness of the head, sense of oppression, coldness of the skin, lividity of the lips and nails. The vomiting had diminished; cramps of the legs were observed in Steinger, whom it was necessary to catheterise, on account of retention of urine. Rougier died at ten o'clock A.M., and Steinger at half-past one in the afternoon.

Thirty-six hours after the death of the latter patient, a post-mortem examination of all the patients was made. The temperature of the air was 68° Fahr. The appearances presented in all the bodies were so much alike, that the same description will serve for all.

The expression of the countenance was calm, the eyelids were half open, the pupils were normal; there was blueness of the nails, hands, and some parts of the skin; the skin was generally discoloured, but nowhere corrugated; *rigor mortis* was moderate. There was no ulceration, nor traces of ulceration, in the pharynx and œsophagus; the stomach and intestines contained a little gas and much turbid fluid; the mucous membrane was generally much softened—it was not ulcerated, but was red at different points. The liver was much congested; the gall-bladder contained a moderate quantity of bile; the spleen was gorged with blood; the kidneys were congested; the bladder contained a little urine, and its mucous membrane presented red spots of small extent. The heart was flabby, and contained a little black blood, and small clots of the same colour; the venæ portæ and inferior cava were distended; the blood everywhere had the appearance and consistence of currant jelly. The lungs were healthy and crepitant, and free from gravitative congestion: there was no effusion in the serous cavities. The brain and spinal cord were much injected; the cerebral spinal membranes were very red; the cephalo-rachidian and sub-arachnoidean fluids were small in quantity, as was also the fluid in the ventricles; some drops of blood exuded from the cut surface of the brain; there was general softening of the cerebro-spinal axis. The muscles surrounding the splanchnic cavities were of a remarkable deep-red colour throughout; the flesh was firm, and only showed slight traces of decomposition three days after death. Chemical analysis of the fluids evacuated and found in the bodies detected the presence of colchicum.

M. Roux, in his remarks on these cases, believes, with the Italian school of toxicologists, that colchicum poisons more by its depressing action than by producing inflammation of the intestinal canal. He founds his opinion on the following facts:

1. In his patients the poison did not commence to act until two hours after it had been taken into the stomach, and then only when it had been absorbed.
2. The depression of the powers was primary, and not secondary to the irritation of the digestive canal.
3. The traces of inflammation of the intestinal mucous membrane were not sufficient to account for death; but the profound alterations of the nervous system were perfectly in relation with the fatal result.

An inquiry was instituted on these cases by the Maritime Prefect of Toulon, and the apothecary by whom the mistake was made was acquitted by a majority of six votes against two. He was, however, removed from his post; but some months afterwards was reinstated. He has since died.

Not many years ago, and in one of the hospitals of Paris, seven patients died almost at the same moment, by taking, by mistake, a preparation of hydrocyanic acid.—*L'Union Médicale*, March 27th, 1855.

Poisoning by Aconite.—Bappoo Kishnura, a Hindoo priest, aged 50, was admitted into the Jamsetjee Jejeebhoy Hospital, in the afternoon of the 4th of August, 1854, under the care of J. PEET, Esq., Assistant-Surgeon. For twenty-four days he had been taking, daily, fifteen grains of a native drug called "bishnak" (a root of the aconitum ferox) as a remedy for leprosy, from which he had suffered for several years. Until the morning of the day of his admission he had been using the black variety of the drug, but at the recommendation of a friend he substituted the white variety. The dose was fifteen grains, and was taken in the morning, at ten o'clock. He soon afterwards began to feel uneasy, had a disagreeable burning sensation in the mouth and fauces, a sense of formication, and some confusion of mind. A friend recommended milk, of which he drank a large quantity. Soon afterwards he vomited freely; and about an hour after the vomiting came, and about four hours from the taking of the drug, he was brought to the hospital. He was then sensible, had a stupid expression of the face, and walked with an unsteady gait, like a drunken person. He complained of heat and burning in the throat, some confusion of mind, and of a sense of formication over the surface generally. There was almost constant vomiting; pulse feeble and rapid; skin coldish, and covered with moisture; pupils natural. A sinapism was applied to the epigastrium, and stimulants of ammonia were administered. The vomiting continued till twelve o'clock. Ultimately the animal warmth returned, the pulse rose, the vomiting ceased. He went home in the course of the day.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

Poisoning by Opium.—A man, of spare habit, and about 25 years of age, was brought to the Bhooj Dispensary by his friends, who stated that he had taken opium for the purpose of poisoning himself. He complained of giddiness and drowsiness. His pupils were contracted, and insensible to light, pulse small and frequent, perspiration profuse. He was treated by Mr. **Sadashew Hemraj**, Sub-Assistant-Surgeon, by an emetic of twenty-five grains of sulphate of zinc, but it did not operate. After this, the stomach-pump tube was introduced, and some tepid fluid thrown in and removed, but no opium could be detected; but it smelt of assafœtida, which was given to him before he was brought to the dispensary. He was made to walk about between two men; but in about an hour stupor came on to such an extent, that he could not be made to move, when he was placed in a chair, and electro-galvanic shocks were passed along the spine and back of the neck. Under this he improved and became alert, when he was again made to walk between two men In about a quarter of an hour stupor again came on, and the electro-galvanic shocks were repeated. In about three hours he was much better, there remaining slight drowsiness only. After recovery, he said that he had taken about three drachms of opium, four hours before he was brought to the dispensary, and that he had not taken any food for sixteen hours before that.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

Poisoning with Mushrooms.—Dr. **Maschka** relates seven cases of poisoning by mushrooms, which occurred in Prague, in September, 1854. The following symptoms were produced:

Cases 1 & 2.—W. M., a boy, aged 7; A. K., a woman, aged 70. In three hours after partaking of a dish of mushrooms, violent vomiting and diarrhœa, of a yellowish-white fluid, set in; the skin became pale and cool, but without cyanosis; the face pale and sunken; the eyes sunken and lustreless; the nose and mouth were dry. Vomiting left a burning sensation in the throat, and the diarrhœa was attended with crampy pains, extending to the lower limbs. The abdomen was not distended or tender; the urine was scanty; the radial pulse could not be felt; the carotid pulse was weak and very rapid; the cerebral functions seemed unimpaired; and the voice was not altered. In spite of treatment with emetics, oleaginous mixtures, and emollient drinks containing vegetable acids, death occurred in both cases in from seventeen to eighteen hours after eating the mushrooms.

Cases 3 & 4.—J. K., a boy, aged 7, and K. J., a boy, aged 10, were attacked, on the night of September 8th, after eating boiled mushrooms in the evening, with violent pain in the abdomen, vomiting, and diarrhœa. They were admitted into hospital at six A.M. on the 9th. The patient J. K. lay motionless, stiff, and speechless; the temperature of the head was raised; the pupils were dilated; there was spasmodic contraction of the jaws, and occasional grinding of the teeth; the skin was cold, and cyanotic in the lower extremities; the upper limbs were convulsed; the abdomen was in a state of meteorismus; there was no vomiting;

involuntary evacuations of fæcal matter occurred; the pulse was very frequent and thready. An emetic, cutaneous frictions with spirits, and a bath, were prescribed; but the patient died twelve or thirteen hours after taking the poison. [The fate of the patient K. J. does not appear.]

Cases 5 & 6.—K. C., a girl, aged 9, and J. C., a woman, aged 36, ate mushrooms at midday on September 7th, and were attacked in the evening with vomiting and diarrhœa. They were admitted into hospital on September 8th. They both complained of severe pain in the head and abdomen; the head was hot; the pupils were dilated; the speech was stammering and very low; the abdomen tender; the skin cool, and the lower extremities cyanotic. Vomiting had ceased; but there were frequent fæcal evacuations. The pulse was very small and frequent, and the strength was much reduced. K. C. was also very restless, and had convulsive movements of the limbs. Baths were used, and extract of opium with alum was given; but the diarrhœa continued throughout the 9th. In the morning of the 10th, sopor and stertorous respiration set in, and the patients died, with convulsive movements of the extremities, from sixty-six to sixty-eight hours after eating the mushrooms.

Case 7.—M. H., a man, aged 36, was seized in three or four hours with vomiting, diarrhœa, and pain in the abdomen. The symptoms which he presented were similar to those already related; and he died in sixteen or seventeen hours.

Post-mortem examinations of all these cases were made, and the *ensemble* of the appearances found is thus described by Dr. Maschka:—(*a.*) There was an entire absence of cadaveric rigidity. (*b.*) The pupils were dilated. (*c.*) The blood was generally of a dark-brown colour, and fluid, and had mixed with it here and there small, spongy, dirty yellow fibrinous clots, which were easily broken down by the finger. (*d.*) There were numerous ecchymoses and sanguineous effusions both in the serous membranes and in the parenchymatous organs. (*e.*) The bladder was excessively distended with urine.

Dr. Maschka observes that there was the greatest similarity between the symptoms in his cases and those which have been described by other writers as attending poisoning by mushrooms.

From a consideration of the remarks of Orfila and Krombholtz, and from some experiments which he performed on rabbits, Dr. Maschka concludes, with regard to the post-mortem appearances, "that entire absence of cadaveric rigidity, fluidity and dark colour of the blood, distension of the urinary bladder from paralysis, and numerous ecchymoses and effusions of blood in the serous cavities, and even in the internal organs, are appearances, which are as a rule observed in the bodies of those who have been poisoned with mushrooms."

Are the symptoms and post-mortem appearances which have been described above peculiar to cases of poisoning by mushrooms? Orfila found absence of cadaveric rigidity, mobility of all the limbs, and dilatation of the pupils to be the results of several poisons. He also observed, after poisoning with chloroform, ecchymoses in the pleuræ and lungs; dark blue spots on the lower borders of the lungs after poisoning with belladonna, atropine, hyoscyamine, and conium maculatum; and ecchymoses of the lungs, with a dark brown spot on the upper surface of the liver, after poisoning with cicuta. James found small ecchymoses in the endocardium and muscular structure of the heart after poisoning with arsenic; and Gaspard observed the same appearances in animals and man after corrosive sublimate; while Dr. Maschka found sanguineous effusions in the pleura after suffocation with carbonic acid gas. But, so far as is known to Dr. Maschka, no poison is followed by so copious extravasation of blood as was observed in his cases; and he hence infers, that the poison of mushrooms has a peculiar influence in producing fluidity of the blood, and consequent extravasation. Whether this be really the case, and whether the extravasations, when there is no evidence, chemical or other, of the presence of a poison, can be considered as certainly, or even probably, warranting the suspicion of poisoning with mushrooms, can only be determined by further researches.

With regard to the chemical detection of the poisonous principle of mushrooms, Dr. Maschka's examinations of the contents of the stomachs of the poisoned individuals add nothing to our knowledge of the subject. He was unable to discover any trace of the poison, either by optical or by chemical tests.—*Prager Vierteljahrschrift*, 1855, Band ii.

Poisoning by Nux Vomica.—Mr. De Crespigny relates the following case:—A Mahratta, admitted at noon of 31st December, 1853, said he had taken four seeds of nux vomica, each weighing three grains, powdered, in order to cure an ulcer on the back. He complained of giddiness, trembling of the limbs, and spasms, and walked with difficulty; he had not vomited, nor was there any inclination to do so. The pulse was natural, the skin warm, and the pupils natural. Two doses, each containing a scruple, of sulphate of zinc were given, but without producing vomiting. Two pints of water were injected into the stomach, and a feather was passed into the fauces, without avail; four pints of lukewarm water were then injected into the stomach, which produced copious vomiting; the injection was repeated in a quarter of an hour. The spasms increased in severity, and opisthotonos came on. There was temporary relief after the vomiting; but the slightest movement immediately produced spasms. The senses were not affected, though the speech was rapid. Vinegar and water were now given, and repeated every ten or fifteen minutes. Soon after the second dose, two strong purgative enemata were administered, without effect. After a third enema had been given, the bowels were relieved; he began to feel easier, and the spasms were separated by longer intervals. The spasms, which were referred to the cervical region, continued till midnight; the cramps extended upwards from the lower extremities; and the diaphragm seemed affected. He complained at one time of numbness of the lower extremities; but there was no loss of sensation. There was great giddiness throughout, and excruciating shooting pains through the head. The latest effects observed were occasional twitchings of the tendons, and a feeling of incipient spasm. He was discharged on the third day.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

Poisonous Nature of Tobaccó packed in Leaden Cases.—The *Union Médicale* for September, 1854, contains some remarks on this subject. The moisture contained in the tobacco oxidizes the lead, and forms a soluble salt. The tobacco becomes covered with a layer of acetate, carbonate, chloride, and sulphate of lead, amounting to from six to thirty grains in half a pound. Tinfoil has therefore, in France, been ordered to be used instead of lead. The presence of lead in tobacco is detected by burning the tobacco, treating the ash with dilute hydrochloric acid, evaporating, and treating the re-dissolved residuum with iodide of potassium, sulphate of soda, and sulphuretted hydrogen.

On a Substance produced near Aden, said to be used by the Somalies to Poison their Arrows.—Dr. Arnott had forwarded to him a watery extract, prepared from the root of a tree called "Wabie," a toxicodendron from the Somali country. The tree grows to the height of twenty feet. The poison is obtained by boiling the root in water until it attains the consistency of an inspissated juice. When cool, the barb of the arrow is anointed with the juice, which is regarded as a virulent poison. Dr. Arnott was informed that death generally took place within an hour. He could not, however, ascertain the quantities made use of by the Somalies. In some experiments on animals, in which the poison was administered by the stomach and by inoculation, which were conducted by Drs. Arnott and Haines, the fact seemed to be established that the poison was of no great activity. When death took place, the symptoms more resembled those produced by nux vomica than by any other agent. No apparent drowsiness; spasms slight at first, beginning in the neck, increasing in intensity, extending over the whole body, and finally stopping respiration, and with it the action of the heart. The experiments of Dr.

Arnott, as regards the effects of the poison when swallowed, differ from those of Dr. Haines. A dog treated by Dr. Arnott died in an hour from a dose of five grains; but two dogs treated by Dr. Haines suffered nothing for two hours after taking the poison. At the end of this time they were shot. A difference in the strength of the extracts seems to account for these peculiarities. When death took place, the post-mortem signs were collapse of the lungs and distension of the cavities of the heart, without any signs of inflammation.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

Swallowing Pounded Glass.—An artist, aged 25 years, was admitted into the Jamsetjee Jejeebhoy Hospital on the 30th of August, 1852, in charge of a policeman, who stated that the man was seized committing a robbery, and either on his way to jail or soon afterwards broke up and swallowed a quantity of glass of a bottle. The remainder of the bottle was produced—a reddish-brown quart bottle, such as Rhenish wine is imported in. The man complained of a little pricking sensation in the throat and stomach; no febrile symptoms were present. Emetics (of sulphate of zinc) were given, but did not cause the return of any of the glass. Later in the day one ounce of castor oil was given, which opened the bowels three times. The stools were not bloody, but in the bottom of the vessel were found a number of pieces of thin bottle-glass, precisely corresponding with the remains of the bottle shown by the policeman. About twenty fragments were thus passed, the largest about an inch long by a quarter of an inch broad; the smallest as large as a grain of rice. There was great tenderness of the epigastrium, so that leeches had to be applied, but no heat of skin or unusual quickness of the pulse. Under the simple treatment of a diaphoretic mixture, this patient had recovered by the 7th of September. The author who relates this case, Dr. R. Hains, remarks that cases of this kind are not uncommon in India, the idea of the poisonous nature of pounded glass being even more generally received amongst the natives of India than of Europe. It is generally taken for the purpose of committing suicide. When such large fragments, slender and sharp-pointed, have been swallowed, some degree of danger must always exist of the pieces striking crosswise in some portion of the intestinal tube, and producing ulceration, which might lead to the laying open of a large vessel, or of the cavity of the peritoneum. Hence it is always advisable to keep the patient under observation for ten or twelve days.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

II. Wounds and Injuries.

Injuries of the Head.—H. J. Carter, Esq., coroner's surgeon, Bombay, records three very curious and instructive cases of injuries to the head, and their results.

Case I. *Hæmorrhage and Death from Laceration of the Parietal Vein.*—A Parsee, in a fit of intoxication, fell with his head against a stone; this caused a wound, from which blood began to flow. He was taken to a liquor shop, and placed on a bench in a small room, about two o'clock in the morning. There he went to sleep, and was not seen again until five a.m., when the servant found him breathing heavily, and at seven he died.

Post-Mortem.—About two pints of blood were found under the head. A small lacerated wound, about an inch long, was found over that part of the sagittal suture where the parietal foramen generally exists. This wound had not extended to the bone, and although insignificant in appearance, it was obvious that the blood had come from it. On raising the scalp, no extravasation of blood appeared on any part of the cranium except opposite the wound, where there was a spot about two inches in diameter. The parietal vein was evidently ruptured, and to give an idea of its size, "the foramen by which it entered the cranium was about a twelfth of an inch in diameter." There was no fracture of the skull. The brain appeared to be more congested than usual. There was no extravasation of fluid in the brain.

The author, in remarking on this case, states that at the inquest he gave it as his opinion that the deceased died from hæmorrhage, but says that, on reflecting upon the subject, he thinks he should modify this opinion by adding, the man might have died from apoplexy brought on by drinking spirituous liquors, and the effusion of blood have taken place after his death, since experience shows that, in India, where the blood becomes rapidly fluid, nearly all the blood in the body will occasionally flow out of a small wound of the head or neck within a few hours after death, if the body be in a horizontal position. This is a very delicate medico-legal point. At the same time, it is not very easy to comprehend how the blood from the lower half of the body could escape from a wound in the upper half, when the heart has ceased to beat, seeing that it could only arrive at the bleeding point after having passed through the right side of the heart into the pulmonic circuit, thence back to the heart, and so through the arteries to the head or neck. Is there motion of the blood after death in India? or can fluid venous blood find its way through the veins in a direction just the opposite of its usual course?

Case II. *Death from Wound of the Vertebral Artery.*—A police naique was stabbed in several places by another policeman. The wounds were inflicted with a double-bladed clasp-knife, both sides of which must have been open, as the wounds were double at each place. He was suddenly heard to cry out, and on another policeman coming to the spot, he was found sitting on a bench, with blood flowing freely from a wound in the neck. He soon became insensible, and died about an hour after the infliction of the wounds.

Post-Mortem.—There were several marks of violence externally. On the right temple and on the left side of the neck, midway between the ear and the acromion, there were in each spot two wounds, one about three quarters, the other about half an inch in length. All were superficial, except the two in the neck. Here the largest and deepest passed behind the carotid artery and internal jugular vein, and between the œsophagus and vertebral column, terminating on the right side of the latter, but injuring nothing of any consequence in its course. The small wound led directly to the interval between the transverse processes of the third and fourth cervical vertebræ, where it ended by dividing the vertebral artery. The body was healthy. When the clasp-knife with which the wounds had been inflicted was examined, both blades were found open, and the little blade bent to one side, which accounted for the difference in the distance between the double wounds at the different places, and the direction which the small blade took to reach the vertebral artery.

Case III. *Fracture of the Odontoid Process, and Dislocation of the Second Cervical Vertebra.*—An engineer apprentice on board-ship was playing with a shipmate, when he tripped backwards over the chain cable, and slipped down, after which he looked up and laughed, and said, "Help me up." On trying to rise he drew his arm out of the hole through which the cable passes down into the hold, and suddenly becoming convulsed, gave a few gasps, and died. He did not appear to fall in the first instance with much force. For six months previously he had seemed to be in good health.

Post-Mortem.—Dr. Shepherd, who examined the body, found on opening the back part of the neck a rupture of the ligaments connecting the first and second vertebræ with each other and with the head, and also a dislocation of the second vertebra, the odontoid process of which was fractured, and pressed on the spinal cord. The body was healthy. The fall appeared to be attended with so little force that the fracture was attributed by some to the efforts made by the boy to recover himself in the act of falling.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

Death from Rupture of the Bladder following a Kick.—Dr. Henry Porter, of Peterborough, relates the case of a man, aged 36, who received, in a fight, a kick in the abdomen. He lost all consciousness until he was carried home, when he gradually recovered his senses, but remained in great pain all the night and the day following. When seen on the following evening, he had passed no urine. A catheter was introduced, but a flow of urine could only be produced by placing him in the erect position, when about four pints were drawn off, very highly tinged with blood. On the succeeding three days during which he lived no urine was passed, except through the catheter and in the erect position. There was no tenderness of the abdomen nor tympanitis, except on the last day, when some tenderness was present. On the morning of the day on which he died (the fourth after the receipt of the injury), he was seized with bilious vomiting. He died during the night. On post-mortem examination, there was a slight bruise externally, between the umbilicus and the anterior superior spinous process of the left ilium; and blood was extravasated between the abdominal muscles at this spot. The small intestines were distended with gas, and firmly agglutinated. The omentum was drawn up. The bladder had a large aperture in its posterior and upper surface, above the reflection of the peritoneum, of sufficient size to admit two fingers. There was no trace of any previous disease of the organ.

Dr. **Porter** remarks that the points of interest in this case are—

1. The absence of signs of peritonitis during life at all commensurate with the amount of inflammatory action observed after death;
2. The length of time which elapsed between the receipt of the injury and death;
3. The question whether the rupture was complete at first, or whether the peritoneal coat gave way at a subsequent period. Dr. Porter believes that the rupture was complete at first, for the following reasons:—First, there was a degree of collapse about the patient from the first; secondly, there was no change in the symptoms from chronic to acute, which would have probably occurred if the peritoneal coat of the bladder had given way subsequently to the injury; thirdly, there was no increased intolerance of pressure nor excess of pain a few hours before death; fourthly, the rupture was very large, and in the situation in which a catheter would pass directly into the cavity of the abdomen; fifthly, it was never possible to obtain urine through the catheter till the man was placed in the erect position.—*Association Medical Journal*, June 22, 1855.

III. MISCELLANEOUS.

Diagnosis of Blood-Stains on Old Clothes.—Dr. **Albert**, of Euerdorf, relates the following case:

A Jew pedler, named Sch. von O., complained before the authorities, that an individual, by name D. von S., had stopped him in a wood near Euerdorf, and, presenting a pistol, had threatened to shoot him if he did not immediately hand over the money he had about him. Although exceedingly surprised and alarmed, complainant stated that he did not lose his presence of mind; on the contrary, he sprang upon his assailant, snatched the pistol from him, and gave him two heavy blows with it on the back of his head, so that the assailant was glad to run off and hide himself in the wood. Complainant said further, that through these two blows the accused must have received a serious injury on his head, and must still bear on portions of his clothes traces of the blood which flowed from the wounds. The accused having on this information been taken into custody, was brought before Dr. Albert. The following was the result of an examination instituted five days after the commission of the alleged crime:

1. The whole of D.'s head was covered thickly with variously-coloured scabs, but there was no wound to be found on it. If such wound had really existed, it must have taken place under some rubbed-off scabs, which must have been speedily renewed; this was all the more probable, as on the spot where it was alleged that the blows had fallen, there were some scabs of a blood-red colour, and seemingly composed for the most part of dried blood. The other parts of the

man's body were covered, but not so thickly, with the same sort of scurfy eruption.

2. On the back of the man's linen jacket there were found twenty-six red stains, each about as large as a halfpenny, and a few more, which had been wiped away, on the hips of the trousers.

As D. declared that these stains resulted from his occupation as a lime-washer, the following examination was made:

1. Thirteen of the stains had a shining, and twelve a dull appearance.

2. Microscopically, the shining stains presented a fibrous sort of tissue hanging together in a mesh. The dark ones, on the contrary, had a looser and more pulverulent consistence.

3. Some of the shining stains, when rubbed, lost their colour, but remained as a tough yellow-brown mass; while the dark stains fell away altogether into powder.

4. Placed under water, the shining stains altered; the red constituents separated, and sank to the bottom without communicating any tinge to the fluid; but the red part of the dull stains was quickly and completely dissolved in water, imparting a red tint.

5. When the two fluids thus produced were tested with caustic ammonia, the first underwent no alteration; the last experienced a change to a violet-brown colour.

6. Nitric acid added to these fluids removed, in the first specimen, the colour of the red fibres, forming with them a light-red precipitate; in the last it caused a dark-brown thick discoloration.

7. Tincture of galls produced, after agitation in the first fluid, a muddiness and a precipitate; in the last it produced no alteration.

From these examinations the inference was drawn, that the fourteen first-named shining stains were blood-stains; but that the dull stains were produced by splashes of a red-coloured wash (*Englisch roth in Wasser gelöst*).

Upon hearing this explanation, the accused man changed his story, and stated that the blood-stains arose merely from his having carried a kid on his back, which he had killed by cutting its throat. Upon this, Dr. Albert instituted another inquiry, to ascertain whether the stains were or were not those of human blood.

A watery solution of one of the blood-stains was treated after Barruel's process with sulphuric acid; and a solution of another was treated as proposed by Friedberg—i.e., it was evaporated in a glass tube over hot coals. Thus treated, it gave forth an empyreumatic odour, but no other specific smell. The fluid that had been treated with the acid, emitted an odour similar to that which one perceives in a damp cellar in which cheeses have been kept.

A few drops of blood were then taken from the accused man, and were treated in a similar way; for the most part, the same results were obtained. The man's scurfy eruption emitted a similar smell.

The blood of several healthy persons was examined in a similar manner in larger quantities. A smell was only caused by evaporation, and this was intolerable.

Goat's and buck's blood, from old and young animals, were subjected to the same test; but nothing peculiar resulted.

Upon these experiments—which, by the way, are most unsatisfactory and loose—the conclusion was arrived at, that the blood-stains were probably not derived from a goat, but rather from the man himself; inasmuch as, on the treatment of his fresh blood with sulphuric acid, the smell emitted was so strikingly like that of his own perspiration and urine.

After the inquiry, the confession of the man proved that the blood-stains did arise from wounds in his own head, caused as the accuser had described.

From further experiments by Dr. Albert, he infers that the statements of Barruel and Friedberg, that the blood of different animals, treated as proposed by them, would betray itself by a smell peculiar to each, is not worthy of credit, and is inapplicable to judicial inquiries.—*Henke's Zeitschrift*, 1855, p. 392.

Infanticide in Prussia; its Detection and its Punishment by Modern Law.—Dr. VON HASELBERG, of Stralsund, concludes the narration of eight cases of infanticide with some medico-legal remarks.

In late years there have been several cases of infanticide in the district of Stralsund. In the eight cases related, pregnancy was denied, parturition was concealed, and the child destroyed. In one instance the mother died; in two cases, the legal proceedings were stayed; in three, the murderesses were declared not guilty by the jury; in one case, a verdict equivalent to "not guilty" was returned; and in one case only was the culprit found guilty. Paradoxical as it may appear, it is nevertheless true, that infanticide is permitted by the Prussian laws. By a clever use of the law as there administered, each unmarried pregnant woman may kill her child and remove it without becoming liable to punishment. She can deny her pregnancy; and knowing this, she resists all inquiry and examination, to make which no one has a right. She must then quietly wait till her confinement. After birth, the mother, without using violence, has only to cover the child carefully with the bed-clothes, and thus induce death by asphyxia. The first breath can with difficulty be prevented. The hydrostatic test affords evidence if the child has breathed and lived. By good fortune, the medical examination may prove only that the child has *probably* lived, and may have died of apoplexy induced by pressure on the head during birth. There are no marks of injury. The fact of murder cannot be proved, and the proceedings are stayed. Or the case may come before a jury (*Geschworenen*), and as soon as the jury hear anything of "probable" or "very likely," or perceive the least hesitation on the part of the medical witnesses, they assuredly return a verdict of not guilty.

Dr. Haselberg further observes, in a general way, that jurors, in his part of the world, are not particularly acute as to the value of evidence, or the skill of medical witnesses. It is usual to summon a second or a third skilled witness; but this may have only the effect of rendering the evidence more doubtful. If there be a difference of opinion, the evidence of the witness who is most popular or eloquent has most weight, though it be of the least real value.

In all these respects, the results obtained by the Prussian medico-legal tribunals are not unlike those obtained by the same tribunals in our own country.—*Henke's Zeitschrift*, 1855, p. 310.

The Composition and Physiological Action of the Water recently used in the Durham County Jail.—A paper on this subject appears from the pen of J. F. W. JOHNSTON, Esq., F.R.S. It seems that the well from which the water was taken has a depth of eighty-four feet, and is situated among the shoals and sandstone of the coal measures on which the city of Durham stands. It is built inside with stone and cement, and the water at present stands in it to the depth of twenty-one feet. Over the water rests an atmosphere of carbonic acid gas.

The pipes formerly used in connexion with the pump of this well were made of lead, but after a time these were eaten into large holes in various places, and coated inside with an incrustation, which is said to have been nearly an inch thick. In 1851 these pipes were removed, and replaced by pipes of cast iron.

The water when recently drawn has no sensible smell, and only a slightly saline taste. When heated in an open vessel, it speedily became covered with a thin white film; when boiled, it assumed a milky appearance, and gave a white deposit of sulphate and carbonate of lime.

When the well was sunk, no analysis of the water was made; and for ten years it was, with a few short exceptional periods, constantly used for the food and drink of the prisoners. By and by, Mr. Shaw, the medical officer of the prison, began to observe that certain peculiar and striking symptoms were manifested amongst the prisoners. Men in all classes suffered equally. These symptoms were those of glandular enlargements of the neck. Mr. Shaw thought that the water of the well was the cause of these peculiarities, but the fact did not come out accurately until six mouths later, when the pump being out of order, and filtered water being supplied from the river Wear, a marked improvement at once took place in the health of the prisoners. An analysis of the pump-water was therefore instituted before its use was recommenced. The results ran thus:—An imperial gallon of the water, evaporated to dryness, left of solid matter, dried at 270° Fahr., ninety-four and a half grains. When dried at only 112°, the solid matter amounted to upwards of a hundred grains. The solid matter consisted of—

	Grains.
Sulphate of lime (anhydrous)	31·38
Carbonate of lime	15·35
Sulphate of magnesia	4·49
Carbonate of magnesia	1·48
Chloride of magnesium	14·01
Chloride of sodium	6·19
Silica	1·28
Water retained by the salts of magnesia after drying at 270° Fahr.	16·82
Nitric acid, ammonia, and oxide of iron, of each a trace.	
Organic matter and loss	3·50
	94·50

Mr. Johnston remarks on this analysis, that the quantity of solid matter which the water contained alone rendered it, according to our present views, entirely unfit for domestic use. At present, waters are considered already somewhat impure if they contain from twenty to thirty grains of solid matter to the gallon. He also observes, that the chemical nature of the solid contents of the water had quite as much to do with its observed effects as the quantity it contained. He refers especially to the amount of sulphate of lime. He refers to the fact that the prevalence of goitre in the Derbyshire and Swiss valleys has been ascribed to the large quantities of magnesian limestone said to be contained in the ordinary drinking and culinary water used in those parts, derived as it is from mountain limestone districts. Mr. Johnston very properly connects these facts and inferences with the peculiar case which he has described, and intimates that the physiological effects produced by these waters may be connected with, or arise as a consequence of, the united action of the gypsum and the chloride of magnesium. In consequence of Mr. Johnston's report, the use of the water is discontinued in the Durham jail.—*Edinburgh Monthly Journal*, May, 1855.

In both a physiological and a pathological sense, the report given above is of deep interest; but it is quite in vain to attempt to unravel the problem involved from the data given. In the first place, it must be ascertained whether the chemical ingredients stated, really exist in the same condition in the water as it was drunk, and as they were found in the crucible. Secondly, particulars are wanting as to the manner in which the water was applied. Was the water drunk by the prisoners, as a general rule, freshly drawn from the well, or after having been boiled? And thirdly, might not some other agent have been driven over with the distilled water, of which we have as yet no account? As the water of the well still remains for observation, and its peculiar physiological effects are thoroughly brought out by a grand accidental experiment, it is to be hoped that the opportunity will not be lost of instituting a series of philosophical inquiries in relation to effects so obvious and suggestive.

HALF-YEARLY REPORT OF MICROLOGY.

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PHYSIOLOGY AND ANATOMY.

I. HISTO-GENESIS OF PARTS.

Epithelium.—GUNSBURG[*] (quoted from Canstatt's Jahresbericht) finds that the epidermal cells are completed in the fifth week of fœtal life; and from the fifth to the tenth week, in the most superficial layers of epidermis he found textureless membrane, with cell-formation interspersed, elementary corpuscles and molecules, from which he concludes that death of the most superficial epidermal layers occurs in this period. Cylindrical epithelium was seen by Günsburg to be developed about the eighth week of fœtal life in the respiratory and digestive mucous membranes; and he observed a layer of epithelial nuclei on the anterior surface of the capsule of the crystalline lens in a fœtus of four months old.

Nails.—GUNSBURG states that, in the embryo of thirteen weeks, the nails consist of lengthened nucleus-holding cells. The *nuclei* are dissolved by acetic acid, the cell-membrane remaining.

Pigment is said by GUNSBURG to be developed as a finely granular covering of the cell-nuclei. The aggregation of the pigment of the iris is said by him to be enclosed in cell-walls at the termination of the second month. There is a case in the Boston Medical Journal, April, 1854, in which the dark colour of the negro's skin is said to have been absorbed.

Lenses, &c.—GUNSBURG[†] states that, in a fœtus six weeks old, he found the entire lens to consist of large transparent nucleus-holding cells. In the seventh week, there were fibres formed in the central parts; and at the eighth week, the fibres were partly entire and partly split.

Connective or Areolar Tissue.—According to HESSLING and REICHERT, the development of this tissue proceeds in all cases from an intercellular substance. Reichert[‡] entirely refutes the statement of Kölliker, that there is a transformation of the spindle or stellate cells of the embryonic areolar tissue into a net-like, or simply cylindrical bundle. He does not deny that, in various forms of embryonic tendons, isolated cells are to be discovered. The chief mass at first is entirely tenacious, finely granular, afterwards obscurely striped, and only artificially divisible into fibrils or fibres, in which elongated oval nuclei are distributed. The spindle-shaped cell-membranes, which are indistinct, and grow into the so-called nucleus fibres, belong to the nuclei.

LUSCHKA[§] considers that connective tissue fibres arise out of the direct conversion of a homogeneous substance, for whose universal validity he has contended, at least as regards the fibro-cartilaginous layers of the pelvic joints; but this observation does not apply to the developmental processes. It was easily seen how the homogeneous fundamental substance of the hyaloid cartilage became changed into fibrils of connective tissue, between which the cartilaginous bodies were placed.

Günsburg says, on the contrary, that in a fifth-week embryo, the dermal fibres are developed out of united fibre cells, and many such fibres become so closely united to each other, that at a later period they appeared as anastomosing fibres. The dermal fibres differed from other connective tissue by much thinner and more delicate fibre-plates.

Fat.—According to ROBIN,[‖] the covering of fat cells is developed last of all the parts; and in atrophy of fat cells it is the first to depart.

[*] Verhandl. der Kaiserl. Akademie, Band xvi. Ab. 1, p. 264.

[†] Untersuchung über die Entwickelung verschied. Gewebe des Körpers.

[‡] Canstatt's Jahresbericht, Band i. p. 27. 1854.

[§] Arch. für Anat. et Phys., Band iii. Heft 2, p. 302.

[‖] Gazette Médicale, No. 16, p. 24.

Blood.—Drummond* seems to deny the position, that the nucleus alone of the original embryonic cell was transformed into the perfect blood corpuscle, such as has been advocated by Wharton Jones and Vogt.

In the blood corpuscles of the larva of the frog, formed out of embryonal cells, as well as in the lymph corpuscles, Drummond noticed that the fat granules which they contained diminished in proportion to the appearance of colouring matter within them; and from this fact, as well as from the observations of Weber on the destiny of the yolk-remains of the chick, he concludes that the colouring matter of blood is formed out of fat.

Drummond found slight fibrin coagulum in the heart of the embryo chick, about the third or fifth day.

Bloodvessels.—The formation of capillaries from star-shaped cells, as advocated by Schwann, is agreed in by Drummond, Günsburg, Kölliker, and Bruch (vide Canstatt). The two latter recommend for observation the tail of the tadpole, as also the fœtal crystalline lens capsule and the allantois. These authors, however, disagree in some points. Kölliker says that the primary origin of bloodvessels is by tubercles from the apposition of rounded angular cells in a straight line, and the absorption of their septa; and that from these tubercles proceed delicately-pointed processes, which, elongating, meet similar processes from stellate cells in the surrounding tissue, and unite with them. At the same time, the other processes of these cells unite, so that a network of the star-shaped cells is formed, continuous with the already-formed capillary tubes. Bruch denies the formation of tubes out of the rounded-shaped cells in the first instance. He also finds that the number of nuclei in the walls of the capillaries does not answer to the number of formative cells, but thinks that an increase of nuclei occurs, by division, after the union of the cells. Bruch also found that, as a rule, the projections from the star-shaped cells which are united into capillaries are arranged, not in a radial manner, but so that the spindle shape of the cells remains ascertainable. He investigated the peripheric extensions of bloodvessels in very small swine and ruminating animals, and saw how the spindle-shaped cells which form the vascular parietes, towards the finest ramifications always become more scanty, until at last but a few, sometimes only a single one, exist. These, which were stronger in proportion to their number, often had thread-like processes and anastomoses so fine, that one would not have imagined them to be hollow had not blood corpuscles been seen within them in places. In the chorion papillæ of the swine, Bruch observed that the formation of the vascular loops of which they are composed differs from that in rabbits and men; that fully-formed bloodvessels existed, and papillæ pushed out from the chorion before the bulgings took place, so that he describes this species of formation of the finer, not properly capillary vessels, as a kind of budding analogous to the production of glandular vesicles. The choroid plexus seemed to have a different formation.

Bruch found the cerebral capillaries fully formed in the bullock's embryo, one and a half inch in length. He states, that the spindle-shaped cells which at first compose the walls of the non-capillary vessels correspond to the inner vascular coat only; to which are added subsequently the annular fibrous and adventitious coat, formed out of new cells; whilst from within, as a secondary production, an epithelium is formed which, even in a very small embryo, can be recognised. This epithelium is said to exist, by Ségond and Robin, in the embryonic fine arteries and veins, but after birth it ceases to exist there. In mammalian embryos, Drummond found the spindle-shaped cells composing the walls of the larger vessels to be arranged in a direction exactly opposite to that described by Bruch. Most of them were placed more or less transversely to the longitudinal axis of the vessels, and only a few existed at right angles. In an embryo four inches long, distinct fibres, formed by the union of spindle-shaped cells, existed in the outer part of the vascular walls. Epithelium was perceived in the interior of the vessels, and this layer, in embryos eighteen inches long, still possessed its primitive appearance. In these embryos, in addition to an annular fibrous coat, Drummond says that a longitudinal fibrous coat existed (which, in Bruch's opinion, was the first one laid down), but not yet as a connected membrane. Kölliker thinks the development of the larger vessels, by the metamorphosis of capillaries, to be very frequent; and for all greater vessels arising after the completion of the organ, he lays claim to this method of formation entirely.

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* Monthly Journal, Sept., p. 214, and Nov., p. 385, 1854.

Muscle.—The abdominal muscles of a four-weeks' embryo appeared to consist of clear smooth tubes, according to GUENSBURG (op. cit.), of a greyish tint, capable of being split by pressure into the finest fibrils, and containing largish oval nuclei. The cross-striping was clearly to be seen. In a seven-weeks' embryo, the small muscles of the neck, the scaleni, intercostals, and psoas muscles, consisted of perfect striped primitive tubes; and in the muscles of the extremities the primitive fasciculi were formed out of united cells: yet, he thinks that the nuclei of these cells form the fibrils, and the fibril-plates originate from a melting down of the nuclear molecules in a direction from the centre. In a ten-weeks' fœtus, the formation of the sarcolemma was decidedly distinguishable from the formation of the primitive muscular fibre. In an eight-weeks' embryo, the muscles of the back were the most developed; and in a three-months' one the glutæi were. The ten-weeks' fœtus possessed perfect primitive fasciculi in the diaphragm and tongue, and those of the heart were half as thin as those of the glutæi, their development being from fibre cells; yet they had no cross-striping, which was found in the heart of a three-months' embryo. Günsburg recognised in the cleft membrane of the fibre cells, already united with fibres, the planning of the cleavage-splitting of the primitive bundles. He thinks the primitive fasciculus of the heart's muscle is formed out of a single fibre cell, whilst all the other muscles arise out of fibre cells from three to eight in number. He says that the smooth intestinal muscular fibres are formed out of a single cell, between the ninth and thirteenth weeks.

In a paper by SAVORY, in the 'Transactions of the Royal Society,' Jan. 11, 1855, the development of muscular fibre immediately beneath the skin of the fœtus of the pig, other animals, and man, is described. The nuclei, round or oval, became arranged and adherent, and then became invested by blastema, so forming elongated masses, the nuclei observing no regularity. The nuclei subsequently fell into rows, and became clearer, at this stage resembling the permanent form of many insects. The fibres elongate, the nuclei separate, and the fibres decrease by the falling in of the lateral bands, formed of condensed blastema. The nuclei fade away in many cases, and the fibre increases by accession of surrounding cytoblasts, which attach themselves externally, and become invested by blastema.

Nervous System.—The development of the axis cylinder in nerves, according to HARTING,* does not correspond to the original contents only of the embryonal cell, but also to the cell membrane; and the nerve-marrow deposits itself between the first secreted elastic sheath and the proper cell membrane, so that the last may be compared to the primordial membrane of plant cells.

Cartilage (Development of).—According to GUENSBURG the parietal membrane is developed in the form of spheroidal vesicles in the germinal layer of the formative cells. In these vesicles no discreet nuclear mass is distinguishable, nor is any molecular matter visible in their interior. The earliest non-nucleated vesicles remain in the middle of a layer of formative cells, unless they pass into a blastema, providing for a secondary development. The residuum of blastema becomes an intercellular mass, as is seen in ossifying cartilage, where a clear plaited membrane is formed. The form of cartilage cells is determined either indirectly by the displacement of intercellular substance, or directly by means of the fibrous capsule membrane.

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* See Canstatt, Band i. p. 63, 1854.

Glands: Salivary Glands.—The sub-maxillary gland of a four-months' embryo was found by **Ecker** to contain pear-shaped acini, which, gradually diminishing, pass into the twigs of the excretory ducts; and out of these acini in course of development the indented blind terminal canals of the perfect organ appear to proceed.

Seminal Glands.—The formation of the seminal canals, according to **Bruch**,* begins in the bullock somewhat earlier than those of the renal canals, in the shape of very short, proportionately wide, round or oval, groups of formative cells, which at first may easily be pressed from each other, then become more firmly connected together as the formation of sheaths goes on, and grow in length with them, but in the end continuously pass into the indifferent formative tissue. Along with the extension of this membrane, an inner cavity is formed, whilst the cells extend, epithelium-like, on the wall. By growth in length arise numerous windings, of which at first nothing is visible.

Kidneys: Wolfian Bodies.—In a bullock's fœtus, 8″ in length, the urinary canals were of variable size, ray-like, and provided with varicose bulgings, according to **Bruch**. In the Wolfian bodies, Bruch found the tunica propria to arise from united cells, and the gland vesicles appeared to develop secondarily, by the growth and bulging out of the membrana propria.

Liver.—In the embryo of the chick, **Remak**† observed round colourless transparent bodies, with a smooth membrane surrounding them, a thick wall consisting of delicate concentric layers, and containing a sharply-defined cavity filled with nuclei. This cavity was about one-third of the diameter of the entire cell, and contained nuclei varying in number up to thirty-two, furnished with simple or double nucleoli. The nuclei varied in size in reverse proportion to the age of the embryo, withstood acetic acid, the enclosing membrane swelling out, the parietal substance becoming transparent, and losing its laminated character under the use of water, acetic acid, and alkalies. The cells shrank under the use of chromic acid, alcohol, and solution of corrosive sublimate, and assumed a brownish colour under iodine solution, but did not become blue under sulphuric acid. In larger embryos, cells having two cavities containing nuclei were found. About fifty embryos above the size of one inch were examined, and all parts of the liver exhibited the same appearances. Before dissolution, the many-nucleated cells divided into cells with single nuclei. Then appeared cells with groups of nuclei more separated from each other, also smaller cells with less nuclei; and finally, cells with two or one nucleus, only distinguishable by their having a larger circumference, from lymph cells. These appearances were observed up to the twelfth day in newly-born rabbits, when, owing to the softened condition and tendency to destruction, the investigation becomes difficult. In the chick, this change begins as soon as absorption of the yolk begins. The above-mentioned cells are considered by Remak to have nothing to do with the hepatic cells, but to be rather elements of the connective vessel and nerve-holding fibre layer of the liver. They seem to have no connexion with the bloodvessels, and are in vain sought for in the blood at the time of their dissolution. Their function is (in the author's mind) most probably to fill up the cavities of the liver before the liver-cylinders extend themselves into lobules.

Spleen.—This viscus, up to the twelfth week, is composed, according to **Gunsburg**, of indifferent formative cells. In the thirteenth week, globular vesicles were seen, with from two to three nucleoli, just like the globules, he says, of the adult spleen pulp, blood corpuscles also, and manifold transition of formative cells into fibres. Capillary, as well as larger, bloodvessels were laid down. At the sixth month the transitional form did not exist; the trabecular scaffolding consisted of numerous ramifying bundles of nucleated fibres of connective tissue, and there were large blood corpuscle-holding cells. Günsburg considers the existence of spleen fibres before the last week of fœtal life to be problematical.

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* Canstatt's Jahresbericht, Band i. p. 70. 1854.

† Müller's Arch. für Anat. Phys. und Wissensch. med., Heft 1, p. 99. 1854.

II. HISTOLOGY.

Epithelium.—The epithelial layer of the *endocardium*, which **Luschka** and **Todd** and **Bowman** speak of as composed of two layers, is thought by **Kölliker** to be only composed of a single layer—the second or deeper one being, in his opinion, owing to the commencement of a diseased thickening of the endocardium.*

The anterior surface of the *iris* is asserted by Kölliker to have a layer of epithelial cells upon it (just as the posterior surface possesses one, the uvea, of pigment-holding cells), which in the newly-born, or in young subjects, are rounded, large, and flattened, constituting, when seen in a fold of the iris, distinct slight elevations. This assertion is opposed to the observations of Mr. Bowman, who thinks that there is *no* epithelium on the anterior surface of the iris. Kölliker even gives to this supposed epithelial layer a partial share in the production of the varied colours of some irides. An epithelium is stated to exist on the anterior surface of the capsule of the crystalline lens by Lohmeyer and Brücke, but this is denied by Kölliker, who asserts that there is one only on the deeper surface, composed of polygonal cells, which after death burst, and furnish the aqua Morgagni.

A distinct epithelium was obtained by **Luschka**, of Tübingen,† out of the pubic and iliac bone-articulation, which the author looks upon as a true articulation. It consisted of rounded cells, finely granulated, and generally containing a clear, refracting, and delicately-defined nucleus. Often the nucleus was invisible, and the corpuscle appeared quite granulated and irregular, possessing much finely granular molecular matter. Acetic acid rendered these cells almost invisible, and they were destroyed by strong potash solution.

Luschka also perceived epithelium of the venous textures, with thorn-like processes projecting sometimes free above the level of the other epithelial cells. These he considers to be agents in the transudations of serous fluid, thinking it improbable that in any normal process nature should act in this matter by simple mechanical transudation, seeing that as no impediment would exist to further transudation, the amount of such fluid-formation would be unlimited. As to this supposition, Henle opposes Luschka. Moreover, Luschka brings forward the ascertained chemical properties of the secretions of serous membranes, as showing that something more than a simple exudation of the blood serum occurs. Luschka supposes that these cells have the secretion formed in them, and that they are then separated. He explains the process by microscopical observations of the venous tissues of newly-dead mammalia and birds. He says that the free surface of the venous tissue contains different epithelial elements, which are perceived to be only a succession of one and the same fundamental pattern, the finely-granulated nucleus holding pavement epithelium. Besides these, there are, in all possible transitions, scantily-granulated spherical bodies with a nucleus. 2ndly. Spherical and, excepting a single contained nucleus, transparent homogeneous and very delicate cells. And 3rdly. Vitreous-looking vesicles, without corpuscular elements, with very delicate structureless walls, and mostly of very large size. These latter bodies are so pellucid and delicately-defined, that they can only be recognised by a suitable illumination. The final behaviour of these pellucid bodies is of the greatest importance. They are seen oftentimes to extend themselves, and at last to burst or dissolve; and out of them smaller and larger transparent drops, like fluid albumen, are seen to issue, which gradually become mixed with the water added to the object. It may further be seen how the cells in this way leave behind them no débris, but are totally dissolved, or only leave a very fine membranous coagulum. In the above description we recognise, according to Henle, the hyaline or albuminous drops which were first seen by Dujardin to proceed out of the walls of infusorial bodies, and which have been since observed in many other higher and lower animals. They were figured by Donders among the epithelium of intestinal villi, under the name of mucous globules.

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* See Canstatt, Bd. i. p. 27. 1854. † Arch. für Phys. Heilk. See Canstatt, Bd. i. p. 27. 1854.

MAYER[*] asserts that, in his opinion, small canals are formed amidst the fine granules which fill the epithelial cells, through which absorbed fluid is drawn by means of some contractile power of the nucleus, which thus becomes the chief organ of imbibition. We need hardly say that the idea is entirely hypothetical.

Here we may allude to a paper by VIRCHOW, in his 'Archiv,' April, 1855, On Cell Pathology, in which he dwells on the irritability inhabiting certain cell-elements, and quite independent of nervous influence; and quotes the observations of Leydig, who shows that the contractile substance of the lower animals, the so-called sarcode, is contained, as in the arm-polyps, in cells.[†] Virchow indeed claims irritability for all cells and cell nuclei, whether isolated, or grown together so as to form definite structures.

The depressions and markings which are visible on the superficial scales of epithelium, are looked upon by KEBER[‡] as pores and chinks, and are considered to be a manifold system of hollow spaces and cavities; and by rubbing in quicksilver, he states that he has witnessed its presence in the pores.

DONDERS[§] found that the fatty particles seen in the cylindrical epithelium of intestine during digestion, existed in all parts of the cell. After death they accumulated into large drops. The nuclei were free from fat.

Ciliated Epithelium was seen by VIRCHOW over the surface of all the cerebral ventricles, but it was the most marked in the fourth ventricle. Günsburg states that absolute alcohol quickly dissolves cylindrical and ciliated epithelium, which swell out and become pale in ether. The cilia remain unaltered in hydrated alkali, the cell walls swelling out, and the cell contents being dissolved. We have ourselves lately seen ciliated epithelium lining the frontal sinuses of a man who had been dead fifteen hours, the cilia working away with some rapidity.

Nails.—According to VIRCHOW,[‖] the lowest cells under the nail were cylindrical, and on the addition of acetic acid extended themselves into lengthened spindle-formed structures, more or less perpendicular to the surface. In the matrix, on the contrary, they were small, flatter, and of a dim yellow colour, with granular contents. The structures, interpreted by Rainey as follicles of the nail stroma, exist, according to Virchow, in a threefold form:—1. As small cylindrical bodies; 2. As large flask or pear-like bodies; and 3. As round concentric masses. All these lie at the bottom of the furrows, between the ridges. The first two were seen by Virchow to be closely connected with the surface. The concentric ones appeared to be entirely independent, and all three consisted of epidermal elements, a special membrane not being recognised. The smallest cylindrical masses contained small cells, disposed in a circular form, with transversely-placed nuclei. In the pear-shaped bodies, the cells of the upper part were arranged transversely; but in the final protuberance, the cells were more parallel with the surface, and contained fat-like glistening masses in the interior. The concentric structures consisted entirely of laminated epidermal plates, or contained a fat-like, glistening, structureless mass. Virchow supposes them to depend on a disturbed development of the reta.

Hair.—It is stated by Mr. P. BROWNE, in the 'Philadelphia Monthly Journal,' March, 1854, that the hair of different human races differs in form. It is cylindrical in the Red Indians, elliptical and oblong in the negro; and in the union of the negro and American, oval and flat intermixed.

Cornea, Lenses of the Eye, &c.—The presence of a true liquor Morgagni during life, which is denied by Kölliker and others, is asserted by LOHMEYER.[¶] Kölliker attributes the appearance of a layer of polygonal cells at the back part of the crystalline lens under the capsule to club-shaped enlarged and flattened ends of the lens' fibres, which are inserted into the inner surface of the capsule, and often leave delicate impressions behind them; and in the same way, the abrupt terminations of the fibres of the lens exist behind the equator of the lens[*]. Kölliker also asserts that the fibres of the lens are, in fact, tubes, which Reichert[†] thinks cannot be the case, from the completeness with which, on drying, their peculiar polyhedral character is retained. He imagines the fine and longitudinally-striped character which they have to be owing to a folding of their membrane, probably. In the neighbourhood of the equator, the nuclei of the original cells of the tubes, having grown into vesicles with nucleoli, remain in the centre of each tube. Kölliker says that, in the central stelliform figures of the lens its substance does not consist of tubes, but is partly granular and partly homogeneous; and the surrounding tubes gradually lose their distinctness on approaching the central "stars." The vitreous body is described by Doncan[‡] as consisting of four kinds of elements:—1. A peripheric layer of cells, strongly refracting light; 2. Very fine fibres, covered with granules, which were situated more at the sides than in the axis of vision; 3. Numbers of granules, of various sizes; 4. Folded membranous shreds in the anterior part of the vitreous body. The cells, which are identical with those described by Luschka, and considered by Brücke to be undeveloped capillary network in the fœtus, fill the entire vitreous body, and disappear from the centre. Those existing at the periphery are looked upon by Doncan as the result of a colloid or mucin-like metamorphosis of the contents, especially the nucleus; and he thinks that the vitreous humour is developed from these cells, which, partly by bursting and partly by the act of transudation, furnish the fluid. Doncan also thinks the textural parts, the fibre's granules, membranous shreds, &c., to be the result of secondary formations in the mucus formed by such cells. According to Hannover (Canstatt), the vitreous body in man consists of sections like an orange, formed by projections of the hyaloid membrane, converging to the centre. Doncan, however, in examining recent specimens without reagents, could see nothing of the membrane in question. He also used specimens hardened by acetate of lead and chromic acid, and in the human body found that the radiated striæ seen on section were owing to membranous septa, but were either indications of zones, as he thought probable from the arrangements of the embryonic cells, or were artificially produced. Kölliker thinks that the adult vitreous body consists merely of a mucus-like substance, all trace of the embryonic areolar tissue which it originally possessed being quite lost.

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[*] Verhand. der Rhein. Natur. Verein, Band x. p. 372. See Canstatt, Band i. p. 28. 1854.

[†] See Müller's Archiv.

[‡] Mikroskop Untersuch. über Porosität der Körper. Quoted from Philosophical Journal, 1854.

[§] See Canstatt, Band i. p. 29. 1854.

[‖] Würzb. Verhandl., Band v. Heft 1: quoted from Canstatt, Band i. p. 30. 1854.

[¶] Zeitschrift für rat. Med., Band v. Heft. 1, p. 56.

Elastic Tissue.—This is said by Harting (quoted from Canstatt) to assume a yellow colour on the addition of nitric acid, owing, as he thinks, to the moisture contained. This tissue is considered by Kölliker—or at least one intermediate between it and connective tissue—to form the proper choroid of the eye, as it does the ligamentum pectinatum of the iris.

Blood—Solubility of Corpuscles.—Dusch tried to ascertain[§] whether the bile dissolved the blood by reason of its water contained, as Virchow and Henle thought, or by its organic elements. He mixed equal parts of blood and ox-gall together, and in about two hours it became almost clear and transparent. He then found that almost all the blood-globules were dissolved, a few which remained being swelled out and cup-shaped. Again, instead of gall, he used distilled water, and the blood was even more entirely dissolved by degrees. In a mixture of three parts gall and twenty of ox-blood, the blood-corpuscles were all found to be globular, whilst in a like experiment with water instead of gall, most of them retained their accustomed character. The experiment with the single elements of bile proved that the glycocholate as well as the taurocholate of soda, the last especially, possessed a strong dissolving power, whilst after a shorter time the influence of those salts on human and frog's blood was to cause a solution of all

[*] Zeitsch. für Wissench. Zool., Band vi. Heft 1.
[†] Canstatt's Jahresbericht, p. 34. 1854.
[‡] Nederl. Lancet, Nos. 11, 12: see Canstatt.
[§] Untersuch. als. Beitrag. zur Pathog. des Icterus.

the blood-globules. The nuclei of frog's blood resisted the longest. The taurocholate of soda had the same influence on pus corpuscles. Blood corpuscles were found to be affected by an alcoholic extract of ox-gall in the same way as by those salts; and in order to prove that in this it is probably only the cholic acid, united with taurin, and glycin, which possesses the dissolving power, Dusch established experiments with taurin, by which the blood-corpuscles were seen not to become dissolved, but, after two hours' influence, were only indented.

Enumeration of Corpuscles, &c.—**Welcker**, continuing his experiments (some of which were alluded to in our April number, 1854) by means of determinate dilution of the blood,* found that in the three first vertebrate classes the proportion of blood to the weight of body was as one to twelve, whereas Valentin estimates it as one to five. He determines the relative proportion of colourless to coloured corpuscles in the blood to be as follows:

In a healthy girl, aged 19, blood taken from a vein gave the proportion as	1 to 157
Blood taken from a hysterical girl in the same way, gave the proportion as	1 to 506
In blood from Welcker himself the numbers were	1 to 341

In these three experiments, a cubic millimètre of blood contained respectively about 12,133, 8201, and 13,369 colourless corpuscles.

The number of colourless corpuscles in blood has also been carefully and elaborately estimated by **Moleschott**.† He finds that, as age increases, the colourless globules diminish in proportion to the red ones. They are increased by a slightly albuminous food in proportion to the like amount of red ones, much more than by food less albuminous; and their average number in man he considers to be about 1 to 357 of the coloured ones. Their number is below the average in those fasting or not menstruating, and in the aged; and in the pregnant, or those menstruating, or in children and young men eating plentifully of albuminous food, the white corpuscles are above the average. Colourless corpuscles were found in the splenic blood of a person one hour and a half after death, in the proportion of 1 to 4·9; but probably cells from the spleen-pulp may have been inadvertently included, owing to pressure of the organ requisite in the examination.

Bloodvessels—Arteries.—The helicine arteries in the penis are considered by **Segond**‡ to be produced artificially by the tearing of elastic fibres which roll themselves up. Such was the view of Henle and Valentin. **Gerlach** and **Kölliker** both agree in thinking them true vascular formations, and not produced artificially. They suppose them not to be cæcal at their extremities, except in rare instances, and point out offshoots of fine vessels from them which, being further continued, end in venous spaces, intercommunicating.§

Capillaries.—As to varieties of capillaries, **Segond** agrees with **Robin** that there are three different kinds—namely, those which consist respectively of one, two, or three coats, which imperceptibly pass one into the other, and under which are included the finest arteries and veins. In the second variety are transversely-placed nuclei, but not in any of them could Ségond perceive any inner epithelium. Robin says that the transversely-placed nuclei of the second class are more scanty in the capillaries of the brain, spinal cord, and medullary tissue of bone. He denies, along with Verneuil (*le système veineux*), the elastic longitudinally-striped or fenestrated coat of veins.‖

Lymphatic Vessels.—According to **Gerlach**, the membrane of the central chyle vessel of the intestinal villi is entirely structureless. These villi, according to Brücke, in many animals, as weasel and mole, contained vessels branching out like network; these vessels were light in colour by reflected light, and dark on transmitted light, differing in arrangement from the transparent chyle vessels, and looking like bloodvessels filled with small brown granules. Contents of the vessels of the villi, similar to these, were observed by Virchow in the human body, consisting in one instance of large-sized angular granules. These granules were not chyle or fat, as they were soluble in alcohol, and insoluble in ether. Some time ago, Brücke and Gerlach had stated that the blood capillaries of the villi were filled with fat-granules, and thus had the appearance of branched lymph vessels. Possibly both observers had examined preparations like those described more recently by Virchow and Brücke. Kölliker withdraws what, in 1852, he stated respecting the origin of lymphatic vessels in the trachea; he thinks it possible that such vessels were only bloodvessels filled with granular matter. He states that the deeply and longitudinally-situated muscular fasciculi of the adventitious coat of the lymph vessels, which are found on vessels of not more than 1-10‴, are distinctive between them and small veins. DONDERS recognises the muscular elements in the covering of the lymphatic glands described by Heyfelder, but not witnessed by Kölliker.*

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* Prager Vierteljahrschr., Band iv. p. 11.

† Wiener Wochenschr., No. 8: from Canstatt.

‡ Traité d'Anatomie Générale.

§ See Canstatt, Band i. p. 46. 1854.

‖ Ibid.

Muscle.—The primitive bundles of striped muscle, according to HARTING,† consist of ellipsoid bodies, arranged in a linear direction of from 0·0001 to 0·0017 millimetre in thickness, and from 0·0002 to 0·0035 millimetre long, filled with fluid, as he concludes, from the alteration in form assumed by them during contraction of the fibrils. By means of a stimulus, they became cylindrical instead of ellipsoid. They are imbedded in an intervening substance. In muscular fibres which have lain for some time in hydrochloric acid, the elements separate in a longitudinal direction, often so complete that the juxtaposition of the elements which lie in a transverse plane alone remains, and constitutes the well-known discs. Muscular fasciculi, which are beginning to be affected by the gastric juice, show the same changes. DONDERS states that the sarcolemma of the heart's muscle is rendered very clear by treating it with diluted hydrochloric acid; and according to Harting, the fibres of the heart's muscle, when seen on transverse section, are found to exist in radii proceeding from the central point of the fibres. This same arrangement has been seen in other muscles by KÖLLIKER. Some remarkable structures, discovered by Purkinje under the endocardium of ruminants, and adopted by Kölliker under the name of "cross-striped muscle cells," have been described by HESSLING.‡ He terms them "grains," and met with them in greatest abundance in the left ventricle, under the endocardium; also in the substance of the heart's walls, especially in the chamois; and not seldom in the outer part of the heart, under the pericardium. These grains, so-called, lying thickly near each other, compose a gelatinous kind of network, visible to the naked eye, under the endocardium. The threads composing the network are surrounded by a sheath, which is—in the sheep, for instance—formed out of a thin structureless membrane, furnished with nuclei; but in other instances consists of a highly vascular connective and elastic tissue, connected with that of the endocardium. The "grains" are described by Hessling as being generally of a rhombic shape, very transparent, and of a waxy consistence. They are sharp at their edges, finely granular, and generally striped in various directions—occasionally being filled with fat granules. In their centre lie from one to three roundish vesicular or granular nuclei with nucleoli, mostly surrounded by fine grey or golden-coloured glistening pigment granules, which in the chamois are very numerous. The nuclei increase by division, and the striped appearance seems rather to answer to foldings of the surface, or shallow impressions. By the use of acetic acid they swell out, the cross stripes being rendered more clear—as they are also by nitric or hydrochloric acids and alcohol. True capillaries, but no nerves, were found amongst the above-named network. There seems to be much doubt as to the character of these grains, which are looked upon by Hessling, as well as by Kölliker, as being possibly pathological. WEBER mentions a case of hypertrophy of the tongue, in which, after amputation, a relapse occurred, and in the portion excised on the second occasion elementary materials resembling embryonic primitive fasciculi were found.

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* See Canstatt, Bd. i. p. 50. 1854. † Ibid. ‡ Zeitschr. für Wissench. Zool., Bd. v. Heft 2.

According to MAZONN, of Kiew, the contractile fibre cells of smooth muscle, described by Kolliker as existing in all four vertebrate classes, do not exist as such, but are only fragments of properly continuous fibres. He denies also the existence of the rod-like or columnar nucleus, founding his observations on muscular fibre macerated in dilute sulphuric acid.

The radiating or dilating fibres of the iris, according to Kölliker, consist of numerous slender fasciculi, uniting here and there in a net-like way, at sharp angles, which pass from the outer edge of the iris, and in an arched form pass into the sphincter, the fibres being often so arranged as to form regular arches. Other weaker fibres pass posterior to the sphincter, or between the fibres towards the pupillary margin, mostly without quite reaching it.

Nervous System: Nerves—Ganglion Cells.—GERLACH withdraws his former views (op. cit.), and now considers the axis-cylinder of the nerve to be a structure different from the nerve-marrow (white substance of Schwann). Among those nerves which are non-medullated, or marrowless, the olfactory nerve of man and the ox are to be ranged, according to KÖLLIKER. The latter establishes the existence of ganglia in renal nerves, only, however, in the main branches of the pelvis; never in the true renal substance in man and calves. In man the ganglia seem to be a-polar, whilst in the sheep numerous fine fibres take origin from them. Gerlach assures himself of the loop-like terminations of primitive fibres of nerves in the secondary fungiform papillæ of the tongue, and he is partly supported also by new examples on the part of Kolliker, who thought he detected these loops, as for instance in warts on the clitoris in man and the pig, and possibly in the iris. The supra-renal bodies were found by Kolliker to have large numbers of nerves distributed to them, nerves without any of the fibres of REMAK. These penetrated in all directions into the central parts, without entering the cortex, forming a very rich plexus, in the mammalia, in the medullary parts, but the method of termination of the nerves could not be ascertained. Ganglia existed on the nerves outside the organ.

Retina.—REMAK describes the structure of the retina at some length.* He states, that next to the "membrana limitans," extend, in the direction of the meridian of the eye-ball, the textural fibre fasciculi. In the interval are crowded thickly-placed multi-polar ganglion cells, which belong to the cell-layer existing on the outside of the optic nerve-layer. One may then discriminate on the inner surface of the retina fibrous and gangliated meridians. The branched projections of the ganglion-cells have the properties of nerve-fibres, and unite with the third very thick fibrous layer, from which exceedingly delicate varicose fibres, like the fasciculi of the optic nerve, run from behind forwards. On this fibrous layer follows again a layer consisting of small multi-polar ganglion cells, which in many places is separated by a very thin fibrous layer from the so-called granular layer. In the last one, neither nerve fibres nor ganglion cells appeared. It consisted of nuclei-holding fibres, arranged in a radial direction, which, on their extended nucleus-holding outer surface are beset with the well-known little rods and pegs. Projections of these fibres pass through the layer of the retina in a radial direction, and form, by means of broader thickly-crowded anastomosing enlargements, the "membrana limitans," without ever affording a resemblance to, or connexion with, the nerve fibres. These radiating fibres, discovered by H. Müller, appear, like the limitary membrane from which they proceed, to serve the retina as a support. In the district of the "yellow spot" there run, according to Remak, the layers of the retina, consisting of nerve fibres and ganglion cells in a gangliated sheet, formed by multi-polar ganglion cells, whose thinned, pitcher-shaped, central part (*Fovea optica*) in the sound eye of a boy, appeared to be perforated by a fissure. Up to this cavity a very thin projection of the inner fibrous layer passed, but here ceased the radiating fibres of Müller. The ganglion cells were surrounded by firm sheaths, from which proceeded the short stems of the pegs existing on the outer side. Between the above-named cavity and the choroid is a substance of deep yellow colour, clear as glass. In the other parts of the spot the stems of the pegs are very long and twisted, and, as it appeared, form a thick, very yielding cushion, upon which the gangliated lamina rested.

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* Deutsche Clinik, No. 16: quoted from Canstatt. 1854.

For **Kölliker**'s observations on the extension and termination of the cochlear nerve of the ear, in which, to a certain extent, he agrees with Corti of Turin, see his Histology.

Wagner[*] considers that all so-called a-polar cells do not exist as such, but that in all cases, excepting perhaps in those of the heart, about which there was doubt, they were mutilated bi- and multi-polar cells. He also considers that uni-polar cells in the brain and spinal cord are doubtful. Many apparently bi-polar cells in the brain he recognised as multi-polar, and he looks on the granular material between the cells as a bed for bloodvessels, and also as a matrix for the formation of new ganglion cells. The union of the ganglion cells described by him in the electrical apparatus of the torpedo, could not be found by **Remak**, who asserts that the cells are all multi-polar, and that their processes partly enter into the roots of the electric nerves, and partly into the spinal cord. The cells in spinal ganglia were found by Remak to be bi-polar; but in the sympathetic ganglia they were multi-polar, with from three to twelve processes. They were more numerous in the solar plexus than in the terminal ganglia. In these ganglia the bi-polar cells differed from those in the spinal ganglia, by the branching of both processes, so that they were equivalent to multi-polar cells.

Spinal Cord and Nerves.—**Bidder** observed the transition of primitive fibre cells of the roots of the spinal nerves, as also the longitudinal fibres of the white substance, into cells of the grey substance in great abundance.

In an Inaugural Dissertation before the University of Dorpat, by **Phillip Owsjannikow**, the author has entered at great length on the structure of the spinal cord and spinal nerves. The conclusions drawn from the observations were as follows :—

1. That all the fibres of the spinal nerves which enter into the spinal cord are united to gangliated cells.

2. That one filament extends to each gangliated cell from the anterior spinal root, and one from the posterior root; a third, a commissural one, from the other part of the spinal marrow; and in many fishes a fourth, passing from the brain. The presence of this single fibre passing to the brain may, as the author throws out, be of moment in reference to the question of the possibility of the same fibre being both afferent and efferent in function, a position which **Du Bois-Raymond** thinks tenable, as judging from his experiments, though, as a rule, this power is not put into action, in his opinion. The author of the paper now considered, thinks, on the contrary, this aforesaid power is always put into use.

3. That from each cell of the spinal marrow, a filament extends to the brain, forming the white substance.

4. That the chief mass of the spinal marrow, containing fibre and cells, is a united areolar web, which being arranged in great abundance about the central canal, and furnished with numerous bloodvessels, produces the ruddy grey colour of the substance which is generally supposed to be owing to pigment cells.

5. That the gelatinous substance of **Rolandi** is connective tissue.

6. That the cells found as well in the posterior horns as in the surrounding substance of Rolandi, are corpuscles of the united web.

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[*] Neurologische Untersuchung. See Canstatt, p. 61. 1854.

7. That the cylindrical axes are of a round form, and are composed of the same substance as the nervous cells.

8. That the cylindrical axes in the grey substance are formed of a membrane peculiar to themselves, which surrounding also the nerve cells, may be separated from the fundamental mass composed of the united web.

9. That in some fishes, the cylindrical axes of the spinal cord are exposed, the cellular web in which they are placed forming no special investment.

10. That in those fishes which have anterior and posterior spinal roots, round gangliated cells are found, sending out in various directions divided branches.

Very extensive observations have apparently been made on the question of the extent to which spinal ganglia are centres of nutrition for the sensitive nerves, by SCHIFF.* He arrives at conclusions differing from those of WALLER, reported on a previous occasion. He states as follows:—When the posterior root, between its origin and the ganglion, is divided, and the stump of the root remaining on the ganglion is short, then it becomes degenerated by inflammation. If the ganglion partook of the inflammation, then the entire nerve degenerated. But if the inflammation was only very slight, then the sensitive nerve remained normal, as well above as below the ganglion, in spite of the separation from the central part: the stump of the nerve connected with the spinal cord alone degenerating In the motor nerves the case was reversed, but in all experiments with these nerves some small normal fibres still remained in the degenerated nerves, and to these corresponded single degenerated fibres in the not otherwise degenerated stump of the motor nerve connected with the spinal marrow. Schiff considers these fibres as returning from the undegenerated sensitive nerves, from which the anterior spinal roots obtain their sensibility. He found only degenerated fibres in the muscles; and thus Waller's view, that no nerve passed into these structures from the posterior root, seemed to be confirmed.

According to WAGNER, the grey commissures before and behind the spinal central canal, are only transverse commissures for the ganglion cells. He states also that he can classify the primitive fibres of the brain in five sets, according to their size. They all have axis cylinders, and union between them exists only by intervention of ganglion cells.

Pacinian Bodies.—Pacinian bodies were observed in the clitoris of the swine by NYLANDER and KÖLLIKER. They had previously been observed by FICK on the glans penis. They have been found by LEYDIG in the interspaces of the bones of the forearm of the mouse, being 0·72‴ long; their central space was filled with pale granular substance.

Uterine Organs, &c.—The amnion of the human fœtus is described by REMAK† as consisting of a thick layer of striped connective tissue, which, at distances of about $\frac{1}{25}$‴, contained foramina of $\frac{1}{50}$‴ in size, and of a granular cell-layer; and between these two layers existed tubular or stellate cells, with one or more nuclei, forming a network with very wide interspaces.

Cartilage and Bone.—According to FORSTER,‡ normal cartilage increases internally by an endogenous increase of the cells, externally by a transformation of connective into cartilage tissue. This transformation does not quite accord with the plan suggested by VIRCHOW, inasmuch as the fibrous blastema of the perichondrium is transformed into homogeneous cartilage substance, the connective-tissue corpuscles increasing and becoming surrounded by a clear glistening capsule. The ray-like processes of the cartilage cells which become bone canals, belong, in his opinion, to endogenous cells; they grow during ossification, gradually pierce the thick capsule wall, and unite with the projections of other cells.

REDFERN does not recognise the measurements made by Toynbee and Carpenter, indicating that the thickness of articular cartilage diminishes as age advances.

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* Arch. des Vereins für gemeinschaft, Arb., Band 1. Heft 4.

† Müller's Archiv, Heft 4, p. 369. 1854.

‡ See Canstatt, p. 67. 1854.

Kölliker also is opposed to the distinction made by Bruch between cartilaginous pre-formed bone, and bone formed out of secondary blastema; for the description of which, see our April number last.

Glands and Ducts: Stomach and Intestinal Glands.—**Gerlach**, like Donders, agrees with Bruch in considering Peyer's glands as analogous to lymphatic glands. No clustered glands were found by **Schaffer**[*] in the mucous membrane of the human stomach; and he looks upon those found by Handfield Jones in the pyloric wall as pathological, and owing to partial destruction of single tube membranes, and the massing together and union of the débris.

Sweat Glands.—These were found, like those in the axilla, by **Sappy**, in the walls of the anterior and lateral parts of the thorax. The difference made by **Kölliker** between the excretory ducts of the large and small sweat glands is opposed by **Tobien**. The latter perceived, external to the epithelium of all gland ducts, a structureless layer, beset with elongated oval nuclei, which he considers to be identical with the layer looked on by Kölliker as muscular. The ducts of large axillary sweat glands were not always found by **Donders** to contain fibre cells; but he always met with a structureless membrane, having at first sight a fibrous look, but which, on treatment with acetic acid or alkali, was seen to be homogeneous, without trace of nuclei.[†]

Salivary and Lachrymal Glands.—According to **Tobien**, great differences existed in the structure of Steno's duct. In an old soldier, two muscular layers were found, the inner one longitudinal, the outer stronger and annular. In the bodies of five young men, an elastic fibre network in four layers took the place of three muscular layers in Steno's duct, alternating in a longitudinal and circular direction, the outer one being longitudinal.

Wharton's duct possessed two elastic layers under its epithelium, the outer one having a circular course; and the pancreatic duct consisted of areolar tissue, including two layers of elastic fibres, the inner longitudinal, the outer circular.

Seminal Glands, &c.—The seminal canals were found by **Kölliker** not to contain any muscular fibre, and at the upper part of the epididymis there were only short fibre cells with short nuclei.

Lymphatic Glands.—These glands have been examined and described by **Beck**.[‡] His results differ somewhat from received views. He found that the coverings of the glands consisted of strong areolar tissue and much elastic fibre, which passing in all directions into the gland, formed a firm stroma, with numerous interspaces or alveoli. He found not any muscle fibre in the covering. Between the fibres of the stroma, in the closest proximity to the absorbent vessels, ran the blood-vessels, which divided into many branches, forming a rich network of vessels around the absorbent glands and alveoli. No direct communication was found to exist between the veins and the absorbents. The spaces which give to the gland a vesicular appearance, contained no special fluid or granules, no pulp or parenchyma, but only elementary lymph corpuscles. The absorbent vessels around the capillary vessels divided freely, formed projections and pouches, which were filled with lymph cells. Beck also found lymph vessels taking their origin in the gland, evidently in connexion with the alveolar network, the thinning of the vessels' walls being quite gradual. Beck agrees with Heyfelder in thinking that no special parenchyma, no free cell-spaces, existed in the lymphatic glands; but that meshes of the stroma were full of absorbent vessels. He thought the round or somewhat oval bodies in the interior of the vessels, and partly in the canals between the fibres, were merely true lymph corpuscles. In his opinion, the lymphatic glands might be defined to be simply a network of well-protected blood-laved absorbents, the starting points of new lymphatic vessels and lymph corpuscles. He compares their structure to that of the spleen, in which he found no special parenchyma, as did not Bennett and Schultz.

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[*] Arch. für Path. Anat. und Phys.

[†] See Canstatt, p. 71. 1854.

[‡] Illustrirte Medizinische Zeitung, Heft 5.

The above description—which is not altogether very complete, inasmuch as the mention of certain important points is omitted, such as the exact relation between the afferent and efferent vessels—will be seen to differ materially from that accepted by Kolliker in several particulars. Beck seems to consider that the morphological elements within the alveoli of the stroma are only elementary lymph corpuscles, as it would seem, formed in the glands. **Kölliker**, on the other hand, considers that there is a definite parenchyma or gland substance, and that the cell elements, &c., have no direct connexion with the lymph as lymphatic corpuscles. He speaks also of the alveoli containing a fine vascular network—a fact not mentioned by Beck. They seem to agree, however, to a certain extent, in considering the alveoli as a modified part of the lymphatic vessels going in and out of the glands. Kolliker had not met with the muscular tissue in the covering and septa of these glands, as had Beck and Heyfelder.

Liver.—The minute anatomy of this organ has been lately examined by **Beale**,* who, from his dissections and injections, comes to the following results:

1. That the essential constitution of the liver is that of a double network of minute vessels, one of capillary bloodvessels, and another of cell-containing tubes, naturally adapted to each other. Both of these sets of tubes in each lobule appear to communicate with those of the neighbouring lobules in all livers excepting that of the pig; and this circumstance is connected with the fact, that in all other animals but the pig the hepatic lobules are not isolated by intervening and limiting fibrous tissue or capsules. As to the latter position, Beale agrees with Weber.

2. That the cell-containing tubes are in all vertebrata continuous with the ultimate fine ducts of the viscus; in some cases directly so, whilst in others, as in the rabbit, and slightly in man and the dog, a fine network of the ducts themselves intervenes. The basement membrane of these tubes being, after fœtal life, incorporated with those of the capillaries, so that the secreting hepatic cells are only separated from the stream of blood by a single intervening membrane. The cell tubules contain the hepatic cells, as also granular and colouring matter and cell débris; the cells observing no order of arrangement, as some have thought, and contrasting in size, &c., greatly with the epithelium lining the ducts, from which they are strictly separated.

3. That the fine ducts are many times narrower at the point where they are continuous with the cell tubes, than those tubes themselves; and that the larger ducts and larger interlobular ducts freely anastomose with each other.

4. That whilst the finest biliary ducts are only composed of basement membrane, that of the larger ones is more complex, containing numerous cavities; especially in the pig, which, although generally considered to be glands, are in fact reservoirs for the bile, retaining it, and bringing it into intimate relation with the abundant surrounding bloodvessels, so that it may undergo requisite changes. This the author also considers to be the function of the vasa aberrantia, so named by Weber.

In this view it will be seen that Beale considers the structure of the liver to be strikingly different from that described by Kolliker and Handfield Jones, and assigns a different office to the secreting and epithelium cells; for whilst the latter looks upon the cells of the ducts as chiefly forming the bile, Beale considers that they stand in relation to the hepatic cells as the columnar epithelium (lining the stomach tubes) does to the secreting cells at the bottom of them.

Beale prepared his specimens by injecting the portal vein with lukewarm water until the bile was washed out of the ducts by it, and then injecting the ducts; after which the portal vein was injected with size. The ducts were also examined in specimens hardened in alcohol, to which a solution of soda had been added, in order to render the sections transparent.

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* Proceedings of Royal Society, June, 1585.

Dusch[*] finds that the hepatic cells are dissolved in bile and in solutions of glycocholate of soda. They also enlarge on the addition of chloroform, according to **Lereboullet**, their contents becoming very clear.

Vascular Glands.—The Spleen.—This organ has been examined at some length by **Fuehrer**,[†] who came to the following results. He states that a free parenchyma did not exist, but the whole spongy tissue was composed of arterial and venous vessels, along with a very remarkable capillary network of so-called capillary cells. The capillary cells are nothing else than the well-known chiefly crescentic fibre cells, with large bulging nuclei, looked upon formerly as muscular, but considered by Führer as the special seats of blood formation. These cells were considered by the author to be very like the free blood-corpuscles floating around. At one time they were smooth and homogeneous; at another, finely granular, and often slightly-coloured yellowish-red. On the other hand, in the splenic blood there were blood-corpuscles, which were provided with a single or double appendage, just like those nuclei after they were squeezed out of the cells. There were found also, especially in spleen rendered spongy by exudation, connecting networks of these crescentic cells, unless the isolated ones were separated by a special sheath-wall, so that they formed in consequence a branched tubular system, in which these nuclei lay disposed singly. Führer noticed the continuous connexion of such capillary-cell network with clear bloodvessels—the nuclei-holding tubes becoming capillaries, which contain young pale blood-corpuscles at various distances—that is, the nuclei of the so-called crescentic fibre cells. Sometimes the nucleus fails in the above cells, and it is only indicated by a spindle-shaped extension of the cell, accompanied by a certain cloudiness. The capillary processes, as Führer calls the projections of the cells, are often so bent towards each other that a completely annular shape is assumed by them. He does not corroborate **Kölliker**'s statement that such curved cells are imbedded in a cell membrane. On the contrary, he sees in the endeavour of the bodies in question to roll themselves into an isolated state, and in their wavy contour, a special contractility, and an indication of their arterial vascular character. Their ready solubility in acetic acid seems to show that it is not merely elastic membrane which forms the tubuli and cell-walls. According to Führer, these bodies are equally extended over all parts of the spleen, only in the spleen-corpuscles they form a much finer network than in the pulp. Führer thinks he distinguishes younger and older capillary cells, inasmuch as those which show only an extension in the place of a nucleus, having sharp-pointed projections, are most probably less developed. Hence, in a more abundant ramification of the capillary cells, quite the peripheric extensions are in this way much diminished in size. In spleens also, with a presumably quickened blood-formation, that is, in the embryotic spleen, very large numbers of such singly-caudate, non-nucleated, as also disjointed capillary cells, exist. Führer then proceeds to point out the primitive origin of these. The above-described special tubular system Führer considers to be the only capillary vascular system in the spleen, but one in its single parts transient, and ephemeral. All those free granular elements which appear on rupture of the spleen he takes to be only blood and nuclei of the capillary cells, separated and pressed out of the finer vessels with the splenic fluid, in various stages of development. All larger cell elements belong not to the normal spleen parenchyma, but are colourless blood-corpuscles of various kinds, poured along with the blood out of larger vessels. Free granular pigment also exists in each spleen, more or less abundant; but blood corpuscle-holding cells, which Kölliker observed, and which some repudiate, are considered by Führer to be rare.

The veins of the spleen were found by him, as by **Gray**, to possess an enormously wide calibre immediately after their appearance. According to Führer, they arise from the coalescence of those capillary cell networks in which the young blood-corpuscles lie in rows, the more closely the nearer to the entrance into the vein. The arterial ramification, on the contrary, is very fine, gradually passing into the capillary network. Our author looks upon the *Malpighian bodies* as consisting of the finest arterial branches, with a crown of capillary cells, very fine and thickly laid. He finds no veins within them, and the arterial branches are so fine as mostly not to admit red blood-corpuscles; hence their white colour. He failed to obtain indication of the presence of any investing membrane of the Malpighian corpuscle, excepting the sheaths of the bloodvessels; and generally there was no clear limit between the corpuscle and the red spleen-pulp. The above views will be seen to differ materially from those of Kolliker, Gunsburg, and Gray. With regard to the observations of the latter observer, it may be well here to notice one or two points which were not commented on in the notice of them given some time ago in this periodical. Thus we find that by him the *smaller veins of the spleen* are considered to have an important relation not only to the pulp, but also to the Malpighian bodies. Each of these bodies seems, he states, to be enclosed completely by an imperfect capsule, formed of the small primary veins. These vessels, of large size, commence on the surface of each Malpighian body throughout the whole of its circumference, and radiating from the central part, join with similar branches, either on the surface or towards the circumference; lastly, these larger veins empty themselves into the neighbouring ones of the pulp. The smaller veins appear to act in carrying off the secretion formed in the Malpighian bodies into the circulation.[*]

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[*] See Canstatt. [†] Arch. für Phys. Heilk., Heft 2, p. 155. Canstatt, p. 72. 1854.

Mazonn states most clearly that the covering and trabecular tissue of the human spleen contains muscular fibre.[†] This was neither found to be the case by Gray, Gunsburg, nor Kolliker. The latter found it, however, in some of the lower animals.

III. REGENERATION OF PARTS.

Epithelium.—Gerlach[‡] and Hessler[§] assert the existence of free nuclei in the lowest layers of laminated integument. Kölliker, Donders, and Harting assert that the complete cells take a part in the formation of the new ones. Kolliker saw the nuclei in the epithelium cells of the tunica vaginalis to contain two nucleoli, and to divide into two nuclei; and among the epithelium of the urinary bladder the frequent occurrence of two nuclei in one vesicle, which, along with Virchow, he saw, proves to him an endogenous cell production.

Henle, however, thinks that an undoubted instance of endogenous cell-formation in the adult human body is wanting, and is necessary before any value can be placed on such fragmentary and ambiguous facts.

According to Donders,[‖] the cylindrical cells of the mucous layer of epithelium are to be looked upon as the locale for the proper newly-formed cells. He thinks they may be, towards the interior, united with the cutis as with the rounded cells of the mucous layer external to them, and withstand the influence of alkali longer than the last. In this case Donders thinks they may be older than the cells of the more superficial stratum, and probably not destined to move outwards.

[*] Gray on the Structure and Use of Spleen, p. 131.

[†] Müller's Archiv, No. 1, p. 25. 1854.

[‡] Handbuch der Gewebelehre, &c.

[§] Quoted from Canstatt.

[‖] Handleiding voor de Natuurkunde van den gezond. Mensch.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

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1. *On the Nature and Treatment of Encephalo-Meningitis.* By M. Léon Liégard, D.M.P. (Revue Médico-Chirurg. de Paris, Jan. et Févr., 1855.)

Dr. Liégard, whose special attention was drawn to the subject of the above-named memoir by the excessive fatality of the meningitis of children in the Hôpital de l'Enfant Jésus, and by the apparent inefficacy of the various remedies of an opposite character administered, supports M. Robin's opinion, that the granulations met with in these cases are not of a tuberculous character, and that we must therefore materially modify our views with regard to the nature and treatment of the disease. It appears that the medical officers of the hospital had been so discouraged by the inutility of applying either the antiphlogistic, the alterative, or the tonic mode of treatment, that they abandoned therapeutic attempts altogether; and, a diagnosis of meningitis having been established, resigned the patients to their fate. The views advocated by the author are mainly based upon Robin's micrographic account of the granulations, so that it is necessary to give a summary of his description.

Robin assumes two varieties. The first is of a yellowish tint, softish to the touch, and friable. Nine-tenths of these granulations consist of a finely granular amorphous matter; they contain a small quantity of cytoblasts, a few capillaries, and occasionally fibro-plastic formations. The second may, and most frequently does, co-exist with the former in the meninges, and is also found coincidently in other serous membranes. It is the form specially known under the term *grey* or *semi-transparent granulation*. Robin finds that the bulk of the product is composed of the same amorphous granular matter as that above described, but that it is firmer; the cytoblasts are in large numbers, as is particularly manifest on the addition of acetic acid. Fibro-plastic elements, and a network of fine areolar tissue, accompanied by scanty vessels, also present. These cytoblasts are described as differing from the tubercle corpuscles (1) in being polyhedric, with irregular and slightly-toothed margins, instead of oval or round; (2) their diameter averages half the size of the latter; (3) the tubercle corpuscle is rendered pale by acetic acid, but is not dissolved; the contour of the cytoblasts is rendered more defined; (4) the granulations contained in the tubercle corpuscle are scattered uniformly through the corpuscle, while in the cytoblasts they are congregated in the middle; (5) although the cytoblasts contain no nucleolus, they belong to the cellular element which commonly exhibits the presence of free nuclei. We scarcely ever find the variety of free nuclei without meeting with some cells containing a nucleus resembling the free nuclei.

Assuming this definition of M. Robin's to be correct, the author regards the inflammation which determines the formation of the granulations to be of a specific character, analogous to that of croup, and attributes the fatality of the disease in the children's hospital to the erroneous assumption that it was of a tuberculous character. M. Liégard then quotes several cases diagnosed as tubercular or granular meningitis, in which cures were obtained by powerful antiphlogistic remedies, especially by venesection or leeches to the mastoid processes, and mercurial inunctions over the abdomen, repeated every three hours, with calomel administered internally. The author admits the impossibility of diagnosing the granular from the simple form of meningitis, and proposes the classification of the disease, according to the violence of the symptoms and the rapidity of its course, into the inflammatory and sub-inflammatory forms. According to the intensity of symptoms, he recommends a variation of the treatment, not in kind, but in degree; and he concludes, both from the character of the morbid changes and the results obtained on the basis of the views advocated, that this fearful malady may be successfully combated by the means proposed, and ought not to be regarded as necessarily fatal.

2. *A Contribution to the Doctrine of so-called Paralysie Musculaire Progressive.* By Dr. Theodor Valentiner. (Vierteljahrschrift für die Praktische Heilkunde, xii. Jahrg. 1855, p. 1.)

The term paralysie, or atrophic musculaire progressive, was applied by Cruveilhier to cases of fatty degeneration of the muscles, resembling those detailed by Dr. Meryon,[*] in which no essential morbid change could be traced in the nervous centres;[†] but in which the gradual and entire loss of power of certain sets of muscles, accompanied, as Duchenne had pointed out, by loss of electric excitability, constituted the main symptoms, while both naked-eye and microscopic examination demonstrated a complete fatty degeneration of the affected muscles. Dr. Valentiner's case is of considerable interest; we can only give it in a much abridged form. It will be seen that the primary lesion in this instance appears to have been in the spinal cord.

A gentleman, aged 45, of robust and athletic habit, and peculiarly gifted, had always enjoyed good health; and although he had occasionally committed excesses in his cups, was generally temperate, and remarkably fond of gymnastics. Ten years previously he once fell on his back, on a sandy soil, from a height of eight or ten feet. About two years after this, the patient, who had experienced no inconvenience at the time of the fall, thought his health failed; his florid complexion became sallow. A year later, in 1847, he had a slight attack of pleurisy; but as late as 1852, he could have experienced no great diminution of physical strength, for he was still able to carry two fifty-pound weights in each hand. In April, 1853, he first consulted Dr. W. H. Valentiner, who found that he had then very little power in his hands; that he was unable to exert any forcible compression, or stretch out his fingers completely; the right hand was the weaker of the two; no emaciation was perceptible; there was a difficulty about all the movements of the body, and the patient had a difficulty in conveying his food to his mouth; no tenderness or change of any kind was perceptible in the spine; no cerebral affections could be traced. The various physicians consulted, regarded the affection as one of the nervous system, without being able to localize it. The patient was sent to Franzensbad, a Bohemian watering-place, where he used the baths, and then went to Nancy, in the south of France. Dr. T. Valentiner (the author) was now consulted, and having become acquainted with Cruveilhier's cases, diagnosed the present one as an instance of fatty degeneration of the muscles, with atrophy of the anterior roots of the spinal nerves. In the autumn of 1853, though the paralysis became more and more marked, some hopes were excited in the patient by the occasional occurrence of sudden and frequent twitchings in the affected muscles. In November, the patient was conveyed back to his home; he then exhibited extreme emaciation of the upper extremities; none of the limbs could be properly extended; in an attempt at walking, the feet dragged on the ground; his back was almost bent double; the face, though already showing symptoms of paralysis, still retained its intellectual expression; sensibility continued unimpaired in all parts of the body. At last, dysphagia supervened, and although the appetite continued good, the paralysis progressed, and a slight attack of bronchitis, in March, 1854, terminated the patient's misery.

The following are the main results of the post-mortem examination:—The deltoids and other muscles of the upper and fore-arm had almost disappeared; the muscles of the hands were entirely converted into fat; the muscles of the cervical and lumbar region presented a tolerably healthy appearance, but in the dorsal region they were pale, and traversed by yellow bands; and one fasciculus closely resembled fat. The microscope confirmed the various degrees of fatty degeneration observed by the naked eye. The muscles of the lower extremities were in a healthier state than those of the upper. On slitting up the entire dura mater of the spinal cord, about one hundred small white bodies were observed scattered over the dorsal surface of the cord, exclusive of the cervical portion; varying in size from a pin's head to a small pea, smooth externally, rough on the inner side, and grating under the knife; under the microscope they exhibited a dense fibrous tissue, interspersed with pigment. All the anterior roots of the spinal nerves were distinctly thinner and smaller than the posterior roots, flabby, resembling a tissue filled with a reddish serum, and exhibiting under the lens a marked vascularity; healthy nerve-fibres were still visible under the microscope, but many were in a state of fatty degeneration. Nothing of the kind was found in the posterior roots; the cord was found abnormally soft at the part where the three lower cervical and four upper dorsal nerves are given off; at this part the distinction between the grey and white matter was almost effaced; numerous glomeruli (granular corpuscles) were found in the softened part; they occurred in the white, as well as in the grey, substance. Scarcely any well-marked ganglionic cells were discoverable in these parts, but were found in the unsoftened portions lower down; no marked lesion could be discovered in the brain; the heart, healthy to the naked eye, showed under the microscope incipient fatty degeneration; much oily matter was contained in the hepatic cells; the nerves of the brachial plexus exhibited no abnormities; excepting congestion of some parts of the lungs, and slight pleuritic adhesions at the right apex, no other visceral lesion was discovered.

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[*] Medico-Chirurgical Transactions, vol. xxxv p. 73.

[†] In one of three cases detailed by Cruveilhier there was atrophy of the anterior roots of the spinal nerves.

The author of the paper is of opinion, and it would appear that he is fully borne out in the view, that chronic arachnitis, first caused by the fall, was the fundamental lesion in the case just detailed, and that the atrophy of the nerves and the fatty degeneration of the muscles were consecutive. It is curious that, considering the site of the arachnoideal formations, there should have been no lesion of sensibility.

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3. *Three Observations of Adherent Pericardium.* By Professor Cejka. (Vierteljahrschrift für die Praktische Heilkunde, xii. Jahrg. 1855, p. 128.)

In each of the cases related by Professor Cejka the diagnosis of complete adhesion of the two laminæ of the pericardium was established during life, and confirmed by the post-mortem. In each there was adhesion between the pericardium and the costal pleura. The characteristic symptom in each case was the depression of one or more intercostal spaces over the heart at the time of the systole. The dulness over the heart was extended, and the sounds only feebly audible. The cases confirm Skoda's views on the subject, for which we may refer the reader to Dr. Markham's translation, p. 327.

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4. *Remarks on some of the Anatomical Conditions that favour the Transmission of Sounds from the Root of the Bronchi to a distant Part of the Thorax.* By E. Barthez, M.D. (L'Union Médicale, June 7th, 1855.)

A considerable difficulty presents itself at times to the auscultator in the interpretation of what Skoda has termed consonating sounds. Dr. Barthez evidently adheres to the theory of the direct conduction by the tissues intervening between the part at which the sound is produced, whether a respiratory murmur or a bubbling or other noise, and the point to which the observer's ear is applied. The following case affords a striking illustration of the physical circumstances attending the production of caverno-amphoric respiration in pleural effusions, and appears to us to be more readily explicable by the theory of consonance than of conduction.

Dr. Barthez was requested by M. Marjolin to see a child, aged 5 or 6 years, affected with a purulent effusion into the pleura, which had opened externally near the nipple by sinuses, which were so extensive as to prevent the air from entering the thoracic cavity. The discharge of pus having been arrested for some days by the dressing, the presence of a certain quantity of liquid in the pleura was proved by general dulness, and bronchial respiration throughout the (upper part of the) thorax posteriorly. Under the clavicle the respiration was manifestly cavernous. On the introduction of a probe into the thorax, carried along the sinus, a continuous jet of pus escaped. While this occurred, the bronchial respiration disappeared gradually at the pulmonary apex posteriorly, to make way for a feeble but normal respiratory murmur in and below the fossa infra-spinosa. Immediately after the evacuation of the pus, and while Dr. Barthez applied his ear to the thorax, tepid water was injected at intervals into the pleura. The injection was effected without the production of any sound, and the thorax was filled without any perceptible phenomena, until at a given moment the bronchial respiration of the infra-spinous fossa assumed the cavernous and even amphoric pitch, which several of the gentlemen present were able to confirm. Afterwards, in proportion as the injected fluid was evacuated, the amphoric respiration disappeared in order to make room for a bronchial blowing, similar to that previously existing. Finally, a powerful inspiration having caused the admission of some air, metallic tinkling became very evident, which was repeated each time a fresh injection was made. From that time, in spite of the injections, the amphoric breathing was not reproduced. The child subsequently recovered, and the normal respiratory murmur was restored on the affected side.

The last circumstance adverted to, the impossibility of reproducing the amphoric sound after the admission of air, is strongly corroborative of the law of acoustics, that the consonance of sound is dependent upon the uniformity of the media through which it is transmitted.

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5. *Investigations regarding the Formation of Cavities in Tuberculous Lungs.* By Dr. Hugo Ruhle. (Inaugural Dissertation, Breslau, 1853.)

The prevailing opinion with regard to the mode in which cavities form in tuberculous lungs is, that after the deposition of the morbid product, secondary ulcerative destruction takes place, by which a breaking up of the pulmonary tissue is effected. Dr. Rühle analyses ten cases of phthisis pulmonalis, in which cavities were found, and concludes that they take their origin in dilatation of the bronchi; and that the ulcerative fusion of the parenchyma surrounding a tubercular deposit is, in the majority of instances, preceded by bronchiectasis. He finds that in proportion as the cavities diminish in size, the more unable we are to discover any limits between the mucous membrane of the bronchus leading into a cavity, and the membrane lining that cavity. The author is of opinion that the microscopic appearances of the membrane are not compatible with the view of its adventitious character. Moreover, the relation of the bronchus to the cavity is regarded as corroborative of Dr. Rühle's doctrine:—"The cavities are always in direct communication with the bronchi, and only one bronchus opens into each of the cavities here alluded to, and the communication is not on one side, but the axis of the bronchus coincides with that of the cavity." The author does not inform us at what time the ulcerative process commences, but states generally that it ensues early, and that, although non-tubercular bronchiectasis may be accompanied by ulceration, the tubercular deposit possesses a peculiar power of exciting the ulcerative process in dilated bronchi.

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6. *Reduplication of both Sounds of the Heart.* By Austin Flint, M.D. (The Western Journal of Medicine and Surgery, April, 1855. Louisville, Kentucky.)

The occurrence of a reduplication of one of the heart's sounds, especially of the first, though not very frequent, is one that most clinical observers have met with. A reduplication of both sounds is so rare, that the following case, which may be regarded as almost unique in the regularity of the phenomena and the completeness of the observation, deserves especial attention.

George Nash, aged 27, boatman, admitted into Louisville Hospital, December 16th, 1853. Excepting an attack of cholera eighteen months previously, he had enjoyed good health till seized with cold and cough six weeks before admission. Cough chiefly at night; soreness under sternum; no hæmoptysis, chills, or loss of appetite; bowels regular; never had rheumatism or acute thoracic affection. On admission: aspect not morbid, pulse normal, respiration slightly increased in frequency, skin and tongue normal, appetite good, bowels regular, no thirst, percussion of thorax clear, breathing movements equal on both sides, sibilant inspiration on both sides. Some dyspnœa for a few days, with palpitation from commencement of attack, requiring him to lie with his head raised. The treatment consisted at first in the administration of syrups of ipecacuanha, squills, and tolu, with a little sulphate of morphia. On December 23rd, seven days after admission, the pulse at the wrist was found too quick to be counted, "but the carotids can be felt and, enumerating in this situation, counting at the same time the heart-sounds, the number of ventricular contractions per minute is one hundred and sixty." No œdema or ascites. Chest well developed. No apex impulse of heart; a feeble diffused impulse felt just below the nipple, and both seen and felt at the epigastrium; no heaving of the chest; the heart-sounds succeed each other so rapidly that it is difficult to state them, but they appear pure, and the first sound is shorter than natural. On December 24th, the absence of pulsation in the jugular veins is noted. On December 28th, Dr. Flint says: "On examination of this patient yesterday, I found a feeble vibratory pulse at the wrist, numbering eighty per minute. I counted it repeatedly, with similar results; on counting the heart-sounds, I found them apparently one hundred and sixty-six beats—i.e., tic-tacs, per minute. I repeated the comparison of the heart-sounds several times, with the same results. To-day I find the same contrast—viz., pulse eighty, and two heart-sounds repeated one hundred and sixty times per minute. Dr. Dickinson counted, without knowing the results of my enumerations, with similar results." The same ratio was observed by Dr. Hardin, on December 31st; by Dr. John Clark, on January 1st, and also on January 3rd and 13th. On the 3rd of January, a faint, short, sharp bruit was noted below the pectoral muscle, with the systole at the right and left apex, but not at the base. On the 13th, a bruit, supposed to be endocardial, accompanied the systole, uniformly higher in pitch than the whispered word *who*, and nearly as high as *R*. On January 25th, œdema supervened, and the dyspnœa became more urgent. The bitartrate of potash, followed by the exhibition of Epsom salts, relieved the dropsy; and on February 14th the patient was discharged, feeling quite well; able to sleep in the recumbent posture, without dyspnœa on taking exercise; no cough, pain, or palpitation. "*The pulse and the two sounds of the heart are eighty-four per minute.* Marked dulness on percussion was observed to the left of the nipple. No point of apex impulse was seen or felt, but a very feeble impulse is appreciable over an area about two inches in diameter. No bruit heard." He left the hospital, and undertook severe work, without experiencing any inconvenience; and when Dr. Flint saw him again, on April 12th, the pulse at the wrist, and the ventricular contractions, were eighty-four each per minute; the sounds of the heart were pure, no apex impulse was appreciable, the dulness on percussion extended an inch to the left of the nipple, and the general health was good. He was again seen by Dr. Flint in February, 1855, when he continued well, and no physical evidences of heart disease were found, save that the area of dulness was somewhat increased.

In considering the causes of the phenomena in this case, Dr. Flint suggests two theories—either that the heart beat in the usual manner, but that, from the weakness of every alternate beat, that was not represented by a corresponding dilatation of the radial artery; or that the reduplication was owing to a want of synchronism between the contraction of each ventricle; each ventricle, as it were, asserting its independence by separate action. Want of synchronism between the ventricles is the cause to which Dr. Williams and Skoda attribute the reduplication of the first sound alone, and it is difficult to assume any other view in reference to Dr. Flint's case; he himself does not express himself decidedly in favour of one or the other, although he inclines to the theory of synchronism. He remarks, that a fact which would be incompatible with it was, that the beats in the carotid artery were equal to that of the heart. "It is indeed wonderful," he says, "that of the halved portions of the systolic contraction of the left ventricle, only one should regularly be accompanied by a radial pulse, but it is, perhaps, quite as difficult to conceive that, in view of the arrangement of the muscular fibres of the heart, the ventricles should contract separately." The only point that suggests itself is as to the possibility of the peculiarity of the heart's action, whether explicable on the one view or the other, having been determined in the first instance by the remedies employed, and especially by the sulphate of morphia.

7. *Elephantiasis Orientalis.* By Allan Webb, M.D., Bengal Medical Service. (The Indian Annals of Medical Science, No. 4, April, 1855.)

The main object of this valuable communication is to determine the nature of elephantiasis by post-mortem examinations, for which the opportunities are not often offered. Dr. Webb states that he has for ten years operated upon elephantoid tumours, without once having an opportunity of examining after death the state of the internal organs in cases of elephantiasis genitalis. The conclusions that he arrives at from the post-mortems which he has made (and of which we shall give a summary), as well as from observation of the vital relations of the disease, are, that it is, in truth, a blood-disease; that it affects the limbs, the subcutaneous cellular tissue, the sheath of the nerves; that it is metastatic, and effects organic changes in the heart, liver, spleen, kidneys, testes, and probably the brain. Dr. Webb sees an analogy between elephantiasis and rheumatism, on account of the first-mentioned features. In regard to the microscopic characters of elephantiasis, Dr. Webb agrees with Mr. Paget in describing it as essentially consisting of a fibrous outgrowth from an albuminous blastema, mingled with elastic tissue, and more or less fat. Professor Macnamara has found smooth unstriped muscular fibres in the tumours that result from elephantiasis of the scrotum, to the presence of which the author attributes the contractions which he has repeatedly seen ensue in the tumours when pricked. Dr. Webb absolutely denies that the disease is ever the result of an inflammation of the veins or lymphatics; at the same time, he regards all the varieties which are met with as essentially the same disease, the main difference appearing to consist in the rapidity with which it is developed, and with which it runs its course. While our author denies that the disease originates in inflammation of the veins or lymphatics, he appears to agree with Dr. Skinner that it has its origin in inflammation of the cellular tissue, into whose stretched and enlarged areolæ a fluid is poured out capable of speedy organization. We do not find that the limitation to the left side, observed by Dr. Rigler,* is confirmed by Dr. Webb; he does not allude to the point, but in the cases which he gives, the right side appears to have been most frequently attacked: in one, the left leg was first attacked, then the right arm, and lastly the scrotum; nor does there seem to be any definite relation between an affection of the lymphatic glands and the occurrence of elephantiasis, as suggested by Dr. Rigler.

The following is a brief summary of the post-mortem appearances found in the necropsies of cases of elephantiasis: from them it would follow, as Dr. Webb points out, that fatty degenerations of various organs are concomitant affections with elephantiasis. Whether they are merely a secondary result of the change of nutrition produced by the drain upon the constitution by the morbid deposit in the scrotum, or extremity, or whether the disorganizations of the viscera are primary affections, must be determined by further investigations.

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* See British and Foreign Medico-Chirurgical Review, July, 1855, p. 246.

Case I.—Man, aged 30. Elephantiasis scroti; duration two years; death from sloughing of scrotum. Fatty degeneration of heart; liver fatty, with old adhesions of right lobe; spleen four times its natural size; kidneys three times their normal size, internally softened and yellow, with a few cysts.

Case II.—Man, aged 30. Elephantiasis of right leg; duration about four years. Amputation; death. Universal effusion between arachnoid and pia mater; liver and heart showed fatty degeneration under microscope.

Case III.—Man, aged 50 Elephantiasis scroti about ten years; of right leg about two years. Death from suppuration and sloughing of leg. Fatty degeneration of heart; thick flakes of fibrin on right auricle; atheroma in descending aorta, and absolute obliteration of aorta about one inch below the cœliac artery; liver cirrhosed, weighing two and a half pounds; spleen softened, three times its normal size; kidneys encysted, and with fatty degeneration.

Case IV.—Man, aged 40. Elephantiasis of scrotum and right leg and foot, preceded by swelling of inguinal glands of same side; duration fifteen years; removal of scrotal tumour; death from diarrhœa, dysentery, and sloughing. Liver pale, spotted with yellow patches, apparently from fatty degeneration; spleen double the normal size, with thickening of capsule. Fatty degeneration of epithelium of kidneys; gangrenous mass in posterior part of right lung; right side of heart of pale brown, marked fatty degeneration throughout; aorta covered with atheromatous deposit.

Case V.—Man, aged 40. Elephantiasis scroti; duration seven years; operation; death three weeks after, with fever and delirium; liver cirrhosed, the colour of the pancreas; inferior cava distended immediately under liver, completely obliterated close to the heart; capsule of right kidney adherent; cysts in kidneys; left kidney enlarged and pale; spleen much enlarged; apparent fatty degeneration of heart; considerable serous effusion upon brain and in ventricles; brain tissue hardened.

Case VI.—Elephantiasis scroti; duration eight years; right side of scrotum first affected. Operation; death. Lungs œdematous; heart fibres had in places almost entirely disappeared, and fat substituted within them; kidneys, and liver, and testes showed fatty degeneration.

Case VII.—Man, aged 40. Elephantiasis of penis and scrotum; duration seven years; operation; jaundice; delirium; death. Effusion of serum, blistering the arachnoid, and causing compression of medulla to the size of a little finger; spots of apoplexy in lungs; heart healthy; liver much enlarged and hardened; covered with granulations, like grains of sand; pancreas and spleen enlarged; kidneys healthy.

Case VIII.—Man, aged 37. Elephantiasis of scrotum two years; death from sloughing. Heart pale and flabby, covered with white deposit; liver enlarged, contained a large abscess; spleen exceedingly pale and shrunk. With reference to the employment of the term "fatty degeneration" in the above, we have to remark that the microscope was evidently employed judiciously, and the characteristics determined by its use.

Dr. Webb observes that, while the Bengalese are especially obnoxious to the complaint, he has met with it in Europeans, East Indians, Armenians, Jews, and Portuguese. Nor does any sex or age afford immunity. Dr. Webb relates the case of a boy, aged one year, who had elephantiasis of the scrotum and penis, and in whom it commenced when he was five months old; of another, aged 14, who suffered under the complaint; and of a girl, aged 17, who had an elephantoid tumour of the right labium, about twice the size of her head.

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8. *On Variola, especially with Reference to its Occurrence and Relations in Berlin during the Twenty Years,* 1834—1853. By Dr. Quincke. (Annalen des Charité Krankenhauses, Sechster Jahrgang, Heft 1.)

In an analysis of the statistics of small-pox, the author touches upon various points of interest. With reference to re-vaccination, he concludes that it becomes necessary after the sixteenth year, as he finds that the susceptibility to small-pox increases in persons vaccinated in the first year of life after puberty, in the ratio of advancing age. In confirmation of the correction of this conclusion, he adduces the results obtained in the Russian army. During the years 1834 to 1853, 811,402 military men were re-vaccinated; of these, 457,581 exhibited the genuine vaccine pustule; the total number of those attacked with variolous disease was 421, of which 217 had varicella, 191 varioloid disease, and 13 genuine small-pox. Four deaths from small-pox occurred among the 457,581, who had been re-vaccinated with success; while 25 died of small-pox among the 353,821 who had been re-vaccinated without any result. The reader will gather from what precedes, that Dr. Quincke regards variola, varioloid disease, and varicella as modifications of the same disease, determined by the constitution of the individual, the genius epidemicus, and other circumstances extrinsic to the disease itself. He states that there is no anatomical difference between the eruptions of these varieties, and gives instances which came under his own observation, in which several members of the same family were successively seized with varicella, variola, and varioloid disease about the same time. He regards the various forms of the local affection resulting from more or less perfect vaccination, as evidence in favour of the view of the identity of the three eruptive diseases alluded to.

9. *On the Communication of Syphilis by Vaccine-Lymph.* (Bullet. Génér. de Therap., July 15th and 30th, 1855.)

THE question of the influence of syphilis upon vaccination is one of importance; the opinion is very wide-spread among the laity, that vaccine lymph taken from an unhealthy child generates disease. A case in point has lately occurred in Bamberg, a town of Bavaria, where a medical man was condemned to two years' imprisonment for having vaccinated several children from a child exhibiting a syphilitic eruption on its face and body. The witnesses asserted that the vaccine pustules had not been properly developed, and were followed by tedious ulcerations. Moreover, nine grown-up persons were asserted to be re-infected by the children tainted through the vaccine pustule. The judgment was commuted in consequence of the opinions expressed by Messrs. Heyfelder and Pauli, two distinguished medical men of Rhenish Bavaria, whose judgment has been supported by that of Ricord and Cullerier, who utterly deny the possibility of communication of the syphilitic poison by the agency of vaccine-lymph. Cullerier, according to the 'Bulletin de Thérapeutique,' from which we extract these facts, states that he has not only vaccinated syphilitic children without ever seeing the vaccinia in any way modified by the syphilitic diathesis, but that he has vaccinated healthy children from syphilitic infants without ever perceiving the slightest unpleasant results. The Société de Chirurgie, through their reporter, M. Brocas, have pronounced absolutely in favour of the views of Messrs. Ricord and Cullerier.

10. *Notice of a Tonic Contraction occurring epidemically in Subjects affected with Typhoid Fever.* By Dr. F. A. ARAN, Physician to the Hospital St. Antoine, &c. (L'Union Médicale, July 19th, 1855).

IN the recent epidemic of typhoid fever, Dr. Aran has met with twelve cases in his service since last January, in which, at an advanced stage of the disease, when convalescence was beginning, the contractions showed themselves. With the exception of a single case, in which the appearances of the contraction coincided with the commencement of the disease, the contractions occurred after the sixteenth day of the fever. In none of the patients did the course of the fever appear in any way influenced by the new symptom, whether the former progressed favourably or otherwise. The attacks were sometimes preceded by formication, pricking, numbness of the extremities, and pain in the chief joints. The immediate seizure was announced by feelings of great anxiety and distress—it occurred at all times of the day. The contractions affected the upper and lower extremities, and especially the former; the two corresponding extremities were generally seized at once, though the arms presented occasional exceptions from this rule, by being attacked singly. The flexor muscles, which were the seat of the affection, exhibited almost incessant fibrillar contractions. When the entire upper extremity was attacked, the fore-arm was bent upon the upper arm, and lay upon the thorax, while the fingers were doubled up on the palm of the hand; the patient was unable to extend the limb, but by gradual manipulation an artificial extension could be obtained, and this gave the patient relief, though the parts resumed their morbid position when left to themselves. When the lower extremities were attacked, the contractions were less general, the leg was extended, the calves hard and painful, and the toes flexed and bent back. In four cases the muscles of the trunk were affected, producing opisthotonos, during which the patient was unable to execute any movement.

During the seizures, the contracted muscles were the seat of intense pain. The attacks lasted from a quarter of an hour to two or three hours, and recurred from two to ten times daily for several days, more or less. After the cessation of the attacks, the disease ran its ordinary course, without any residuary affection, beyond an occasional numbness of the affected parts. Three of the patients died, but this result was attributable to the severity of the fever, and not to the tetanic affection.

Dr. Aran is at a loss to account for the phenomenon which, according to the inquiries he instituted, appears to have been peculiar to his patients. He was inclined at first to set it down to the mercurial treatment which he habitually employs, but this conclusion is negatived by the fact, that of the twelve patients affected, five took no mercury The employment of baths was regarded as having benefited the patients, and in one severe case of opisthotonos, chloroform inhalations gave relief.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

Communications from the Syphilitic Section and Clinic of the Charité at Berlin. By Dr. Baerensprung.—Under this title, Dr. Baerensprung makes several interesting observations upon various points relating to syphilitic diseases, some of which we here extract.

Chancre of the Urethra.—After giving the particulars of the six cases he has met with during the year, he thus sums up:—1. Chancre of the urethra sometimes occurs as the only affection, while, in other cases, chancres co-exist elsewhere. 2. It may be easily overlooked, and then the bubo or secondary symptoms which result may be supposed to be produced independently of primary symptoms. 3. It may lead to perforation of the canal, and consecutive infiltration of urine and urinary abscess. 4. It is most easily produced in persons having wide urethræ. 5. It is most usually situated at the anterior part of the urethra. In only one of the six cases was it as low down as two inches. 6. Stricture may result from its cicatrization. 7. The chancre is always accompanied by discharge from the urethra; but if gonorrhœa does not exist, this is always sparing, notwithstanding the assertions of Vidal and others, who maintain that the contact of syphilitic virus with the mucous membrane of the urethra induces violent gonorrhœa. 8. The presence of a flocculent discharge from the urethra is well calculated to excite suspicion of the existence of the chancre, but does not prove it. In ordinary gonorrhœa, flocculi are not infrequently observed, consisting of emballed epithelial and pus cells. It is only when the microscope exhibits among these flocculi destroyed corium-fibres, that we can conclude as to the presence of an ulcer. 9. The mixture of blood with the urethral secretions was observed in all the author's cases; but as this may be also present in gonorrhœa, it is not decisive. Bleeding, however, only occurs in gonorrhœa when there is severe inflammation or painful erections, and then it may be very abundant. Small discharges of blood, that are repeated daily, without any cause, and are intimately mingled with the secretion, should always lead to the suspicion of the existence of a chancre. 10. Induration in the course of the urethra is no certain sign; for while it is only found in the indurated form of chancre, we may sometimes, in simple gonorrhœa, feel hard knotty points, due to local exudations into the corpus cavernosum. 11. Œdematous tumefaction of the lips of the orifice of the urethra, which existed in all the author's cases, also occurs in the inflammatory stage of gonorrhœa. In urethral chancre, however, it continues longer, and even when no inflammatory symptoms exist. It is therefore a sign well calculated, in conjunction with the other symptoms, to excite our attention. 12. The positive result of an inoculation affords a certain proof of the existence of urethral chancre; but a negative result is no proof of its absence. In one of the author's cases, in which no result followed inoculation, an autopsy exhibited a chancre; and in another, in which the diagnosis had been derived from the microscope, constitutional syphilis occurred some time after to confirm it.

Dr. Baerensprung indeed believes that the microscope suffices for the establishment of diagnosis. It may be objected, that gonorrhœal ulcers may determine a destruction of the mucous membrane also. This, from analogy and direct observation, he denies. In all cases in which he has submitted gonorrhœal discharge to the microscope, he has only found pus and epithelial cells, and never connective or elastic tissue; while daily experience teaches us, that although blennorrhœa very frequently induces superficial erosions of mucous membranes, as in the prepuce and vagina, it never gives rise to deeper ulceration penetrating the corion.

Abscess of Bartholin's Glands.—This is one of the varieties of abscess of the labia so often met with in prostitutes. Gonorrhœal inflammation is propagated along the excretory duct of the gland, which, in place of its normal, scanty, clear, and filiform mucus, now secretes a considerable quantity of purulent fluid, its vesicles having become dilated, and the entire gland converted into a cyst secreting pus. In some cases, we may press out through the duct as much as a watch-glassfull of thin filiform fluid, which, besides pus-corpuscles, contains pavement epithelium. Huguier terms this parenchymatous abscess, distinguishing it from multilocular, which has its site in the interlobular cellular tissue. The latter is of much more frequent occurrence, and rapid development, and may attain the size of an egg. It cannot be emptied by its duct; and if not opened, it breaks, and discharges a thicker pus, with which mortified tissue is mingled. After it has been emptied, the hard and enlarged gland may be felt at the bottom, and if laid bare by further incision of the walls of the abscess, appears as a blood-red granular body. Not infrequently short, wide-mouthed fistulæ remain; and in other cases, the glandular texture seems to be destroyed, inasmuch as acinose structure comes away with the mortified tissue. In one case, the place of the gland was entirely occupied by cicatricial tissue.

Among several hundred cases of blennorrhœa in women, the author has met with this affection 47 times in the year. In 30 of these, the blennorrhœa was confined to the duct, while in 17 the gland had suppurated. Although, as a general rule, the blennorrhœa of Bartholin's gland ceases at the same time as that of the vagina, in other cases it is very obstinate, and continues long after the other parts of the mucous membrane have resumed their normal secretions. Such cases require especial attention, for the blennorrhœa here will infect just as readily as that of the vagina. Solution of nitrate of silver, introduced into the duct by Anel's syringe, or touching the orifice with it in substance, hastens the cure. If an abscess forms, it must be opened, and its cavity touched with the nitrate. The same means usually suffices for the healing the funnel-shaped fistula that sometimes remains.

The author has several times observed condylomata developed exactly in the orifice of the duct, and this is also a frequent site of chancre. It has happened, also, that the inner wall of a glandular abscess has become converted by chancrous poisoning into a large open sore.

Syphilitic Affections of the Rectum: (1) *Abscess.*—A much more serious affection than the abscess of the vulva above mentioned, are abscesses in the vicinity of the anus, between the vagina and rectum, on account of their tendency to produce perforation on one or both sides, and thus give rise to fistulæ. The existence of such in phthisical and hæmorrhoidal subjects is well known; but too little attention has been paid to the especial frequency of their occurrence in syphilitic women. In the course of a year, Dr. Baerensprung has met with 16 cases either of such abscesses or the resulting fistulæ. The cause has always been a syphilitic ulcer, which in 7 of the cases was seated at the posterior commissure, and in most assumed a phagadænic character. The author at first believed the starting point of the ulcer was a lymphatic gland; but he can discover no lymphatic glands between the rectum and vagina, but only connective tissue abundantly supplied with veins, and on that account especially disposed to inflammation. Sometimes these abscesses acquire a large size, and are extensively diffused around the anus. The explanation of the difficulty of healing such abscesses is usually sought for in the contractile action of the perineal muscles, and especially of the sphincter ani. The author has tried the effect of a subcutaneous section of the sphincter; but, although the action of the sphincter was completely destroyed immediately after the operation, it was recovered again in a few days. A repetition of the section in nowise facilitated the closure of the fistula. Another probable reason for the difficulty of healing such abscesses may be found in the constant transudation of the intestinal gases that takes place, communicating to the pus the fæcal odour, even when no communication with the rectum exists. Moreover, the pus is always mingled with a large quantity of coagulated blood proceeding from the abundant veins of the connective tissue. This was observed even in two cases which reached only a small size, and were opened early. The most important rule in treating them is to open them early, in order to prevent their bursting into the rectum. Sometimes, however, they are found so high up, that this can be done only with difficulty, if at all.

(2) *Fistulæ.*—From abscesses of this kind originate fistulæ. Of these 12 have occurred in the same space of time—3 incomplete, 5 complete, and 4 recto-vaginal. In the first of these, very careful and repeated examination failed to detect any internal opening. Ribes states that he never failed in 75 cases to find this opening immediately above the outer sphincter, and always within six lines of the anus; but the author's experience, both in syphilitic and non-syphilitic cases, leads him to conclude that fistulæ occurring after abscess are at first incomplete, and only afterwards open into the rectum; while, moreover, the inner opening, when it does exist, is often higher—viz., in the vicinity of the internal sphincter.

(3) *Stricture.*—Stricture of the rectum often occurs in syphilitic patients, especially female ones; but they usually only come under notice after having long existed. In two cases given by the author, the formation of circular strictures had been preceded by fistulæ. In one of these, after performing incision without avail, he accomplished dilatation with bougies; and in the other, after forcing the stricture with the fore-finger, he also succeeded in dilating it by the long-continued use of bougies. In these cases, the altered mucous membrane was alone implicated, the stricture evidently originating in cicatrization of ulcers of that membrane. In other cases, however, the tissues beneath the mucous membrane are concerned, and a larger portion of the walls of the intestine may become thickened and knobbed; such cases often admitting of only palliative treatment. Most practitioners believe that these syphilitic strictures are the sequence of primary affections of the rectum, due to unnatural copulation; and the author has met

with such cases; but he believes they may also be produced by constitutional syphilis, leading to the production of condylomata and ulcerations of the mucous membrane.—*Annalen des Charité-Krankenhauses*, Jahr. vi. pp. 1—56.

On the Utility of Decoction of Rhatany in Keratitis. By Dr. A. QUADRI.—Dr. Quadri observes, that of all the inflammations of the eye, keratitis is one of the most frequent and most obstinate. Experience has proscribed the employment of mineral astringents. Among those of the vegetable kingdom the laudanum formed by the combination of crocus and opium sometimes produces excellent effects; but in scrofulous ophthalmia, which is frequently but a keratitis, it occasionally gives rise to prolonged and mischievous irritation. The author had tried various other substances, as tannin, calumba, &c., without any definite results, when he resorted to rhatany. The experience of six years has convinced him of its value. Its application merely induces a sensation of dryness in the interior of the eye, and in a short time the pain and photophobia are mitigated, and the weeping is much diminished. When the irritation has thus become calmed in two or three days, the rhatany may be replaced by the more powerful laudanum, more or less diluted. The rhatany is insufficient in the corneitis accompanying blennorrhœal ophthalmia, but in scrofulous and all other forms of keratitis its efficacy is constant. It is prepared by boiling half an ounce of the root in twelve ounces of water, or decoction of elder-flowers, down to half the quantity, and filtering. It should be freshly prepared, and may be used three or four times a day.—*Annales d'Oculistique*, tome xxxiii. 87.

On Wounds of the Heart. By Dr. PURPLE.—No opinion is more generally prevalent than that wounds of the heart are necessarily, and in most cases immediately, fatal. In order to test its correctness, Dr. Purple has tabulated the particulars of forty-two published cases. Among the observations made upon these are the following:

1. Wounds of the heart are, in general, not immediately fatal. The fact of the continuance of the functions of the organ, in spite of severe wounds and the presence of foreign bodies, is sometimes astounding. In a case recorded by Dr. Randall, three shots were found in the right ventricle, and two in the right auricle, the wounds having completely cicatrized. The boy (aged 15) lived sixty-seven days, and died apparently of pneumonia. In a case related by Dr. Constantin, a lunatic lived four days after severely wounding the right auricle and ventricle, transfixing the organ with an ebony paper-scraper. Two remarkable cases are referred to, in which foreign bodies had been long lodged in the heart, no wound of the viscus being discoverable. A soldier died ten weeks after receiving a wound near the left axilla. The ball was found firmly bound down by the *columnæ carneæ* of the left ventricle. No cicatrix was discoverable, and it was supposed that the ball had perforated the pulmonary vein, and passed thence into the left auricle. In the other case, a piece of stick, three inches and a half long, and half an inch in diameter, was found in the cavity of the right ventricle. The patient lived thirty-seven days, and it is not known how the foreign body found its way into the cavity. Another curious circumstance in these cases is observed when the ball penetrates the thorax, and perforates one or both ventricles, without producing any corresponding wound of the pericardium.

2. The cause of delay of the fatal termination is a question of deep interest to both the surgeon and the medical jurist. Besides the circumstances which must obviously modify the result, such as the size, direction, and complications of the wound, the condition of the patient's health, and the treatment he is subjected to, Dr. Purple adverts to another. "The study of these cases has led us to believe that the concussion or shock of the organ has more to do with sudden death than any other one cause. A careful examination of all cases of injury of the heart, shows that the primary symptoms are combined in the presence of shock; the suspension of the movements of the organ, the cold, clammy sweat, the death-like

pallor, syncope, and suspension of consciousness, all spring directly from the shock, and are present in a large majority of gun-shot, incised, and the larger punctured wounds. In those cases in which the patient survives the shock, and reaction supervenes, death occurs from other causes, being mainly due to the occurrence of inflammation in some one or other of its forms, consequent upon the injury done to the pericardium or lungs."

3. In respect to *prognosis* the author agrees with Mr. Baird, that these wounds, although always dangerous, are not all necessarily mortal, and that in some cases hopes of recovery may be entertained, especially if the wound has not penetrated deeply, or if the symptoms of consecutive inflammation are of a moderate character. Instances are recorded where the patient has lived a number of years after an incised wound, or even with a ball imbedded in the ventricle. The comparative mortality of heart-wounds shows that the average duration of life is greater if the left ventricle be the seat of injury; the right ventricle being, however, the part most frequently injured.

Dr. Purple concludes his paper with some observations upon the forensic relations of the subject, showing that a wound of the heart cannot be considered to have been necessarily fatal, when the patient has died from the inefficient treatment of the consecutive complications.—*New York Journal of Medicine*, vol. xiv. pp. 411-34.

On Extirpation of Fistula in Ano. By M. Richard.—In this communication M. Richard has in view only simple cases of fistula, which constitute four-fifths of those met with. Whether an abscess has preceded or not—and its absence is far more common than usually supposed—a fistulous track extends from some part of the circumference of the anus into the rectum, just above the sphincter. The track is nearly straight, without any diverticulum, is usually very short, and easy of examination, providing this be done more gently than is often the case. The symptoms are much the same in all. There is little or no pain in defæcation, unless there has been great constipation, accompanied by painful spasm of the sphincter; and then, as in true fissure of the anus, it is only some minutes after the passage of the stools that the pain is felt. But there is a constant irritation, which is increased by walking, watching, long sitting, and stimulating food. With this is joined itching, from eczema of the orifice. Baths, rest, regimen, touching the anus and the track with concentrated solution of nitrate of silver or tincture of iodine, assuage or remove these annoying symptoms. The linen is stained by two very different fluids—one colourless, sticky, and staining like semen-spots, is furnished by the eczema or intertrigo; and the other, consisting of prominent, scaly, yellow spots, is true pus coming from the fistula itself. The patient need not be asked whether the linen is stained with fœcal matters, as in these cases such never traverse the fistula; but we may learn from him that wind escapes involuntarily from the fundament.

Slight as the suffering from this infirmity may seem, an operation should always be advised; for the reported cases of cure are of doubtful authenticity. Iodine and other injections do not succeed in these simple cases, it being a fact which experience alone could have taught, that multiple, long, and sinuous fistulæ are those in which these prove efficacious. The operation M. Richard prefers is that of *extirpation of the track of the fistula.* He has had recourse to it about thirty times, and has always found it innocuous and effective. A grooved director is introduced into the track, the length of which varies from one to two centimetres, and its end is brought out at the anus. This is done very gently, in order to avoid causing pain or laceration of the track, which would prevent the success of the operation. One assistant draws up the buttock forcibly, and another keeps the integuments around the anus as much on the stretch as possible. When the walls of the track are firm, and the folds of the anus well effaced, we may shave along the lower part of the director at a single rapid stroke; but this is a doubtful procedure, for the bistoury, having its back applied to the anal lip of the sound side, and its edge pressing the internal orifice of the track, may find the latter so soft and depressible as to recede without being cut, and the tissues becoming lacerated during the effort, only an incomplete extirpation results. It is better to plunge the point of the bistoury under the probe to the middle of the track, and having thus fixed the tissues, detach the inner half of the fistulous canal by two or three rapid but well-combined movements, so as to sacrifice as little as possible. Then the external half may be extirpated at one single stroke. Sometimes, when the parts are not kept tense enough, a tenaculum is to be plunged under the track, and the extirpation is practised at leisure by cutting from without inwards. In short, the surgeon must extirpate the canal with as little loss as possible, consistent with the complete removal of the pyogenic membrane.

It may be asked, what are the advantages of this operation, which is more difficult to perform and to bear, and leaves a larger wound than that of incision? The advantages are summed up in a word. We are enabled to dispense with tents and all dressings. Tepid fomentations night and morning, and attention to cleanliness, are all that are required. Two hours after the operation all pain has ceased, and confinement to bed continues only for from twenty-four to forty-eight hours. No accidents or ill consequences result, the wound, in contact with the intestine at one end, and closed in by the margin of the anus at the other, seeming to enjoy all the immunity of subcutaneous wounds. Complete cicatrization may, however, be delayed for even several months, without either patient or surgeon having cause for discouragement.—*Bulletin de Thérapeutique*, tome xlviii. pp. 537-61.

On the Employment of Hæmospasia in Affections of the Eye. By M. Warlomont.—During a recent visit to England, for the purpose of witnessing the present condition of ophthalmic surgery here, and his report upon which is for the most part very favourable, M. Warlomont observes he found no use made of hæmospasia in combating the congestion so frequently witnessed after the operation for cataract. In Belgium, this powerful revulsive means was introduced into ophthalmic practice by M. Stiévenart, of Mons, and is now constantly employed in his eye-hospital. Whenever a person who has been operated upon manifests the least tendency to reaction, Junod's exhausting-boot is immediately applied. It is renewed two or three times daily, and sometimes oftener, frequently inducing syncope, and avoiding thus the loss of blood, which it is often of importance to prevent, especially in the aged. M. Warlomont, who is in the daily habit of employing this apparatus, also strongly recommends it. Its application is very simple, and its employment most valuable, not only under this particular circumstance, but under a great number of others, in which powerful revulsion is indicated, as, for example, in congestive amaurosis.—*Annales d'Oculistique*, tome xxxiv. p. 12.

On Subcutaneous Wounds. By M. Bouvier.—M. Bouvier, after reviewing the condition of our knowledge with respect to subcutaneous wounds, lays down the following propositions: 1. Subcutaneous, like all other descriptions of wounds, are the source of traumatic irritation, and induce a local inflammation, with which is connected the mechanism of their cure. 2. They are especially characterized by the slight degree of inflammation and irritation they occasion, and the immediate cicatrization, unpreceded by suppuration, which is the consequence. Hence the advantages which different surgeons have, since 1816, recognised as attaching to subcutaneous operations, and in particular those having for their object the section of tendons or muscles. 3. The occlusion of subcutaneous wounds, completed in their second phasis, or after the healing of the puncture in the integuments, gives rise to their relative innocuity and mode of cicatrization, by applying the tissues one against the other, excluding the contact of all foreign bodies, and multiplying the relations of the organizable liquid or blastema with organized and living matter. 4. Suppurative inflammation is excited exceptionally in subcutaneous wounds, in consequence of accidental local irritation, too considerable an effusion of blood, or defective occlusion of the wound, which then falls into the class of traumatic lesions having communication with the exterior. 5. The cicatrices of the various tissues in subcutaneous wounds healed by immediate organization, do not differ in their nature from those which succeed to many suppurating wounds. The formation of pus is only detrimental to the production of perfect cicatrices under certain conditions.

In respect to this last position, we may extract a few of M. Bouvier's remarks:

Among the circumstances that influence the perfectibility of cicatrices—i.e., the reproduction of the injured tissue—are, the kind of wound, the situation of the divided parts, and the mode in which the healing is effected. Thus, in wounds with loss of substance and of large surface, in which the tissues implicated form separate masses that are entirely exposed, the reparation is usually limited to the production of a cutaneous cicatrix, with some rudiments of cicatrix proper to the other tissues. But if the wound, though an exposed one, be extensive rather in depth than in superficies, the tissues occupying the deeper parts may become united by cicatrices proper to them, independently of the new skin destined to close the entry of the wound; and such union is even possible when the two sides of the divided tissues are not in actual contact, providing the separation be not too considerable. The distance within which this is possible, and the length of cicatrices that can be thus formed, vary in different tissues.

Suppuration of even deep wounds exerts two kinds of influence on the reparation of tissues other than cutaneous: 1. If the suppuration be so abundant that the extremities of the divided tissues float in pus, and the neighbouring tissues are destroyed around them, consecutive union is very imperfect, or does not take place at all. 2. Even in the absence of such unfavourable conditions, intense, extensive, or prolonged suppurative inflammation may impair the regularity of the reparative action by accidental adhesions, and by the fusion and induration of the different tissues, which it determines far more easily than does the moderate and circumscribed inflammation of subcutaneous cicatrizations. Still this is only exceptional and accidental in the healing of deep-seated wounds. Far more numerous are the cases in which, in spite of the suppuration, the vacancy in each tissue is definitively filled up by cicatricial tissue similar to that which is supplied in non-suppurating wounds. That this is the case with osseous tissue is familiarly known; and the facts in relation to tendinous tissue, though less known, are no less real, as the writings of Ammon, Blandin, and M. Bouvier himself, amply testify. Under the above-mentioned conditions, also, the nature of the substance intervening between divided muscles is the same, whether there be suppurative inflammation or not. The subject of the reunion of nervous tissue is still a matter of dispute: but it has not been demonstrated that the absence of suppuration, *cæteris paribus*, renders the union more complete, or is the sole condition necessary for reproduction.—*Archives Générales*, tome vi. pp. 54—72.

On the Operation for Partial Staphyloma of the Cornea. By Dr. A. QUADRI.—Little has been written upon partial staphyloma, and in this paper the author recals attention to a mode of operation long since instituted by his father, J. B. Quadri, of Naples. The case in which he exemplifies it occurred in a child, aged 6 years, who, as the result of a scrofulous ophthalmia, exhibited a staphyloma at the lower and inner parts of the left cornea, which gradually increased until the pupil was covered, and vision lost, about a third of the cornea remaining uncovered. The patient being etherized, a knife was passed through the cornea, as in the operation for cataract. It was found to be so thin that, after the incision, it became flaccid, and the staphyloma disappeared. The flap was seized by the forceps, and a small portion of its lower part cut off. The eye was kept closed until the third day, when, on examination, a loss of substance was observed at the base of the staphyloma, having a circular form, like an ulcer of the cornea, and being surrounded by a whitish aureola. To prevent prolapse, the eyelids were again carefully closed, and the case left to nature. At the end of eighteen days the eye was reopened, and cicatrization was found to be complete. The cornea seemed, perhaps, a little flattened, as compared with that of the other eye. The upper part, which had continued transparent, had descended to opposite the pupil, so that the patient now began to see again. After employing laudanum for some time, the cornea became clearer around the cicatrix, and the portion of it covering the pupil was quite transparent. There only remained an anterior synæchia, which prevented the pupillary motion at the lower part.

This observation not only shows the great utility of the operation, but also, contrary to the opinion of Scarpa, the little thickness of the cornea in some of the staphylomas of children. In the present case it was thinner than in the normal state, and had neither the hardness nor alveolar structure indicated by Scarpa.

M. Warlomont states that, since he has received M Quadri's communication, he has practised this operation for partial staphyloma twice, with success. In the first of these cases, in which there existed a considerable staphyloma at the lower part of the cornea, with preservation of some transparency in the upper segment, the ablation of a flap restored to the cornea nearly its normal form and convexity; and the operation in the second case was still more successful. It is to be observed that the loss of substance practised on the cornea should be much less considerable than would seem to be required for the correction of the deformity. The facility with which the lips of the wound unite renders the operation, when skilfully performed, almost inoffensive; still it is a delicate operation, and for its due performance, especially in children, the induction of anæsthesia is indispensable. As to the place of election for the excision of the flap, we have always to fear, when it is made below, that the resulting cicatrix may intercept some of the luminous rays. When made at the upper part, a portion of the iris might chance to become engaged in the wound, and the pupil might then be drawn up beneath the upper lid. Seeing, too, the difficulty of operating upon the inner side, M. Warlomont recommends that the excision should be made externally, as any cicatrix or traction of the pupil that might result would prove there of comparatively little consequence.—*Annales d'Oculistique*, tome xxxiv. p. 17.

On the Treatment of Artificial Anus with Irreducible Eversion. By M. Gosselin.—A man, aged 68, of good health and robust constitution, having undergone the operation for inguinal hernia, October 19th, the intestine was found gangrened, and was excised. Between then and the 20th November the gangrened parts had come away, and an artificial anus had formed, with an irreducible eversion of the intestine, the size of a walnut. No stool by the natural passages had occurred since the operation. The upper part of the small intestine was the part implicated, inasmuch as, half an hour after food had been given, there was an escape of a greenish fluid, without fæcal odour, which continued to flow for some hours. There was no tendency to a spontaneous cure, as manifested by the formation of Scarpa's infundibulum; and the patient was quite unable to bear the application of compresses, which was several times tried. Rapid emaciation ensued, not only on account of the short track the aliments had to traverse, but from the small quantity of these taken, the patient having little appetite, and wishing to avoid the irritation of the erythematous skin produced by the issue of the fluids. He would, indeed, have sunk, had not his strength been somewhat maintained by injecting, two or three times a day, about a pint of broth into the lower end of the artificial opening. Finding however that, in spite of this aid, the patient's strength was still sinking, M. Gosselin resolved to have recourse to an operation for the radical cure of the affection by section of the septum, for which he employed Dupuytren's enterotome. This was applied on the 20th, and removed on the 29th November, the first natural stool taking place on the 27th, stools continuing to pass from time to time. They then became more rare, and the fluids escaped from the wound pretty much the same as before the operation. The eversion, too, increased daily in size, and became more and more irreducible. On examination with the finger, the septum could now only be felt at seven or eight centimetres' depth, while the two ends of the intestine communicated by a sort of cavity in common. The external opening, though much narrowed, still admitted the thumb, and the integuments were drawn towards it, and had a tendency to sink in there. The obstacle to a definitive occlusion was presented by the irreducible eversion; and M. Gosselin resolved (28th December) to complete the treatment by a supplementary autoplastic operation. By this he was desirous of procuring a raw surface of the everted intestine, and bring the integuments in contact with it. The mucous membrane was first carefully dissected off, which implied a very tedious procedure, as the thin fragments were easily torn, and it was very important not to penetrate to the peritoneum. Its removal was executed without going beyond the muscular layer, and was rendered easier by the thickening which it and the subjacent cellular tissue had undergone. This gave but little pain, and was attended with little hæmorrhage. The patient had been kept fasting from the day before, in order that the intestinal contents might not interfere with the operation or the subsequent adhesive process. Next a circular incision of the skin was performed, at a centimetre and a half from the edge of the opening, and all the skin between this opening and the incision was removed. This stage was of shorter duration, and only presented some difficulty, by reason of the condensation of the subcutaneous cellular tissue. The denuded surfaces were now inflected towards each other from one side to the other, and retained there by an assistant. The disposition given to them was such that the approximated bleeding parts in reality formed two planes—a superficial one, at the level of which the abdominal wall was in contact with itself, and a deeper one, produced by the contact of this same part with the intestine, deprived of its mucous membrane. Five points of quilled suture were passed through the superficial plane of the wound; and, as the parts were somewhat tense, an incision of six or seven centimetres in length was made on each side of, and at about two fingers' breadth from, the suture—comprising the skin, subcutaneous cellular tissue, and aponeurosis of the great oblique. The patient was kept fasting, and was sustained during this period upon broth glysters. The sutures came away on the sixth day, adhesion having taken place. Shortly after, however, there was some discharge of intestinal fluids near two of the points of suture, which remained fistulous until the 15th February, by when, through the agency of compression, they had become closed. At the present time, the cicatrix is complete, and feels very solid. At its level, the integuments adhere to a deep-seated, dense mass, which appears to be the everted part quite confounded with them.—*Revue Médico-Chirurgicale,* tome xvii. pp. 334—9.

QUARTERLY REPORT ON MIDWIFERY.

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I. Menstruation: Physiology and Pathology of Non-pregnant State.

1. *On the Modifications of the Uterus at the Menstrual Epoch.* By M. Ch. Judee. (Rev. Méd.-Chir., April, 1855).
2. *Memoir on the Origin and Development of Retro-uterine Hæmatocele.* By M. Laugier. (Acad. des Sci., Feb. 26th, 1855. Rev. Méd.-Chir., May, 1855.)
3. *On the Production of Ovarian Cysts from Graafian Follicles.* By Rokitansky. (Schmidt's Jahrb. Jahrgang, 1855. No. V.)
4. *On Bleeding Fibroid Tumours of the Uterus.* By J. H. F. Albers. (Deutsche Klinik., No. IX., 1855).
5. *On the Removal of Large Fibrous Tumours from the Uterus.* By Macher. (Schmidt's Jahrb., No. VI., 1855).
6. *On the Employment of Alum in the Treatment of Diseases of the Female Genital Organs.* By M. Gautier. (Rev. Méd.-Chir., January, 1855).

1. M. Ch. Judee relates the appearances found on post-mortem examination of the bodies of three women who died about the epoch of menstruation. His observations are of great interest, and as it is of importance to collect facts of this nature, we give them in detail. Case 1, was that of a young woman who died of acute delirium, on July 17th, 1853, in the Salpêtrière. A few drops of blood had escaped from the vagina the night before she died. The body of the uterus was voluminous; its walls thickened. On the whole internal surface was seen a layer of gelatinous appearance and consistency, of about two millimetres in thickness. The colour of this was light red; it ceased abruptly at the upper orifice of the neck. The interior of the neck was of a yellowish colour; it appeared to have undergone no modification under the influence of menstruation. The os tincæ was swollen, so as to narrow the orifice; its colour of a deep blue: some spots were black like lung struck with apoplexy. On squeezing it, droplets of blood oozed out. On section (longitudinal) of the organ, the tissue of the body and of the neck was found to be normal; at the level of the lips of the os uteri this appearance vanished, and was replaced by a kind of magma, containing in its interior traces of fibres, presenting much the aspect of pulmonary apoplexy.

Appendages of the Uterus.—The right ligament exhibited marked congestion, and a beautiful capillary network. The same injection was seen on the outside of the Fallopian tube; the inside was in the same condition, especially near the uterine end. The kind of membrane, of light red, described in the uterine cavity, existed also near this point, and was thick enough to nearly close the tube. (Owing, we presume, to a printer's error in the original, we are unable to refer the following appearances to the proper ovary—both are called left.) On the external surface was a prominence the size of a cherry, of a deepish black, surrounded by a red areola, fading away in intensity. On one of its sides was a hole, admitting a probe. On dividing the ovary, there were seen, besides numerous scars, four bodies, of various shapes. The deepest, of the size of a hemp-seed, looked like a white membrane folded many times on itself. The next, somewhat bigger, was principally formed by fibres, crossed in all directions, colour bluish. The third was nearly superficial, bigger than either of the preceding, its colour deep red. The fourth was the biggest of all, and equal to one-third the bulk of the entire organ. It was immediately in apposition with the fibrous envelope of the ovary. It was formed of two distinct parts: one external, of irregular form, toothed throughout its interior, and of a deep red colour; then there was a membrane investing all the toothings, and serving as a pouch, for a clot of a colour much less deep than the most external one. It cannot be doubted that these two latter were corpora lutea.

Case 2.—A woman entered the Salpêtrière on January 27th, 1854, and died on the 6th of February following; she was seized with delirium, then a few days afterwards the menses appeared, and did not cease to flow till six days before death.

The uterus appeared more voluminous than usual. Its internal surface was lined by a kind of membrane, thick and hard, of a deep brown red. This could be easily raised off by the handle of the scalpel; it disappeared at the level of the neck. The os tincæ was dark red, soft, and easily lacerable. The left Fallopian tube was congested; the left ovary the same, it contained a corpus luteum filled with coagulated blood, of the size of a nut, and on its surface a scar.

Case 3.—A woman, aged 23, entered the Hôtel-Dieu on August 6th, and died the same day. She had hanged herself.

The uterus seemed increased in volume. The cavity was larger than in the physiological state; it was lined by a soft reddish membrane of five or six millimetres' thickness, easy to remove by scraping with the handle of the scalpel. Examined by a lens, it presented the appearance of a very full and beautiful capillary network, enclosed in a membrane like a mucous membrane. This membrane was continued into the Fallopian tubes, but ceased at the orifice of the os internum uteri. The os tincæ, on the other hand, was strongly congested, and resembled in aspect an apoplectic lung. The right ovary, which was very large, contained a corpus luteum of the size of an almond, showing a scar yet imperfectly closed.

M. Judée enunciates the following conclusions:

1. The blood which escapes by the vagina at the menstrual epoch proceeds neither from the cavity of the uterus nor from that of the cervix.

2. It proceeds entirely from the lips of the cervix, which are strongly congested at that time.

2. The memoir of M. LAUGIER contains a valuable examination of the principal questions relating to the origin and development of retro-uterine hæmorrhage. This name is given to a cyst of blood situated between the rectum and the uterus, which, pushing forward the posterior wall of the vagina, gradually filling the pelvic cavity, and rising above the iliac-fossa, may open by itself in the vagina or rectum; the contents being clots deposited on the walls and in the cavity, together with a thick, viscous, syrupy blood, of the colour of dregs of wine, or chocolate.

The situation, progressive and almost periodical course, the contained liquid, the accompanying metrorrhagia, characterize this cyst with sufficient precision to render the diagnosis almost certain.

The only tumour of the cavity of the pelvis with which it might be confounded, is another sanguineous cyst, that results from tubal or interstitial extra-uterine gestation in those very rare cases in which the rupture of the Fallopian tube and uterine fibres does not immediately, or very quickly, entail the death of the woman. Of this M. Laugier knows but two instances.

The history of retro-uterine hæmatocele is quite new, and as yet incomplete. Is the encysted blood contained in the peritoneum? Can it be accumulated beneath the utero-rectal cul-de-sac outside this membrane? But the most interesting question is as to the origin of the blood.

A specimen has been presented to the Surgical Society by M. Denonvilliers, which suggested the idea that if in spontaneous ovulation the migration of the ovule is imperfect, and hæmorrhage considerable, some blood may fall into the retro-uterine cul-de-sac; and that, if an adhesive peritonitis sequestrate this effusion, the retro-uterine hæmatocele is formed. To look upon the imperfect migration of the ovule as the cause of the hæmatocele must be regarded as a simple hypothesis, and especially because complete injection is not incompatible with retro-uterine effusion of blood; since this sometimes commences some days after menstruation, at an epoch when the ovule is already in the Fallopian tube.

The progressive growth of the hæmatocele also calls for an explanation. But since all observers have remarked the coincidence of a menstrual epoch with the commencement of the hæmatocele, and some go so far as to regard it as the effect of a deviation of the catamenia—another hypothesis less founded than the first—it is natural to assign a part to the phenomena of spontaneous ovulation in the etiology of the disease.

Is there coincidence between the formation of the hæmatocele and spontaneous ovulation? In the first place, the simultaneity of the hæmatocele and the eruption of the catamenia is not perfect. Sometimes the catamenia appear as usual, a metrorrhagia follows, but for two or three weeks, then stops, and on the very next day an acute abdominal pain indicates the commencement of the hæmatocele. In other cases the catamenia have passed eight days, presenting nothing particular, when acute pains arise, a metrorrhagia takes place, and then the hæmatocele. In another case abdominal pains follow a metrorrhagia of three weeks' duration; six months later, the catamenia last five days instead of three, and it is after this more abundant menstruation that abdominal pains reappear. Dating from this moment, there is suppression of the menses, and development of the hæmatocele.

In a fourth case, delay of three weeks; then flooding, lasting for six weeks; at length, hæmatocele.

In a fifth case, delay of two months; then flooding; and eight days afterwards signs of hæmatocele.

In a sixth patient, acute abdominal pains; six weeks afterwards the menses appear, last for four days, as usual; on ceasing, pains more severe, and growth of the hæmatocele.

In a seventh case, the menses had taken place; then acute and sudden pains occurred; next period, a delay; the menses appear, and last but two days, pains increasing in severity; at the third epoch, menses last but twenty-four hours, and the pains increase at the same time as the hæmatocele becomes developed.

Several facts result from these observations.

1st. Most frequently the hæmatocele begins after the catamenia, sometimes several days after their cessation. Now, according to the theory of Bischoff, the ovule leaves the vesicle at the moment when the menstrual flow ceases. The beginning of the hæmatocele would, on that theory, therefore coincide with the escape of the ovule, or would follow closely upon it.

2nd. In more rare cases the suppression of the menses, or their progressive diminution, is the sign of its appearance.

3rd. An almost constant phenomenon, either after a delay or following a regular epoch, is a prolonged rather than a profuse flooding, which precedes and accompanies the hæmatocele, and is renewed at each epoch of increased development.

There is, therefore, almost complete coincidence between the moment of spontaneous ovulation and the commencement of the hæmatocele.

A second relation between this disease and vesicular ovulation is, that all the circumstances which, during or very near the menstrual epoch, are of a nature to increase the sanguineous congestion of the ovaries, may become predisposing or accidental causes of hæmatocele.

Spontaneous ovulation and retro-uterine hæmatocele have a character in common, which establishes that their seat is in the ovary.

The hypogastric pains felt during menstruation are generally referred to the uterus. A more special examination has enabled M. Laugier to determine, that in the great majority of cases there exists a pain at the level of one or both ovaries. M. Laugier has extended this observation to the origin of hæmatocele; he has observed this unilateral ovarian pain in every case of retro-uterine hæmatocele. In hæmatocele the unilateral pain is much more intense than in spontaneous ovulation. From the commencement the woman has felt it acutely, and at a more advanced period, when the sanguineous cyst occupies almost the whole pelvic cavity, and sometimes rises into the two iliac fossæ, the surgeon may still detect this ovarian pain, which is increased by gentle pressure.

M. Laugier then recites several anatomical observations. In the specimen referred to at the beginning of this article, the ovaries corresponded by one of their aspects to the interior of the sanguineous focus, and both presented on this aspect several apertures opening into the cyst. The interior of these apertures was roughened by reddish fibrinous products. The two ovaries were covered on the peritoneal side with false membranes, which sealed them to the broad ligament.

In a specimen examined in 1853, the only one possessed by the Musée de la Faculté, the ovary opened into the cyst, retains only traces of its tissue. It is an agglomeration of five or six nodules of the size of hemp-seed, of fibrinous consistence. They project into the cyst of the hæmatocele. On the peritoneal side, they are covered by the ovarian envelope and false membranes, which make a thick shell.

In another specimen, presented some weeks ago to the Anatomical Society, the left ovary, mis-shapen, globular, and reduced to the size of a nut, formed a part of the wall of the sanguineous cyst, and opened into its cavity. Its tissue was friable, reddish-yellow, and quite similar to the fibrinous *débris* contained in the cyst of the hæmatocele.

Lastly, one of the autopsies related in the thesis of M. Vignès, demonstrates that the ovary may be completely destroyed. All the signs of hæmatocele had been observed during life. At the autopsy, the Fallopian tube and ovary had disappeared.

Here, it is not simply a congestion carried to the point of causing the hæmatocele at the time of spontaneous ovulation; it is a kind of breaking-up of the ovary, the tissue of which, profoundly altered, diminishes little by little, and is at last destroyed.

M. Laugier relates a case from veterinary medicine, proving that during the rut in animals, the ovary may be the seat of similar disorders. On the 20th September, 1852, a cow perished rapidly from internal hæmorrhage at the epoch of the rut. The abdominal cavity contained twelve quarts of blood. The left ovary was the size of a man's head. Its cortical envelope, which in the natural state is dense and compact, was so thinned by distension, that its fibres were completely separated from each other. This monstrous ovary was deeply cracked in a line with the greater curvature. The interior presented a tissue giving no resistance to the edge of the knife; it was no more than a reddish pulp, breaking under the finger, and closely resembling the splenic pulp. The corpora lutea were completely isolated from the tissues of this organ; they had preserved their normal character; their more compact structure had probably kept them from sanguineous imbibition. In the interior of the Fallopian tube there was a small vesicle, the size of a kidney-bean, filled with a yellow fluid: this seemed to be the ovule. Who could hesitate in recognising here a hæmorrhage, at first intra-ovarian, and which, if less profuse and less rapid, would have given rise to a hæmatocele?

M. Laugier concludes with the following deductions:

(1.) Spontaneous ovulation is, truly, the exciting cause of intra-uterine hæmatocele.

(2.) The physiological congestion of the ovary during spontaneous ovulation, with persistence of the opening of the Graafian vesicle, does not give rise to hæmatocele.

(3.) For this to be produced, there must be an exaggerated congestion, sometimes brought about by accidental causes, the action of which is exerted during, or some days after, menstruation. Abortions are not, as has been supposed, the immediate causes of hæmatocele.

(4.) It is especially the returns of spontaneous ovulation that gradually augment the volume of the hæmatocele.

(5.) The successive ovarian vesicles open into the hæmatic cyst, and remain gaping, so that the ovary is destroyed by a small number of spontaneous ovulations taking place in the conditions presented by this organ from the commencement of hæmatocele.

(6.) The rupture of a Graafian vesicle furnishing the passage opened for the blood which escapes from the ovary, the cyst of the hæmatocele will be for the most part intra-peritoneal.

(7.) Spontaneous ovulation and hæmatocele have a common character in unilateral abdominal pain, the seat of which is the ovary, in which vesicular ovulation is taking place.

(8.) Rut may cause in animals an ovarian congestion, followed by rupture of the organ—that is, accidents resembling retro-uterine hæmatocele.

3. A woman, aged 26, who had a year previously given birth to a child, and had subsequently menstruated, was supposed to be in the fourth month of pregnancy, when severe and continuous hæmorrhage set in, followed by the discharge of an hydatid mole, weighing three pounds, and death three days later. The left ovary was the size of a child's head, the right of a fist. Both were filled with cysts, varying in size from a cherry to a walnut; most of them contained a coagulum of blood, the surface of which was invested with a layer of pure fibrin. Some of the cysts were filled with a yellowish, others with a greenish, fluid. In one of the cysts, a delicate bladder, consisting of a finely-granulated membrane, was suspended, and was also lined by a membranous delicate layer of fibrin, in which numerous round and fusiform corpuscles were found. Many other cysts contained a coagulum consisting of fibrin, and invested and traversed by whitish membranous matter. At the surface of the ovarian tumours there were, here and there

between the larger cysts, follicles varying in size from that of a hemp-seed to that of a bean, containing a colourless or greenish-brown fluid, with membranous flocculi. All the cysts contained a pulpy, very opaque ovule; their zona pellucida was not well defined externally, and the germinal vesicle was only detected in a single ovule.

4. Dr. **Albers** relates two cases of fibroid uterine tumours, in illustration of the pathological fact, that a rich vascular network is at times observed on those fibroid tumours which project into the uterine cavity, and are invested by the mucous membrane. This erectile network, according to Dr. Albers' observations, is produced only in those cases in which the tumours have reached a certain size—that of an apple at least—and may be the occasion of persistent and fatal hæmorrhage.

Case 1.—A woman, aged 30, who had borne two children favourably, was received into the Clinique six weeks after her third labour, on account of persistent flooding. On examination, the os was found more open than usual, and bright red blood issuing from it. No treatment availed. The patient died, four weeks after admission, of exhaustion. There was found in the fundus uteri a fibroid growth of the size of a large apple, covered by mucous membrane, half imbedded in the muscular wall, half projecting into the uterine cavity. The mucous membrane was of velvetty softness, and consisted of a great crowd of fine vessels, which on one side formed a decided projection; in this place there were open vessels, and the whole spot had a gnawed appearance. This erectile tissue lost itself in the edges in the mucous membrane, but stretched over the whole tumour, which was a simple fibroid. From its size, it was judged that it had existed before the commencement of the third pregnancy.

Case 2, is described by G. Tuerk, in his Dissertation, 'Iconographia ingentis tumoris fibrosi in utero primæ parturientis. Bonnæ, 1854.' A woman, aged 29, had always menstruated regularly, from the age of 17 until the commencement of pregnancy. Pains came on in the sixth month; it was found that the cavity of the pelvis was almost filled by a soft tumour; the head was high above the brim. After a time the head came down, compressing the tumour, and, being seized with forceps, was delivered. The tumour immediately resumed its former position in the pelvis. The patient did well for a time. But a year later she again miscarried at the sixth month, with great flooding. Two months after this, flooding recurred, and again from time to time at shorter intervals, so that six months after the abortion she was carried off. There was found at the lower part of the right side of the uterus a fibroid tumour, surrounded by muscular fibres; on the left, a smaller globular fibroid, stretching out the mucous membrane, projected into the uterine cavity; and in the walls of the fundus ten more small tumours.

Dr. Albers concludes that the blood poured out of these tumours, comes not from the vessels in the tumour itself, but from the vessels which are found on its periphery. The vascular network that surrounds the fibroid swelling, and causes the flooding, has the greatest similarity with that found in the neighbourhood of hæmorrhoidal tumours.

5. **Macher**, of Mainz, has related two cases of fibroid polypi of the uterus, of pathological and therapeutical interest. A woman, aged 46, had for a year suffered from menstrual irregularity, and for eight weeks had observed strong flooding. There was found a fibrous polypus, the size of a child's head. It not being low enough for ligature, a hole was made in a soft place with the finger, and emollient injections introduced. As the polypus some days after was still unfit for operation, several pieces of the fibrous tissue were removed, whereupon a quantity of bloody pus flowed. On the following day the polypus had all vanished, excepting a few membranous shreds, which were hanging out of the os uteri. Nature had effected the greater part of the operation. Under injections—firstly mucilaginous, then of quinine—the remains were removed.

The other case is from Unger:—A large fibrous polypus was cast off spontaneously, removal from the vagina only being called for.

M. Gautier reports various modes in which alum is employed with success at the Lourcine, at Paris, in diseases of the genital organs of women. It may be applied in the form of powder to the vagina by means of the speculum, or a piece of cotton-wool of the size of a walnut, containing, wrapped in its centre, a teaspoonful of powdered alum, and having a thread attached to it, to aid subsequent removal, may be placed in the vagina in contact with the cervix, by aid of the speculum; or plugs of lint, smeared with an alum ointment—half alum, half lard—may be inserted: fine ones may be made for introduction into the cervical canal—these were contrived by M. Guéneau de Mussy; lastly, alum may be used in solution, by injection or irrigation. M. Gautier especially recommends the dry aluminous plug in blennorrhagia, and in vaginal vegetations.

II. Gestation: Fœtal Physiology and Pathology.

1. *Observations on Fœtal Pulsations.* (From Dr. Pulling's Semi-Annual Report of the New York Lying-in Asylum. Amer. Med. Monthly. March, 1855.)
2. *Reduction of the Size of the Fœtus by Regimen during Gestation.* By M. P. Dubois. (Journal de Méd. et de Chir. Prat., and Rev. Méd.-Chir. Juin, 1855.)
3. *On Congenital Dropsy of the Kidneys.* By Rud. Virchow. (Verhandl. der Phys. Méd. Gesellsch. in Würzburg. 1855.)
4. *A Case of Natural Labour in a Negro-girl, aged Thirteen.* By W. S. Stoakley, M.D. (Med. Exam., Philad. April, 1855.)
5. *A Case of Hæmorrhage during Gestation from Cauliflower Excrescence, and Cure by Excision.* By Dr. Jos. Spaeth. (Wochenbl. d. Zeitschr. d. k. k. Gesellsch. d. Aerzte zu Wien. 2nd April, 1855.)

1. In Dr. Pulling's 'Report of the New York Lying-in Asylum' are some interesting observations on the fœtal pulsation. The fœtal circulation was examined in 33 cases. In 2, the fœtal heart could not be heard. In the remaining 31, the point at which its pulsations were most distinct, during the last month of pregnancy, was from one to three inches below the horizontal plane of the umbilicus, being situated on the median line once, to its right five times, to its left twenty-five times. In two instances it changed from the right to the left side of the abdomen, a few days previous to labour. The frequency of the fœtal pulse before the commencement of uterine contraction, varied from 104 to 168, averaging 134. In 2 cases, in which the mothers entered the asylum labouring under a severe bronchial affection, attended by much vascular excitement, it was 152 and 160 respectively. The fœtal pulse during labour averaged about 128. Its range was not fully determined, it being subject to great variation. In one instance, in which ergot was given two hours prior to delivery, exciting the uterus to strong contraction, the frequency of the pulsations was immediately diminished from 140 to 128 per minute, remaining thus depressed until the expulsion of the child.

2. A case observed by M. Dubois throws some light on the question, whether the size of the fœtus can be reduced by following a particular regimen during gestation? A woman, shortly after pregnancy, suffered intolerable pain after taking food, so that she was afraid to eat; she restricted herself to the most scanty diet until delivery. The child was born at eight months and a half; it weighed scarcely 1500 grammes, but it was very lively, and sucked well. The placenta presented nothing particular but smallness. A case is referred to in which M. Depaul succeeded in reducing the size of the fœtus by spare regimen; and M. Dubois refers to another of a woman who, having escaped after a bad labour from pelvic contraction, was subjected in her second pregnancy to a very strict

regimen, and the child was so small that it was delivered naturally, yet in condition to live.

3. Rud. Virchow's communication is one of great interest, in a pathological and obstetric point of view:—A woman delivered herself secretly in a wood: the dead child became the subject of medico-legal investigation. The kidneys were found enormously enlarged. The appearances, which are minutely described, lead to the conclusion that the kidneys were affected by cystoid degeneration, with complete atresia of the pelves and papillæ. Virchow then proceeds to relate other instances of the same degeneration which he had observed. In every case it was found that the point of departure of the cystic formation was ectasia of the urinary canals or of the Malpighian bodies.

Virchow then refers to this affection as a cause of obstruction to labour. Several cases of difficult labour from this cause are known. With such a disease, the prolonged life of the child is scarcely possible.

4. Dr. Stoakley, of Northampton County, Virginia, relates the case of a negro girl, of thirteen years of age, who brought forth a healthy child, of the ordinary size, after a natural labour of a remarkably easy character.

5. Dr. Jos. Spaeth relates an interesting case of excision of the os uteri during pregnancy, under the following circumstances:—A woman, aged 29, was pregnant a third time. About the end of the fifth month, without obvious cause, copious hæmorrhage in clots suddenly appeared. This ceased under rest and acids, and at the end of a week a whitish muddy discharge set in. There were no pains. In the eighth month hæmorrhage returned; the fœtal heart was heard. A tumour the size of the fist was felt in the vagina, covered with clots, and resembling the placenta in feel. The tumour was attached by a narrow neck to the posterior lip of the os uteri. Blood flowed freely from the surface of the tumour. A ligature was applied round the neck of the tumour, which was then divided by scissors below the ligature. The bleeding ceased, and no fever followed. The ligature fell next day. On the eighth day after excision the membranes burst; the fœtus presented by the breech; it was born alive, but weak, being of eight months' growth, and died shortly. The patient recovered well. Remaining afterwards under the observation of Dr. Spaeth, she suffered no pain, leucorrhœa, nor flooding.

The examination of the tumour showed that it was a specimen of that form of epithelial cancer which Sir Charles Clarke called the cauliflower excrescence.

III. Labour.

1. *On Uræmic Eclampsia.* By F. Wieger. (Schmidt's Jahrb. No. 7, 1855.)
2. *Two Cases of Dystochia; one from Enormous Enlargement of the Kidneys, the other from a Carcinomatous Liver.* By MM. Siebold & Noeggerath. (Rev. Méd.-Chir., Janv. 1855.)
3. *A Cephalo-Pelvimeter.* By Ans. Martin. (Schmidt's Jahrb. No. 7, 1855.)
4. *On Turning as a Substitute for Cephalotomy in Contraction of the Pelvis.* By M. Dubreuilh, fils. (Rev Méd.-Chir., Mars, 1855.)
5. *A Case of Symphyseotomy.* By M. Maslieurat Lagémard, of Grand-Bourg. (Bull. Gén. de Thérap., tom. xlvii. onzième livrais.)
6. *A Case of Extra-Uterine Gestation cured by Gastrotomy.* By M. Rousseau. (Bull. Gén. de Thér., Mai, 1855.)
7. *On Extra-Uterine Gestation.* By Bremont (Gaz. des Hôp. 24, 1855), and Monod (L'Union Méd. 1855).
8. *Perineal Fistula left by the Transit of the Infant through the Perinæum.* By J. Y. Simpson, M.D. (Edinb. Med. Journ. July, 1855.)

1. **Dr. Wieger** proposes to defend that new view of eclampsia which regards it as a complication of uræmia, against the older views sustained by L'Huillier and Depaul. He divides his memoir into six parts.

(1.) *Critique of the Negative Observations.*—The author seeks to show that the cases of eclampsia without albuminuria recorded by L'Huillier, Depaul, and Mascarel, must be eliminated, either because the albumen was not searched for with sufficient care, or because the absence of albumen does not prove the absence of uræmia.

(2.) *On the Condition of the Kidneys.*—He seeks to invalidate the assertion of Blot that the kidneys in eclampsia are not always diseased, by the collection of as many cases as possible. He maintains that in albuminuria, fibrinous cylinder-casts are always found in the urine, and that these often increase after delivery, and are found, as Braun and Litzmann have shown, so long as there is albumen in the urine.

He gives a table, which shows that up to the tenth day of the puerperal state anatomical lesions in the kidneys are always found; and that profound alterations of the kidneys are more frequent than congestive conditions.

He concludes this section with the following deductions:—*a*. The kidneys may be diseased without albumen being separated in the urine in observable quantity.

β. The absence of albumen at a given time is no sure proof of the absence either of disease of the kidney or of uræmia. The presence of albumen at a given time stands in no relation to the stage of the disease.

γ. The albuminuria increases at the approach of labour, during labour, and the fits of eclampsia.

δ. The kidneys cannot pour out albumen in any considerable quantity, or during a certain time, without becoming clogged up and diseased.

ε. The disappearance of the kidney-disease is often complete and effected in a short time; often it persists in a slight degree, and becomes aggravated in following pregnancies.

ζ. When it persists during the puerperal state, the disease of the kidney induces other attacks, or causes complications, or aggravates existing ones.

η. Albuminuria grows with the occurrence of complications.

(3.) *Albuminuria and Œdema—a. On Albuminuria.*—Is there albuminuria without nephritis? The author cites instances from Simon, Schmidt, Henoch, and Canstatt, to prove the affirmative. As to the curability of the nephritis which attends pregnancy, he shows that as it depends upon transitory conditions, it is not like Bright's disease, which is commonly dependent upon persistent or recurrent external causes, as cold, &c.

By adducing the statements of Blot and Litzmann, which exhibit the presence of albuminuria in fifty-six primiparæ out of a hundred and seventy-eight, and in twenty-two multiparæ out of a hundred and fifty-nine, he confirms the opinion, that the first labour is a predisposing cause of albuminuria and eclampsia.

β. Of Œdema and Anasarca.—The presence or absence of this symptom has no constant value in diagnosis. It is not always present in Bright's disease.

(4.) *On Uræmia and Uræmic Symptoms—a. On Uræmia.*—Uræmia, the result of nephritis, is characterized in its chemical relations by the retention of water and excrementitial matter in the blood, which in its turn is impoverished by the loss of albumen, and sometimes of globules. The excrementitial matter is thus driven to the skin, stomach, or salivary glands, and even to the lungs, or accumulated in the serous cavities or cellular tissue, to be taken up again into the blood, to aggravate the uræmia. For this reason, a strong diuresis may often persist a long time without exercising a remarkable influence on the nervous system. The degree of intoxication can only be determined by simultaneous examination of the blood and of the urine. He cites two cases of Gegenbauer and Chiari; in the latter, urea and, by the decomposition of this, ammonia, were found in considerable quantity in the blood, and the prophecy that eclampsia would break out was verified. The woman died.

β. On Uræmic Convulsions.—In seventy-eight cases of Bright, Barlow, and Frerichs', there was amaurosis and amblyopia ten times; syncope, nineteen times; singing in the ears and deafness, ten times; convulsions, fourteen times. The cerebro-spinal symptoms which precede the attacks of eclampsia have the closest resemblance with those of Bright's disease. Out of a hundred and forty cases of eclampsia collected into a table, forty-three showed premonitory symptoms. Of the cases in which the eclampsia broke out before labour, there were forty per cent.; of those in which it began during labour, thirty per cent.; and of those in which it began after labour, twenty per cent., which were attended with premonitory symptoms. As premonitory symptoms the author enumerates vomiting and diarrhœa, but principally headache, disturbance of intellect, and often delirium; cramps and amblyopia, sometimes followed by blindness, not unfrequently precede. The changes of the pulse and pupils are too uncertain to be considered.

As characteristic of the so-called uræmic eclampsia, the author mentions that no predisposition lies at the foundation of eclamptic convulsions; they cannot become habitual; as a rule, the fits are frequently repeated. The disease is never chronic; it makes no periodical relapses, and seldom returns in subsequent pregnancies. The symptoms are those of epilepsy without the cry at the onset.

The question as to the relation between epilepsy and eclampsia the author regards as not settled, but inclines to the view that epilepsy predisposes to eclampsia. Apoplexy of the brain and membranes may cause fatal convulsions, and appears frequently as a complication of Bright's disease, and makes the diagnosis more serious.

(5.) *Etiology and Prognosis.—External Predisposing Causes.*—The bad method of living of the poor favours the disease, and is the reason why it is more frequent in lying-in hospitals than in private practice.

Individual Predisposing Causes.—First pregnancies, twin pregnancies, are mentioned. Dubois has pointed to distortion of the pelvis and rachitis, duration of labour, and mal-position of child, asphyxia, indigestion, mental excitations, as fright, anger.

The *mortality* is, according to Murphy, 24 per cent.; Blot, 35½ per cent.; Lever, 28 per cent.; the author, 30 per cent. The mortality among the children is given by Blot as 67 per cent., and by the author as 45 per cent. Many children died shortly after birth, without inspection revealing any tangible cause of death. Frerichs, Litzmann, and Braun, ascribe this death to intoxication of the blood.

(6.) *Treatment.*—The author divides the prophylaxis into a remote (against the albuminuria), and a treatment against the uræmic prodromata, shortly before the labour. The peculiar condition of the blood indicates the following fundamental rules of a rational treatment:

α. The blood must be improved by good nourishment, tonics, and iron: Miquel recommends a vegetable diet. *β.* Exciting diaphoresis by baths, &c. *γ.* Gentle purgatives. *δ.* Maintenance of the urinary secretion by gentle diuretics. *ε.* Direct action upon the renal obstruction, by abstraction of blood from the region of the kidneys.

Treatment of the Uræmic Pre-current Symptoms before Labour.—Tartar emetic, vapour baths, and scarification of the œdematous parts, are considered. Chailly recommends chloroform when there is great tenderness in the uterus. The author regards it only as a palliative against the convulsions, and not as against the fundamental evil, the uræmia. *General bleeding* he regards as a precious means. *The expectative method*, the author regards as admissible when the convulsions are not strong, or first appear during the expulsion of the child. *Opium*, much praised by many authors, is, according to the author, chiefly useful after delivery. *Cold affusions* are recommended by Recamier and Booth.

Coma after cessation of the fits, the author treats with diaphoretics, salines, and diuretics, since the condition of the brain is caused, for the most part, not by hyperæmia, but by serous infiltration. Of *revulsive measures*, the author rejects cantharides. Of anti-spasmodics, musk has been useful after too great depletion.

The author then adduces several cases in which artificial delivery was resorted to, showing that often there is a rapid cessation of the attacks after the emptying of the uterus, and that the mortality is about the same.

Abortion.—Convulsions which appear before the period of viability of the fœtus, end for the most part with its expulsion.

In eclampsia before the beginning of labour, he enjoins excitation of the pains and hastening of delivery. He uses secale cornutum for this purpose, when the head cannot be reached by the forceps.

2. M. Siebold, of Göttingen, relates a case of dystochia caused by enormous development of the kidneys in the fœtus. The head presented, but the expulsion of the child was arrested by the size of its abdomen. Repeated tractions were required. The circumference of the abdomen was seventeen inches. The child respired a few times and died. On inspection, two enormous tumours were found in the abdomen; these were the kidneys; the two weighed two pounds; each was six inches long, four wide, three thick. They presented convolutions resembling those of the brain. On removing the capsule, small cysts, containing a transparent serosity, were seen scattered about. On section, the cortical substance could not be distinguished from the pyramids; the greater portion consisting of cysts visible to the naked eye. The bladder was empty; the supra-renal capsules were atrophied. By aid of microscope, it was ascertained that the smaller cysts were dilatations of urinary tubuli.

The case related by M. Noeggerath occurred in the polyclinicum of M. Kilian, on the 8th September, 1854. The head presented, but was arrested, by failing uterine contraction, in the pelvis. The forceps were applied, but powerful traction, aided by strong efforts on the part of the patient, failed for some time in delivering the head. The head extracted, the shoulders resisted, and were with difficulty delivered. The principal mass of the fœtus, which had been dead ten days, consisted of abdomen, which was four times the normal size. The liver extended from the xiphoid cartilage to the pubis, and stretched across from one iliac spinous process to the other, so as to cover all the abdominal organs. It weighed two pounds and a quarter. The proper tissue of the liver was seen at intervals; but in its greater part it was replaced by a heteromorphous mass, similar to the grey substance of the encephalon. The microscope demonstrated the carcinomatous nature of the liver.

3. M. Martin describes a new instrument adapted for the double purpose of measuring the pelvis and the fœtal head. It is more simple than that of Kiwisch and Germann, but requires, as the inventor admits, great practice in order to use it. The instrument is thirteen inches long, of which seven belong to the part to be introduced into the genital organs, and is made of fine steel. It is bowed inwards in such manner that it can embrace the head in every direction, whilst it is bent outwards in a spiral form, in order to take the measures of the pelvis. In the middle part of the instrument are the measure and an index worked by a screw. (These details are insufficient, without plates, to give a clear idea of the construction of the instrument.)

4. M. Dubreuilh, fils, has related to the Medical Society of Bordeaux two cases in which turning was resorted to in contraction of the pelvis in lieu of craniotomy, according to the recommendation of Professor Simpson.

In the first case, a primipara, aged 22, healthy, was in labour at full term. The waters escaped on February 8th, pains having been felt the day before; labour continued throughout the 9th, and on the 10th, M. Dubreuilh being called, ascertained that the head was arrested at the brim; the promontory was greatly projecting, the antero-posterior diameter measuring only eight centimetres. The forceps, which had been tried before, were now tried again, without success.

M. Dubreuilh then resolved upon version; he withdrew the legs, abdomen, and arms without excessive difficulty, but the head resisted powerful traction. The forceps could not be applied. He tried in vain to bring the head through by passing the fingers into the mouth. Smellie's scissors were used to perforate the skull through the mouth. This emptied, the diameter did not lessen, and the neck was felt to yield under traction. The sharp crotchet was then deeply fixed in the right orbit, and at last the head released. It was found to be a very large one, and the diameters very slightly reduced, notwithstanding the escape of the brain. The patient recovered well.

In the second case, a woman, aged 30, was at full term, in her eighth pregnancy. In the two previous labours the fœtus had been mutilated on account of contraction of the antero-posterior diameter of the brim. The same proceeding was about to be instituted, when M. Dubreuilh coming in, proposed version After unheard-of efforts (*efforts inouis*), M. Dubreuilh brought down the feet, then trunk and shoulders. The head made great resistance; but energetic traction on the body, in the direction of the pelvic axis, brought it forth. The child was dead, and but recently. The woman recovered.

5. M. **Maslieurat-Lagémard** relates a case of symphyseotomy, an operation interesting at least on account of its rarity. The subject of it was a woman who had borne two children without anything remarkable occurring. In a third labour —November, 1847—the head was arrested, and symptoms of exhaustion set in: the forceps were tried several times in vain. Turning was then resorted to, the feet were brought down, but the head resisted all endeavours at extraction. The woman had now been three days in labour. Choice lay between Cæsarian section and symphyseotomy. The latter was selected. An incision was made in the median line of the symphysis, the cartilage was then divided by a blunt-pointed bistoury. The separation of the iliac bones was effected by pressing with each hand upon the antero-superior spines of the ilia. A slight crackling was heard in the sacro-iliac articulations, and the two branches of the pubis were found sufficiently parted to admit the finger. As soon as this was done, a very gentle traction on the body of the fœtus sufficed to bring forth the head, and finish the labour. The child was dead. A bandage was applied round the pelvis to bring the pubic bones together. Three days after, shivering and acute pains in the right leg set in; phlegmasia dolens became developed. The patient recovered, notwithstanding, and renewed her occupations at the end of fifteen days. M. Maslieurat-Lagémard has seen her recently; she has been delivered again since of a living child, well-formed, without difficulty.

It is right to add that M. Maslieurat states that he was unprovided with the instruments requisite for cephalotripsy or for ex-cerebration.

6. The case of M. **Rousseau** is one of unusual interest. It was one of primitive abdominal extra-uterine gestation, cured by gastrotomy. A woman who had borne one child was pregnant again. During the first months she felt passing pains in the left abdomen, and during twenty-four hours, vesical tenesmus. The menses were suppressed. Nine months after the presumed date of conception, the movements of the fœtus, which had been felt earlier and stronger than in her former pregnancy, were no longer perceived. Shortly afterwards, a discharge of blood took place by the vagina; and for fifteen days the secretion of milk, which had set in with the cessation of the movements of the fœtus, increased in activity. At this time fever, disorder of general health, and emaciation began, and the patient entered the hospital at Epernay on October 31st, 1852. In November six cauterizations were made on the left iliac region, where the head was felt. On December 6th, the section was completed by the bistoury, and the cavity of the amnion penetrated; the head of the fœtus was opened; the bones of the arch and the brim removed; the placenta and membranes were left *in situ*. The operation was effected without opening the peritoneum. No peritonitis followed; but

phlebitis in both arms appeared. Emollient injections first, then chlorinated injections, were thrown into the amniotic sac. Quinine was given. The phlebitis ended in recovery. The capacity of the amnios diminished day by day. The placenta continued to live, and took part in the cicatrization, which proceeded rapidly. The wound contracted, leaving only a small fistula.

7. The following cases of extra-uterine gestation are interesting. The first is related by M. BREMONT:—A lady, aged 37, who had borne two children—the last eight years ago—resorted to the thermal baths of Chaudes-Aigues, for the purpose of restoring menstruation, which had ceased, as she supposed, from a cold. Examined by M. Bremont, she was discovered to be pregnant; and it was considered by him to be a right tubal gestation. He endeavoured to hasten delivery by baths, local bleedings, and belladonna to cervix. In some weeks' time, the uterus descended lower into pelvis; and gradually the os externum uteri, then the os internum dilated, until he was enabled to pass the finger into the uterine cavity. He found the orifice of the right Fallopian tube open, like a cut quill, whilst the left was in normal state. Three weeks later, after strong pains, the birth of a dead six months' child took place. For six weeks after, the patient felt pains in the abdomen, and discharge of blood from the uterus, until the escape of a half-decomposed cyst. From that time she recovered.

The other case is recorded by M. MONOD:—A woman, aged 32, had had a natural labour six years before. In August, 1851, she believed herself again pregnant, although menstruation appeared regularly. In October, strong abdominal pains set in, resembling peritonitis, and were met by antiphlogistic treatment. During January and February, 1852, the movements of the fœtus were very painful. In April the patient looked for her delivery, and labour-pains set in, but ceased. Next day, vomiting, great pain in the abdomen, fever, and delirium appeared; antiphlogistics employed. Examination revealed complete anteversion of the womb; by rectum, a large hard tumour was felt behind uterus; external examination discovered a tumour filling the whole abdomen as high as the navel. Extra-uterine gestation diagnosed. Three weeks later the woman was well again. In September, 1854, the swelling had somewhat lessened. Severe pains came on again. A trocar was thrust in on the right of the navel, and about three pints of an odourless, yellow, thick fluid withdrawn; the size of the abdomen having returned, after eight days the paracentesis was repeated, when much pus, mingled with hair, came away. On the 6th January, 1855, five punctures had been made. She was now very pale and emaciated; the abdomen exhibited only doubtful fluctuation, and deep crepitation was felt. The uterus was anteverted; diarrhœa came on, discharging membranous substances and albuminous flocculi. On the 13th January a fresh puncture was made, which gave exit to much stinking pus and discoloured fluid. By means of Vienna paste and an incision, the next day a larger opening was made, through which several fœtal bones were drawn out on the 16th. The patient sunk, and died on the 17th. *Autopsy*:—In the rectum, signs of old inflammation; uterus anteverted: its cavity empty; the os uteri plugged up with a gelatinous mass; left ovary and tube healthy; right tube hypertrophied, pervious. The embryonic sac was attached to the right broad ligament, to the posterior surface of the uterus, to the greater and lesser omentum, to several points of the abdominal wall, and to a part of the rectum; at the end of the sigmoid flexure there was a large opening from the cyst into the rectum, and another led from the cyst into the right Fallopian tube. The walls of the cyst consisted of a double membrane. The right ovarium was atrophied. In the cyst were the remaining parts of the fœtal skeleton. There was no trace of a placenta.

8. Professor SIMPSON describes a preparation in the obstetric museum of the University of Edinburgh, which exhibits a fistula in the centre of the perineum, persisting after perforation by the passage of the child during labour. The Professor also quotes two similar cases, the only ones with which he is acquainted—

the one described by Marter, of Königsberg,[*] and the other by Halmagrand, of Paris.[†] M. Halmagrand cured the fistula in his case by dividing the anterior bridle of the perineum, cutting a raw surface at the seat of the fistula, and merely maintaining in apposition the thighs. Reunion and cicatrization took place.

IV. PUERPERAL STATE: LACTATION.

1. *Resorption of the Placenta.* By Dr. SABATIER. (L'Union Médicale, Avril, 1855.)
2. *On Mother's and Nurse's Milk.* By Dr. HERM. PLOSS. (Journ. f. Kinderkrankh. l. 2, 1855.)

1. DR. SABATIER relates three cases in which he thinks the placenta was absorbed.

CASE 1. A woman, aged 35, three and a half months pregnant, aborted of a fœtus after an injury. Copious hæmorrhage followed, but no placenta. The uterus contracted, and the os closed up. On the fifth day shivering and fever set in. Lochia abundant, purulent, and horribly offensive. On the tenth day a violent shivering and expectoration of a great quantity of fœtid matter, having same odour as the lochia. As the expectoration went on, the lochia diminished. The patient recovered well.

CASE 2. A woman pregnant the first time was delivered of two girls, but the placenta did not follow. Dr. Sabatier introduced his hand into the uterus, and felt the placenta partly detached. The detached portion was brought away by the hand. On examining it, there was found a deep fissure corresponding to the adhering part. The patient said that in the sixth month she had received a violent blow from a stick, a kind of crack had been felt in the uterus, and intense pain; the next day a little blood escaped by the vagina. After delivery, shivering, sweats, fever set in on the fourth day, and the lochia began to be offensive. For some days she got worse, until on the twelfth day a fresh shivering fit occurred, pain in the chest, and copious expectoration of fœtid matter. This continued for fifteen days, the lochia having ceased from its commencement. The sweats also ceased from the same time. The patient recovered. No escape of placental tissue was detected.

CASE 3. This patient was only seen by Dr. Sabatier casually. She aborted at the fourth month. The symptoms related were similar to those of the preceding cases.

Dr. Sabatier concludes that it is by the lungs that retained placenta is got rid of.

2. Dr. PLOSS's 'Memoir' is an interesting contribution to a subject in which our knowledge is very defective—namely, the variations that take place in the constituents and nutritive properties of human milk.

The water is increased by improper food, bad digestion, and in a peculiar manner in so-called strong constitutions. The child falls off, becomes anæmic, and its nightly cries indicate an unsatisfied call for nutrition. With this condition there is much urine, and scanty stools. The water is diminished by recurring pregnancy during menstruation, by intervening illness, especially by acute colitis and chronic enteritis. A diminution of solid food, and increased imbibition of water, are recommended; and if pregnancy recur, weaning.

The solid elements are increased under the conditions just named, as in colitis. The milk becomes too nutritive and difficult of digestion. There is diminution of the solid elements when nourishment is bad, in advanced age, typhus, and in chronic tuberculosis without diarrhœa.

Casein appears increased in much-developed breasts, menstruation, acute disease, and mental disturbance. The child soon suffers from constipation, aphthæ, and lastly, marasmus. The casein is diminished when nourishment is bad, in robust constitutions, chronic diseases, typhus.

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[*] Siebold's Journ für Geburtskunde, vol. ix. p. 726.

[†] Démonstrations des Accouchements, p. 577.

The butter is increased in much-developed breasts, pregnancy, acute, and still more in chronic disease. In this case, also, the nutrition of the child is gradually impaired. The woman should take free exercise in the open air, and a diet as free as possible from amylaceous and fatty materials; the child should take the breast more sparingly. The butter is diminished when nourishment is bad, in mental commotions, and tuberculosis with diarrhœa. Here an amylaceous and fatty diet is useful, and bodily and mental quietude.

The sugar is but seldom increased. It is diminished by absolute fasting, in robust constitutions, during menstruation, and in acute diseases. This deficiency may be supplied by administration of milk-sugar to the nursling, whilst the nurse may take amylaceous and saccharine diet.

The salts are increased in acute disease, especially typhus, and these occasion diarrhœa in the child. They are diminished in chronic diseases, especially in those of the intestines. Both nurse and nursling should take phosphate of lime and common salt.

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